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> " *To the solid ground*
> *Of Nature trusts the mind which builds for aye.*"—WORDSWORTH

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INDEX

ABERCROMBY (Hon. R.), the Aurora and its Spectrum, 173
Abney (Capt. W. de W., F.R.S.), Work in the Infra-red of the Spectrum, 15
Acetate of Soda, Heating by, 344
Acta Mathematica, 180
Adamson (Chas. Murray), "Another Book of Scraps principally relating to Natural History," 480
Aerolite near Alfianello, 496
Aeronautics, *see* Balloons
Africa: Joseph Thomson's Expedition, 21; H. M. Stanley's Report of his Expedition, 21; Milan Society's Expedition, 230; Lieut. Wissmann's Expedition, 348; Wissmann and Pogge's Expedition, 401: Central and West, H. Capello and R. Ivens, 391; Ethnology of North, Prof. A. H. Keane, 408; "Afrika's Ströme und Flüsse," Dr. J. Chavanne, 447; "Africana," Rev. D. Macdonald, 5[illegible]6; Dr. Emil Holub's New Expedition, 542
Agram, Earthquakes at, 348
Agricultural Students' Gazette, 230
Agriculture in Madras, 607
Aino Ethnology, Dr. J. J. Rein, 365; Prof. A. H. Keane, 389
Ainos, Bear Festival among the, 19
Air, Elasticity of, M. Kraevitch, 183
Air, on the Movements of, in Fissures and the Barometer, A. Strahan, 375, 461
Air-pump, Double-action Mercury, Serrawalle, 324
Airy (Sir G. B., F.R.S.), on the proposed Forth Bridge, 131; C. S. Smith, 99; B. Baker, 222
Airy (Dr. Hubert), the Magnetic Storm and Aurora, 86; Hovering of Birds, 294, 336, 388, 412; Soaring of Birds, 590
Albuminous Bodies, on the Isomerism of, Shigetaké-Sagiura, 103
Aldis (Prof. W. Steadman), "Introductory Treatise on Rigid Dynamics," 265; the Sea Serpent, 338
Aletia xylina in United States, Hibernation of, Dr. C. V. Riley, 214
Aleutian Islands, Fauna and Flora of, 520
Alfianello, Aerolite near, 496
Algæ in Palæozoic Strata, Invertebrate Casts *versus*, 46
Algæ, Fossil, Marquis de Saporta, 514
Algeria, Projet de Mer Intérieure dans l', 133; an Algerian Winter Resort for Gout and Rheumatic Patients, 113; Heavy Rains in, 229
Allen (Grant), Prof. Owen on Primitive Man, 31; the Shapes of Leaves, 439, 464, 492, 511, 552; F. O. Bower, 552; Leaves and their Environment, 604
Almanac, the Churchman's, for Eight Centuries, 386
Altai Exploration Expedition, 261
Altitude and Weather, Dr. Woeikoff, 223
Aluminium, New Method of Producing, Morris, 183
Amateurs and Astronomical Observation, W. F. Denning, 434
America: American Naturalist, 24, 189, 450; American Journal of Science, 71, 450, 521; American Journal of Forestry, 89; Mathematics in, J. W. L. Glaisher, F.R.S., 193; American Researches on Water Analysis, 211; American Antiquities, Prof. F. W. Putnam, 277; Ensilage in, Prof. James E. T. Rogers, Prof. J. Wrightson, 479; Proceedings of the American Association for the Advancement of Science, Prof. T. G. Bonney, F.R.S., 501; Evolution of the American Trotting Horse, W. H. Brewer, 609; *see also* United States, &c.
Amsterdam, Earthquake at, 517
"Ancient Scottish Lake Dwellings," by Dr. Munro, Sir John Lubbock, M.P., F.R.S., 145
Ancient Monuments, Worthington G. Smith, 182
Andernach, Discovery of Remains of Prehistoric Animals at, 445
Anderson (Dr. J., F.R.S.), Catalogue of Mammalia in Calcutta Museum, 172
Anemometric Observations on the *Djighit*, Dondojiroff, 445
Animal Intelligence, Dr. Fritz Müller, 240; J. G. Grenfell, 292; J. Birmingham, 337; Dr. J. Rae, F.R.S., 366; D. Pidgeon, 366
Animals, Protection of, International Congress for, 444; Benevolence in, Oswald Fitch, 580; Geo. J. Romanes, F.R.S., 607; on the Sense of Colour amongst some of the Lower, Sir John Lubbock, Bart., 618
Annalen der Physik und Chemie, 143, 281, 330, 450
Annonay Montgolfier Celebration, 517
"Another Book of Scraps," C. M. Adamson, 480
Antarctic Expedition, New Italian, 230
Anthropology, an Urgent Need in, W. L. Distant, 101; of the Jews, B. Blechmann, 113; Anthropological Institute, 119, 191, 215, 307, 332, 426, 475; Anthropo-Geographie, Dr. F. Ratzel, 125; Notes in the Solomon Islands, H. B. Guppy, 605
Antinori (Marchese O.), Death of, 132
Apatite, on a Fine Specimen of, from Tyrol, lately in the possession of Mr. Samuel Henson, 608
Arabians, Nautical Matters among Mediæval, 42
Aradas (Prof. Andrea), Death of, 160
Arc Lamp, Lever's, 274
Archegosaurus, Indian, R. Lydekker, 411
Archene, Earthquake at, 277
Archibald (E. Douglas): Shadows after Sunset, 77; Increase in Velocity of Wind with Altitude, 243; Diurnal Variations in the Velocity of the Wind, 461; Stevenson's Observations on Increase of Velocity of the Wind with the Altitude, 506
Archives des Sciences Physiques et Naturelles, 93, 143, 306, 473
Arctic Expedition, New Swedish, 400
Arctic Voyaging, Season of 1882 exceptionally adverse to, 372
Argentine, Description Physique de la République, d'après des Observations Personnelles et Étrangères, Dr. H. Burmeister, 28
Argyll (Duke of): Mimicry in Moths, 125; Transit of Venus, 156; Hovering of Birds, 312, 387; Metamorphic Origin of Granite—Prehistoric "Giants," 578
Arithmetic, Elementary Chemical, Sidney Lupton, 76
Armour-plate Experiments, Recent, 405
Asia (Central), Dr. Regel's Explorations in, 446
Asphyxiation in Rapid Falling, 62
Association for Improvement of Geometrical Teaching, 246, 299, 580
Astronomy: Astronomical Column, 20, 43, 161, 210, 248, 300, 324, 348, 374, 400, 424, 445, 497, 517, 540, 567, 589, 617; Methods employed in Astronomical Physics, M. Wolf, 372; Amateurs and Astronomical Observation, W. F. Denning, 434; Astronomical Photography, Edward C. Pickering, 556
Athens, Dr. Schliemann's proposed Excavations at, 276
"Atlantis," a Search for, with the Microscope, Dr. Arch. Geikie, F.R.S., 25
Atmosphere, Pollution of the, J. J. Murphy, 241; H. A. Phillips, 127, 266
Atti della R. Accademia dei Lincei, 354
Auden (A. W.), "The Vampire Bat," 411
Auerbach, Sound-Vibrations of Solid Bodies (glass cylinders) in Contact with Liquid, 325
Aurora Borealis: Prof. Lemstrom's Experiments with, 322; Sophos Tromholt, 394; in Sweden, 496
Auroræ: C. L. Wragge, 54; Prof. Herbert McLeod, Rev. C. J. Taylor, Rev. S. H. Saxby, 99; A. Batson, 99; J. Rand Capron, 149, 413; J. J. Murphy, 434; at Gastemünde, 374; Swan Lamp Spectrum and the, J. Rand-Capron, 149; Swan Lamp Spectrum and, J. Munro, 173; Aurora of November 17, Admiral Ommanney, F.R.S., Prof. Tacchini, J. R. Capron, W. M. F. Petrie, H. D. Taylor, E. Pollock, J. F. Cole, A. Batson, F. R. Clapham, T. W. Backhouse, Prof. L. G. Carpenter, 138 *to* 142; T. W. Backhouse, W. M. F. Petrie, Dr. H. Muirhead, 315; Auroræ in Sweden, 113; in Belgium, 160; Aurora and its Spectrum, Hon. R. Abercromby, 173; J. R. Capron, 198; the Heights of Auroras, T. W. Backhouse, 198; Auroral Meteoric Phenomena of November 17, 1882, Dr. Groneman, 388; T. W. Backhouse, 41[illegible]; A. Batson, 412; Auroral Experiments in Finland, S. Lemström, 389; Rowell's Experiments on Auroræ, 443

Avalanche in Western Switzerland, 181
Ayrton (Prof. W. E.), Electric Railways, 255

Bacillus of Tuberculosis (Koch), Preliminary Note on the, J. W. Clark, 492; Watson Cheyne's Report on, 563
Backhouse (Thos. W.), Comet 1882 *b*, 52, 338; Aurora of November 17, 1882, 141, 315, 418; the Heights of Auroras, 198
Baillie-Grohman (W. A.), "Camps in the Rockies," 551
Baird's Hare and its Habits, 241; T. Martyr, 266
Baker (B.), Sir Geo. Airy on the Forth Bridge, 222
Balkan Peninsula, Earthquake in the Northern Part of, 19
Ball (Dr. R. S.), Transit of Venus, 157; Great Tides, 201
Balloons: Centenary of the Discovery of, 277; Tissandier's Electromagnetic Engine for directing, 323; Extraordinary Run of a small Hydrogen, 400; Electricity as a Motive Power for, Napoli, 517
Baltic, Seals in the, 133
Baluchistan, Wanderings in, Major-General Sir C. M. MacGregor, Prof. A. H. Keane, 359
Barfoot (W.), the Sea-Serpent, 338
Barkas (T. P.), the Magnetic Storm and Auroræ, 87; an Extraordinary Lunar Halo, 103
Baro-manometrical Experiments, Kraevitsch, 324
Barometer, on the Movements of Air in Fissures and the, A. Strahan, 375, 461
Batson (A.), Auroral "Meteoric Phenomena" of November 17, 1882, 100, 141, 412
Bavaria, Lower, Discoveries of Gold and Silver in, 399
Baxter College in Dundee, 247
Bear Festival among the Ainos, 19
Bees, Senses of, 46
Beetles, "Die Kafer Westfalens," F. Westhoff, 239
Belgrade, Skeleton of Mammoth found at, 63
Bellesme (Prof. J. de), on Claude Bernard, 317
Benevolence in Animals, Oswald Fitch, 580; Geo. J. Romanes, F.R.S., 607
Ben Nevis, Meteorological Observatory on, 18, 39, 175, 399, 411, 487
Bennett (A. W.), Nesting Habits of the Emu, 530
Berlin: Physical Society of, 95, 143, 216, 236, 284, 380, 403, 476, 571, 620; Physiological Society of, 120, 191, 216, 355, 379, 403, 452, 524, 595; Berlin Agricultural Museum, 180
Bernard (Claude), Prof. J. de Bellesme on, 317
Berson (M.), Magnetisation of Metals, 183
Bertillon (Dr.), Death of, 444
Bhamo, Colquhoun's Journey to, 63
Bidwell (Shelford), "Electrical Resistance of Carbon Contacts," 376
Binary Star, ζ Cancri, 424
Binary Star, *p* Eridani, 589
Binary Stars, 518
Biology in Italy, 46; Biological Notes, 91, 230; New Lantern Slides for Illustrating Biology, 276; Deductive Biology, W. T. Thiselton Dyer, F.R.S., 554
Bird of Paradise, New Species of, 276
Bird-track by Sea-shore, apparent, 91
Birds, a History of British, Part xv., Wm. Yarrell, 98
Birmingham (John), Transit of Venus, 1882, British Expeditions, 180; Intelligence in Animals, 337
Birmingham Natural History and Microscopical Society, 587
Bischoff (Dr. T. L. W. von), Death of, 180
Blackheath, "Denehole" at, 324
Blanford's Sheep, 415
Blasius (Dr.), Fossil Souslik in Northern Germany, 247
Blastopore, Prof. F. M. Balfour on the Existence of a, 215
Blackman (B.), Anthropology of the Jews, 113
Bles (E. J.), a "Natural" Experiment in Complementary Colours, 241
Blomfield (Rev. L.): Ticks, 552; *Helix pomatia*, L., 553
"Blowing Wells," 375
Bobenhausen Pile Dwellings, 160
Bohdalek (Dr.), Death of, 399
Bohemia, Earthquake in, 400
Bologna, Earthquake at, 445
Bombay, Deaths from Snake-bite in, Sir Joseph Fayrer, F.R.S., 556
Bonney (Prof. T. G., F.R.S.): Hornblendic and other Schists of the Lizard District, 71; Proceedings of the American Association for the Advancement of Science, 501
Botanic Gardens, Calcutta, Report of the Royal, 114
Botanical Garden, Saharunpur, 588
Botanical Year Book, Pringshei[illegible] 502
Botany, Text Book of Morpholo[illegible]nd Physiological, Julius Sachs, Prof. E. P. Wright, 263
Botany of the *Challenger* Expedition, W. Botting Hemsley, 462
Bottomley (J. T.), Thomson[illegible]se-Mill Dynamo, 78
Boulger (Prof. G. S.), Epping Forest, 455
Bowditch (Dr. H. P.), Optical Illusions of Motion, 183
Bower (F. O.), Mr. Grant Allen's Articles on "The Shapes of Leaves," 552
Braces or Waistband? 531, 553, 580
Branfill (B. R.), an Extraordinary Meteor, 149
Brassey (Sir Thomas), "The British Navy; its Strength, Resources, and Administration," W. H. White, 549
Briault on *Isanemones*, 20
Brewer, Distiller, and Wine Manufacturer, 311
Brewer (W. H.), Evolution of the American Trotting Horse, 609
British Association, Proposed Visit to Canada, 41, 88, 299, 398, 538, 546, 587; Protest against Meeting in Canada, 209; and Local Societies, 132
British Birds, a History of, Part xv., Wm. Yarrell, 98
British Circumpolar Expedition, Capt. Dawson, R.A., 484
"British Navy; its Strength, Resources, and Administration," Sir Thomas Brassey, W. H. White, 549
Britten (F. J.), Watch and Clockmaker's Handbook, H. Dent Gardner, 76
Brocklehurst (T. U.), "Mexico To-day," 503
Bronze Hatchets Discovered at Salez, 540
Brown (E.), the Magnetic Storm and Aurora, 86; Meteor, 508; the Zodiacal Light (?), 605
Brown (J. Campbell), Practical Chemistry, 75
Brunton (Dr. T. Lauder, F.R.S.): on Inhibition (of Structural Functions) and Action of Drugs thereon, 419, 436, 467, 485; Action of Calcium, &c., on Muscle, 450
Brussels Observatory, 445
Buchan (A.), Diurnal Variation of Wind on Open Sea and near and on Land, 413
Buckley (Arabella), Winners in Life's Race, 51
Buenos Ayres, the Comet 1882 *b* at, 62
Bulletin de l'Académie Royale des Sciences de Belgique, 71, 281, 473
Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, 94
Bulletin de la Société d'Anthropologie de Paris, 214
Bulletin de la Soc. Imp. des Naturalistes de Moscou, 47, 189
Burmah, Colquhoun's Journey to, 92
"Burman, The," Shway Yoe, 5; Dr. E. B. Tylor, F.R.S., 6
Burmeister (Dr. H.), "Description Physique de la République Argentine d'après des Observations Personnelles et Etrangères," 28
Burton (Richard F.) and Verney Cameron, "To the Gold Coast for Gold," 335
Burton (F. M.), Sap-flow, 530
Butterflies, Natural Enemies of, Henry Higgins, 338
Butterflies of India, Burmah, and Ceylon, Marshall and Nicéville's, H. J. Elwes, 50

Cacao: How to Grow and How to Cure it, D. Morris, 566
Cacciatore (Prof.), Transit of Venus, 180
Calcium, Lockyer's Dissociation Theory of, Hermann W. Vogel, 233
Calcium, Barium, and Potassium, Action of, on Muscle, Doctors Brunton and Cash, 450
Calcutta: Report of the Royal Botanic Gardens, 114; Catalogue of Mammalia in the Museum, Dr. Anderson, F.R.S., 172; Proposed Exhibition in, 209
Callard (T. K.), Palæolithic River Gravels, 54
Cambridge: New Professorships at, 469; Philosophical Society, 62, 119, 143, 403
Cameron (Verney), Richard Burton and, "To the Gold Coast for Gold," 335
Campbell (Lewis, LL.D.), the Life of James Clerk Maxwell, 26
"Camps in the Rockies," W. A. Baillie-Grohman, 551
Canada, Proposed Visit of the British Association to, 41, 88, 299, 398, 538, 546, 587; Protest against, 209
Candolle (A. de), "Origine des Plantes Cultivées," 429
Cape Horn, French Mission to, 344
Cape Sea Lion, 415
Capello (H.) and Ivens (R.), Central and West Africa, 391
Caprifig and Fig, Relations of the, W. B. Hemsley, 584

Capron (J. Rand): a Lunar Halo, 78; the Magnetic Storm and Aurora, 83; Shadows after Sunset, 102; Aurora of November 17, 139, 149; Swan Lamp Spectrum and the Aurora, 149, 198; the Weather, 198; Foam-balls, 531
Carbo-hydrates in the Animal System, Physiology of, Dr. F. W. Pavy, F.R.S., 618
Carbon Contacts, Electrical Resistance of, Shelford Bidwell, 376
Carboniferous Vertebrate Palæontology, Notice of some Discoveries recently made in, T. Stock, 22
Cariamas, Flamingoes and, James Currie, 389
Carlisle (Bishop of), Weather Forecasts, 4, 51
Carlsruhe, Meteor at, 540
Carnivora, Siwalik, Richard Lydekker, 293
Carpathian Club, 108
Carpenter (Prof. L. G.), Aurora of November 17, 141
Cascia, Earthquake at, 63
Cash (Dr.), Action of Calcium, &c., on Muscle, 450
Caspian, Changes of Level of, 541
Cassell's Encyclopædic Dictionary, 423
Cassell's Natural History, 367
Cassine, Earthquake at, 299
Cassini Division of Saturn's Ring, 374
Cassowary, the One-wattled, 153
Cathodes, Chemical Corrosion of, Dr. Gore, F.R.S., 374
Caucasus: Volcanic Eruption in the, 63; Geography of the, 470; Administration of Public Instruction of, 497; Geographical Society of, 424; "Bulletin" of Society for History and Archæology, 497
Cecil (Henry): Comet 1882 *b*, 52; the Transit of Venus, 159; Hovering of Birds, 388; Meteors, 483
Central Asia, "Travels and Adventures East of the Caspian during the Years 1879–81, including Five Months' Residence among the Tekkés of Merv," Edmond O'Donovan, Prof. A. H. Keane, 359
Ceraski's Variable Star, U Cephei, 424
Cesati (Baron V.), his Botanical Collection, 20
"Ceylon, Lepidoptera of," L. Reeve and Co., 150
Challenger Expedition, Botany of the, W. Botting Hemsley, 462
Challenger Reports (Zoology, ii., iii., and iv.), 73
Challis (Prof. James, F.R.S.), Death of, 132
Chambery, Phylloxera in, 133
Chandler (Prof.), the Comet, 81
Changy on Incandescent Lamps, 209
Channel, Mountainous Seas in Calm Weather, 540
Channel Tunnel, Prof. W. Boyd Dawkins, F.R.S., 338
Chavanne (Dr. J.), "Afrika's Ströme und Flusse," 447
Chemistry: the Chemical Society, 47, 118, 191, 234, 306, 402, 426, 522, 570, 595; Recent Chemical Synthesis, 49; Practical Chemistry, J. Campbell Brown, 75; Elementary Chemical Arithmetic, Sidney Lupton, 76; Principles of Chemistry, Prof. Mendeléeff, 113; Chemical Notes, 301; Electrolytic Balance of Chemical Corrosion, Dr. Gore, F.R.S., 326; Properties (Physical and Chemical) of Simple and Compound Bodies, De Heen, 422; Chemistry of the Planté and Faure Accumulators, Dr. J. H. Gladstone, F.R.S., and Dr. A. Tribe, F.R.S., 583
Chevreul (M.): nominated President of the French Société Nationale d'Agriculture, 277; his long Scientific Career, 422
Cheyne (Watson), the Bacillus of Tubercle, 563
China: Foreign Education in, 90; Chinese Definition of Death, 160; the Electric Light in, 209; R. K. Douglas on, 221; Comets as Portents in, 229; "Chinarinden in Pharmakognostischer Hinsicht dargestellt, Die," F. A. Flückiger, 287; Scientific Heresies in, 342; Chinese Educational Mission, 566; Miss Dr. Howard's Success in, 567; Lead Ore discovered in, 588; Telegraph Extension in, 588; Waste Paper in, 588; Coroners' Science in, Robert K. Douglas, 612
Chlorophyll Corpuscles of Hydra, Prof. E. Ray Lankester, F.R.S., 87
Christie (W. H. M., F.R.S.), the Magnetic Storm and Aurora, 82
Cinchona Culture, Handbook of, Karel Wessel van Gorkom, 287
Circle, Medioscribed, 607
Circumpolar Expedition: Notes from Capt. Dawson's Letters, 103, 242, 484; Dr. Rae, F.R.S., on, 508
Civil Service Examinations, Natural Science in, 321
Clapham (F. R.), Aurora of November 17, 141
Clark (J. E.), the Magnetic Storm and Aurora, 84
Clarke (C. B.), Equal Temperament of the Scale, 240
Clark (J. W.): Condensation of Liquid Films on Wetted Solids, 370; Preliminary Note on the Bacillus of Tuberculosis (Koch), 492
Clerk-Maxwell on Stress, 314
Clouds, Magnetic Arrangement of: C. H. Romanes, 31; Rev. W. Clement Ley, 53
Coal Fire, Flames in, 183
Coal Gas, Lime as a Purifier of Products of Combustion of, Dr. Joule, F.R.S., 496
Coan (Rev. Titus), Death of, 398
Cobbold (Dr. T. Spencer, F.R.S.): Ticks, 552; *Simondsia paradoxa*, 547
Cold, Unprecedented, in the Riviera—Absence of Sunspots, C. J. B. Williams, 551
Cole (J. F.), Aurora of November 17, 141
Colour, on the Sense of, amongst some of the Lower Animals, Sir John Lubbock, Bart., 618
Colours, Complementary, 78; J. J. Murphy, 8; C. R. Cross, 150; at Niagara Falls, H. G. Madan, 174; "Natural" Experiment in, E. J. Bles, 241; Chas. T. Whitmell, 266; Novel Experiment in, John Gorham, 294
Colquhoun (A. R.): Journey through Yunnan to Burmah, 92; Journey to Bhamo, 63
Coltsfoot, Early, R. McLachlan, F.R.S., 266
Comets: Schmidt's Cometary Object, 20; the Great Comet 1882 *b*, 4, 21, 43, 56, 80, 161, 210, 300, 349, 400, 446, 540, 589; Major J. Herschel, Geo. M. Seabroke, Arthur Watts, 4; Spectroscopic Observations of, 24, 42; C. J. B. Williams, 29; W. J. Millar, 29; W. Higginson and B. Manning, 29; seen at the Paris Observatory, 42; J. P. McEwen, 52; T. W. Backhouse, 52, 338; G. M. Seabroke, 52; H. Cecil, 52; B. J. Hopkins, 62, 78; at Buenos Ayres, 62; J. K. Rees, Prof. Chandler, 81; "Anomalous" Tail of, Rev. T. W. Webb, 89; Major J. Herschel, 101; Commander Sampson, U.S.N., Dr. C. J. B. Williams, F.R.S., 108; Division of the Nucleus of the, 113; W. C. Winlock, 128; Dr. Doberck, 128; A. Ainslie Common, 150; F. Stapleton, 150; Photograph of, 180; during last November and December, C. J. B. Williams, F.R.S., 198; Elements of, Vice-Admiral Rowan, Prof. E. Frisby, 226; J. P. McEwen on, 247; W. T. Sampson, 266; Dr. B. A. Gould, 267; E. Ristori, 314; Orbit of, E. Ristori, 388; Ephemeris of, Prof. Frisby, 415; Prof. Schiaparelli on the, Francis Porro, 533; Comet 1882 *c*, 161, 248; Comets as Portents in China, 229; Figure of the Nucleus of Comet of 1882 (Gould), Prof. Holden, 246; D'Arrest's Comet, 324, 567, 589, 618; Reported Discovery of a Comet, 324; Denning's Comet, 343; Comet of 1771, 374; Comet 1883 *a*, 445, 517; Comet of 1812, 498
Common (A. Ainslie), the Comet, 150
Complementary Colours, 78; J. J. Murphy, 8; C. R. Cross, 150; at Niagara Falls, H. G. Madan, 174; a "Natural" Experiment in, E. J. Bles, 241; Chas. T. Whitmell, 266; Novel Experiment in, John Gorham, 294
Condensation of Liquid Films on Wetted Solids, J. W. Clark, 370
Congo (Upper), Departure of Belgian Expedition for, 182
Coniferæ, Female Flowers in, 231
Conte (Dr. Le), on Darwinism, 275
Cooke (Conrad), &c., "Electric Illumination," 264
"Cookery, the Rudiments of," A. C. M., 323
Cooley (W. Desborough), Death of, 469
Copernicus, Darwin and, Prof. E. du Bois Reymond, 557
Coral-eating Habits of Holothurians, Surgeon-Major H. B. Guppy, 7; Saville Kent, 433; J. G. Grenfell, 508
Cordiner (R.), Age of Dogs, 79
Corona, Photographing the, Dr. W. Huggins, F.R.S., 199
Coroners' Science in China, Robert K. Douglas, 612
Corpuscles, Red Blood, Prof. Quincke on, 355
Cosmical Dust collected by M. Marx, Prof. Lenz, 422
Cothenius Medal, 540
Cowan (Mr. D.), Explorations in Madagascar, 372
Crombie (J. M.), Function of Membrana Flaccida of Tympanic Membrane, 129
Cross (C. R.), Complementary Colours, 150
Crystal Palace Electricity and Gas Exhibition, 180
Cryptogamic Flora of Germany, Austria, and Switzerland, Mary P. Merrifield, 385
Cryptophyceæ, a New Genus of, 230
Cumming (Miss Gordon), "Fire Fountains," 525
Cunningham (David), Hovering of Birds, 336

Cunningham (Major Allan), Roorkee Hydraulic Experiments, 1; the Indian Survey, 97
Cunningham (J. T.), Zoological Station in Naples, 453
Currie (James), Flamingoes and Cariamas, 389
Cutting Tools worked by Hand and Machine, Robert H. Smith, 577
Cyprus, Earthquake in, 497

Dachstein Glaciers, Decrease of, 42
Dairy Industry in France, 90
D'Arrest's Comet, 324, 567, 589, 618
Darwin (Charles), on Tower-like Dejections made by Worms, 20; Enthusiasm in Sweden on the Memorial to, 275; Darwin and Copernicus, Prof. E. du Bois Reymond, 557; Hostile Criticism in Germany of the Address on, 565
Darwin (Prof. G. H., F.R.S.), Numerical Estimate of the Rigidity of the Earth, 22; Elected Plumian Professor of Astronomy and Experimental Philosophy at Cambridge, 275; Formation of Mudballs, 507
Darwinism, Dr. Le Conte on, 275
Dary (Georges), on Electric Navigation, 41, 42
Davis Lectures for 1883, 565
Dawkins (Prof. W. Boyd), Channel Tunnel, 338
Dawson (Capt.), Notes on Circumpolar Expedition from Letters of, 103, 242, 484
De La Rue's Diaries, &c., 161
De Morgan (Augustus), Memoir of, R. Tucker, 217
Death, Chinese Definition of, 160
Deaths from Snake Bite in Bombay, Sir Joseph Fayrer, F.R.S., 556
Dechevrens (Father Marc), a Curious Halo, 30
Deductive Biology, W. T. Thiselton Dyer, F.R.S., 554
Deep-Sea Exploration, French, 587
"Deneholes" at Blackheath, 324
Denmark: Oyster Fisheries of, 346; Glacial Phenomena in, Prof. Johnstrup, 373
Denning (W. F.): Transit of Venus, 158; Markings on Jupiter, 365; Amateurs and Astronomical Observation, 434; the Large Meteor of March 2, 1883, 461
Denning's Comet, 348
Deprez's Electrical Theories, 399
Derby Free Library, Reference Catalogue, 62
Desborde (Col.), on Banks of Niger, 424
Dewar (Prof.) and Prof. Liveing, Origin of Hydrocarbon Flame Spectrum, 257
Diamond in its Matrix, a, 90
Dickert (Herr Thomas), Death of, 399
Dickson (Dr. Oscar): his liberality in aiding Scientific Research, 444; Account of Swedish Expedition to Greenland, 541
Dier (Prof.), Shadows after Sunset, 150
Dijmphna, Search Expedition for the, 64
Diluvial Mammals, Discovery of Remains of, on Wolga, 373
Dip-Circle, Goolden's Simple, 127
Direct-Vision Prisms, 182
Distant (W. L.): an Urgent Need in Anthropology, 101; "Rhopalocera Malayana," 444
Diurnal Variation in the Velocity of the Wind, E. Douglas Archibald, 461
Djighit (the), Domojiroff's Anemometric Observations on, 445
Doberck (Dr. W.): the Comet 1882 *b*, 129; Transit of Venus, 158; appointed to the Hong Kong Observatory, 565
Dobson (J. L.), the Magnetic Storm and Aurora, 87
Doe with Horns, 209
Dogs, Age of, R. Cordiner, 79
Dolmen, Discovery of, at St. Pierre Quiberon, 540
Domojiroff's Anemometric Observations on board the *Djighit*, 445
Doradûs, Supposed Variable μ, a Spurious Star, 498
Double Stars, Measures of, 182
Douglas (Robert K.): China, 221; Coroners' Science in China, 612
Draper (Prof. Henry): Death of, 88; Obituary Notice of, 108
Dredging Implement, a New: Prof. A. Milnes Marshall, 11; W. A. Herdman, 54
Dreyer (J. L. E.), Transit of Venus, 158
Dublin: Royal Society of, 235; Experimental Science Association, 571; Aurora at, 589
Dumas (J. B. A.): Presentation to, 132; his Address at the Commemoration of the Fiftieth Anniversary of his Membership of the Paris Academy, 174; the Dumas Medal, 227; Illness of, 346
Duncan (Prof. P. Martin, F.R.S.), Heroes of Science, 76
Dust Showers in Norway, 496
"Dutton (Capt. C. E.), a Monograph by," Tertiary History of the Grand Cañon District, Dr. Arch. Geikie, F.R.S., 357
Dyer (W. T. Thiselton, F.R.S.): Influence of "Environment" on Plants, 82; the Sacred Tree of Kum-Bum, 2[illegible]4; Deductive Biology, 554
Dynamics, Introductory Treatise on Rigid, W. Steadman Aldis, 265
Dynamo, Thomson's Mouse-Mill, J. T. Bottomley, 78
Dynamo Machine, Elphinstone-Vincent, 516
Dynamo-electric Machine with Continuous Induction, Prof. Pfaundler the Inventor of, 517
Dynamo-electric Machines, Recent, 58

Earth, Numerical Estimate of the Rigidity of the, Prof. G. H. Darwin, F.R.S., 22
Earth Currents during Magnetical Perturbations, 89
Earth's Crust, Physics of the, Rev. O. Fisher, 76
Earth's Surface, Causes of Elevation and Depression of, W. F. Stanley, 523
Earthquakes: in the Northern part of the Balkan Peninsula, 19; at Cascia, 63; in Perugia, 90; in the Valais, 181; in Greece and Panama, 248; at Murcia, in Spain, at Archena, &c., 257; at Halifax, Prof. Geo. Lawson, 293; Clifton, Geo. F. Burder, 293; Hastings, R. H. Tiddeman, 293; at Cassine, 299; at Agram, 348; at Freiburg-im-Breisgau, 374; in Silesia and Bohemia, 400; in Hungary, 423; at Bologna, 445; in Japan, Proposed Study of, 469; in Cyprus, 497; at Amsterdam, 517; in Hungary and Italy, 540; at Pedara in Sicily, 567
Earthslip in Switzerland, 209
Earthworms in New Zealand, 91
Eastlake (F. W.), Geology of Hong Kong, 177
Eclipse, Total Solar, of May 6, 1883, 111, 248; the English Observers, 346, 398, 556, 567
Eclipse, Solar, Total, of May 17, 1901, 424
Eclipses, Solar, the Recent Coming and Total, J. Norman Lockyer, F.R.S., 185
Edinburgh: Royal Society of, 168, 283, 307, 403, 428, 452, 548, 571, 595; Mathematical Society of, 346, 500, 595
Education: Foreign Education in China, 90; of our Industrial Classes, J. Norman Lockyer, F.R.S., 248; A. J. Mundella on, 276; Prof. Huxley on, 396; Minister of, 495; in Japan, 617
Edwards (E. W. W.) and Higgins (C.), the Electric Lighting Act, 1882, 410
Eggs, Effect of Railway Transport on, 444
Egyptian Exploration Fund, 401
Elastiche Nachwirkung, M. Hesehus, 183
Electricity: International Electrical Conference, 18; Tricycles propelled by, 19; Electric Illumination of a Railway Bridge over the Ticino, 20; Electric Lighting, Sir C. W. Siemens, F.R.S., 67; Electric Lighting at Society of Arts, 133; Electric Light in China, 209; "Electric Illumination," Conrad Cooke, &c., 264; Priority in Photographing with Electric Light, 276; Public Electric Lighting, 384; Electric Lighting Act, 1882, C. Higgins and E. W. W. Edwards, 410; Electric Light at Savoy Theatre, 418; Electric Lighting in Switzerland, 497; in Paris, 516; Some Points in Electric Lighting, Dr. John Hopkinson, F.R.S., 592; Electric Navigation, Georges Dary, 41, 42; M. Tresca's Papers on Electrical Measures, 62; Electrical Transmission of Force and Storage of Power, Sir C. W. Siemens, F.R.S., 67, 518; Electrical Disturbances in Sweden and Norway, 89; Electrical Exhibition at the Crystal Palace, 180; Electric Reaction, Prof. Melde, 183; Electrical Phenomena, 199; Electric Railways, Prof. W. E. Ayrton, 255; "Electricity," by Robert M. Ferguson, 264; Electrical Exhibition at Vienna, 373, 444; "Electricity," a New Hungarian Journal, 299; Il Potentiale Elettrico nell' Insegnamento Elementare della Elettrostatica, Prof. A. Serpieri, 312; Electric Railways, 338; "Electrical Resistance of Carbon Contacts," Shelford Bidwell, 376; Electrical Theories, Deprez's, 399; Light-Phenomena of Electric Discharges, Dr. Hertz, 403; Influence of Vacuum on Electricity, A. M. Worthington, 434; Electro-technical Society formed at Vienna, 469; Successful Trial of an Electrically-moved Tram-car, 470; Electricity as a Motive Power for Balloons, Napoli, 517; Proposed Electric Railway from Charing Cross to Waterloo, 566
Electrolytic Balance of Chemical Corrosion, Dr. Gore, F.R.S., 326
Electromotive Force and Resistance of Batteries, Effects of Temperature on, W. H. Reece, F.R.S., 426

Electromotive Force of Galvanic Combinations, Kittler's Normal Element of, 325
Electronomie Industrie, Manuel d', R. V. Picou, 146
Electroscope, a Modification of the Gold Leaf, F. J. Smith, 102
Elephant in Italy, Discovery of Fossil, 181; Threatened Extinction of the, E. E. Prince, [illegible]
Elger (P. G.), the Magnetic Storm and Aurora, 85
Elphinstone-Vincent Dynamo Machine, 516
Elwes (H. J.), Marshall and Nicéville's Butterflies of India, Burmah, and Ceylon, 50
Emu, Nesting Habits of, A. W. Bennett, 530
Encyclopædic Dictionary, 423
Enemies, Natural, of Butterflies, Henry Higgins, 338
Energy in the Spectrum, Distribution of, Lord Rayleigh, F.R.S., 559
Energy, Transmission of, on Marcel-Deprez System, 372
Engineering Laboratory, University College, 160
"Ensilage in America," Prof. James E. T. Rogers, Prof. Wrightson, 479
Entomological Society, 451, 500
"Environment" on Plants, Influence on, W. T. Thiselton Dyer, F.R.S., 82; Howard Fox, 315
Eocene Section, the Lower, J. Starkie Gardner, 331
"Episodes in the Life of an Indian Chaplain," 99
Epping Forest and the Railway Companies, 399; Prof. G. S. Boulger, 455; Conservation of, from the Naturalist's Stand-Point, 447
Equal Temperament of the Scale, C. B. Clarke, 240
Ergot, Poisonous Principle of, 517
Eridani, Binary Star *p*, 589
Ernst (Dr. A.), Abnormal Fruit of Opuntia Ficus-Indica, 77
Essex County, Mass., Flora of, 173
Essex Field Club, 298, 347
Ether, the, and its Functions: Prof. Oliver Lodge, 304, 328; S. Tolver Preston, 579
Ethnographical Exhibition at Stockholm, Dr. A. B. Meyer, 370
Ethnology, Aino: Dr. J. J. Rein, 365; Prof. A. H. Keane, 389
Ethnology of North Africa, Prof. A. H. Keane, 408
Etna (Mount), Eruption of, 422, 496, 515, 539
Evans (C.), Palæolithic River Gravels, 8
Evolution of the American Trotting Horse, W. H. Brewer, 609
Evolution, Hypothesis of Accelerated Development, by Primogeniture and its Place in the Theory of, Prof. A. A. W. Hubrecht, 279, 301
Explosive Waves, Berthelot and Vielle on, 301
Eye, Internal Reflections in the, H. Frank Newall, 376

"Fall Machine," Poggendorff's, 300
Falling, Asphyxiation in Rapid, 62
Farr (Dr. W., C.B.), Death of, 587
Fatio (Dr. Victor), Fishes of Switzerland, 220
Fauna, Pelagic, of Freshwater Lakes, 92
"Faune des Vertébrés de la Suisse," Dr. Victor Fatio, 220
Fayrer (Sir Joseph, F.R.S.), Destruction of Life in India by Poisonous Snakes, 205, 556; Destruction of Life in India by Wild Animals, 268; Speke and Grant's Zebra, 604
Feed-Water Heater and Purifier, Geo. Strong, 90
Ferghana Naphtha Well, 445
Ferguson (Robert M.), "Electricity," 264
Fermat, the Statue of, 180
Ferns of Kentucky, John Williamson, 336
Fertilisation of the Speedwell, A. Ransom, 149
"Feuille des Jeunes Naturalistes," 19
Fibre-balls, 580
Fig and the Caprifig, on the Relations of the, W. Botting Hemsley, 584
Fingal's Cave, Is, Artificial? F. Cope Whitehouse, 285
Finland, New Lyceums in, 42; Auroral Experiments in, S. Lemstrom, 389
Finsbury Technical College, 318; Opening of, 425
Finsch (Dr. Otto), his Exploration of New Guinea, 43; his Report of his Travels, 181
"Fire-Fountains," Miss Gordon Cumming, 525
Fires in Russia, Forest, 113
Fish, New Deep-Sea, from Mediterranean, Dr. Giglioli, 198
Fish Culture Association, National, 323
Fisher (Rev. Osmond), Physics of the Earth's Crust, 76
Fisheries Exhibition, the International, 322, 389, 468, 496, 516 536
Fisheries, Herring and Salmon, 442
Fishes of Switzerland, Victor Fatio, 220
Fishes, the Skeleton of Marsipobranch, W. K. Parker, F.R.S., 330
Fissures, on the Movements of Air in, and the Barometer, A. Strahan, 375, 461
Fitch (Oswald), Benevolence in Animals, 580
Flames in Coal Fire, 103
Flamingoes and Cariamas, James Currie, 389
Flegel (Robert), News of, 230
Fleische (E. von), Wartmann's Rheolyzer, 127
Flora of Essex County, Massachusetts, John Robinson, 173
Flora, Siberian, 445
Flora, Vernal, Origin of our, J. E. Taylor, 7
Flower (Prof W. H.), Lectures on Anatomy of the Horse, 372
Flower, Two Kinds of Stamens with Different Functions in the same, Dr. Hermann Muller, 30; Dr. Fritz Müller, 364
Flowers, Double, Dr. M. T. Masters, 126
Flowers, Insects Visiting, 498
Fluckiger (F. A.), "Die Chinarinden in Pharmakognostischer Hinsicht dargestellt," 287
Foamballs, J. Rand Capron, 531
Fohn, the, A. Irving, 605
Fonvielle (W. de), "Les Passages de Venus," 132
Food Adulteration Act of 1881, 301
Foraminifera in British Museum, Catalogue of Fossil, Prof. T. Rupert Jones, 173
Forbes (H. O.), Forbes' Visit to Timor-Laut, 159
Forbes (W. A.), Zoological Expedition up the Niger, 14; Letters from Shonga, 276; Death of, 614
Force, Transmission of, by Electricity, Dr. C. W. Siemens, F.R.S., 67
Forecasts, Weather: Bishop of Carlisle, 4, 51; Rev. W. Clement Ley, 29; C. W. Harding, 79
Forel (Prof.), Recent Rhone Glacier Studies, 183
Forest Fires in Russia, 113
Formic and Acetic Acid in Plants, 91
Forms of Leaves, Sir John Lubbock, Bart., 605
Forth Bridge, New, 62, 89; Sir Geo. B. Airy on the, C. S. Smith, 99, 131; Herbert Tomlinson, 147; B. Baker, 222
Fossil Elephant in Italy, Discovery of, 181
Fossil Souslik in Northern Germany, Dr. Blasius, 247
Fountain (J. M.), the Weather, 32
Fox (Howard), Influence of "Environment" on Plants, 315
France: Annual Meeting of Institute of, 18; Dairy Industry in, 90; Heavy Rains in, 229; French Mission to Cape Horn, 344; Annual Meeting of French Learned Societies, 539
Frankland (Dr. E., F.R.S.): Smoke Abatement, 407; Chemistry of Storage Batteries, 568
Freiburg-im-Breisgau, Earthquake at, 374
Freshwater Lakes, Pelagic Fauna of, 92
Friele (Hermann), "Der Norske nord-hass-expedition, 1876–1878," Dr. J. Gwyn Jeffreys, F.R.S., 457
Frisby (Prof. E.): Elements of the Great Comet 1882 *b*, 226; Ephemeris of it, 415; Transit of Venus, 266
Fritsch (Prof.), On the Torpedo, 403
Fulgurite found near Warmbrunn, 540

Galloway (William), Hovering of Birds, 336
Galton (Francis, F.R.S.), Hydrogen Whistles, 491
Galvanometers, on the Graduation of, for the Measurement of Currents and Potentials in Absolute Measure, Andrew Gray, 32, 105, 319, 339; New Torsion, 620
Gambetta's Body, Autopsy of, 247
Gardner (H. Dent), Britten's Watch and Clockmaker's Handbook, 76
Gardner (J. Starkie), the Lower Eocene Section between Reculvers and Herne Bay, 331
Garman (S.): Scream of Young Burrowing Owl like Warning of Rattlesnake, 174; a Possible Cause of the Extinction of the Horses of the Post-Tertiary, 313
Garrod (Dr. A. B., F.R.S.), on Uric and Hippuric Acid, 451
Gases, Action of Gravity on, Kraevitsch, 324
Geestemünde, Aurora at, 374
Geikie (Dr. Arch., F.R.S.): a Search for "Atlantis" with the Microscope, 25; Recent Researches in Metamorphism of Rocks, 121; "Geological Sketches," G. K. Gilbert, 237, 261; "Text-Book of Geology," G. K. Gilbert, 237, 261; Tertiary History of the Grand Cañon District, 357
Geikie (Prof. James, F.R.S.), Aims and Method of Geological Inquiry, 44, 64
Geodetical Operations, Norwegian, 224, 341
Geographentag, Third German, 447

Geographical Notes, 21, 43, 63, 92, 161, 400, 424, 541, 589
Geography in Schools, 209
Geography of the Caucasus, 470
Geology: Railway Geology—a Hint, 8; Aims and Method of Geological Inquiry, Prof. James Geikie, F.R.S., 44, 64; Geological Society, 71, 118, 215, 282, 331, 355, 426, 474, 500, 547; Distribution of Medals, 427; Geology, Part I, A. H. Green, 98; Geology of Hong Kong, F. W. Eastlake, 177; "Geological Sketches at Home and Abroad," Arch. Geikie, LL.D., F.R.S., G. K. Gilbert, 237, 261; "Text-Book of Geology," Arch. Geikie, LL.D., F.R.S., G. K. Gilbert, 237, 261; New Appointments for the Geological Survey of Scotland, 246; Geologists' Association, 523
Geometrical Teaching, the Association for the Improvement of, 246, 299, 580
German Aeronautical Society, Meeting of, 348
German African Expedition; Drs. Wissmann and Pogge, 92
German African Society, Report of, 92
German Fishery Society, 276
German North Polar Expedition, 43
German Ornithological Society, Annual Meeting at Berlin, 19
German Society for Prevention of Pollution of Rivers, &c., 89
Gibney (Robert Dwarris), the Zodiacal Light (?), 605
Giglioli (Dr.), New Deep-Sea Fish from Mediterranean, 198
Gilbert (G. K.), Arch. Geikie's "Geological Sketches at Home and Abroad," and "Text-Book of Geology," 237, 261
Gill (H. C.), the Magnetic Storm and Aurora, 85
Glacial Formations of Russia, 497
Glacial Phenomena in Denmark, Prof. Johnstrup, 373
Glacier Motion, Solar Radiation and, Rev. A. Irving, 553
Glacier Studies, Prof. Forel on Recent Rhone, 183
Glaciers, Decrease of the Dachstein, 42
Gladstone (Dr. J. H., F.R.S.), Chemistry of the Planté and Faure Accumulators, 583
Glaisher (J. W. L., F.R.S.), Mathematics in America, 193
Glazebrook (R. T., F.R.S.): a Common Defect of Lenses, 198; "Physical Optics," 361
Goddard (A. F.), Intra-Mercurial Planets, 148
Gold-leaf Electroscope, a Modification of the, F. J. Smith, 102
"Gold Coast, to the, for Gold," Richard F. Burton and Verney L. Cameron, 335
Good (S. A.), Extraordinary Lunar Halo, 150
Goolden's Simple Dip Circle, 127
Gore (Dr., F.R.S.): Electrolytic Balance of Chemical Corrosion, 326; Chemical Corrosion of Cathodes, 374
Gorham (John), Novel Experiment in Complementary Colours, 294
Gorkom (Karel Wessel van), Handbook of Cinchona Culture, 287
Gottingen, Royal Society of Sciences of, 48, 452
Gould (Dr. B. A.), Comet 1882 *b*, 267
Gout and Rheumatic Patients, an Algerian Winter Resort for, 113
Govi (Prof.), Expansion of Bulbs of Liquid Thermometers, 209
Granite, Metamorphic Origin of, Duke of Argyll, 578
Gratings, Concave, Prof. Rowland's, 95
Gravels, Palæolithic River: C. Evans, 8; Wm. White, 53; T. K. Callard, 54; Worthington G. Smith, 102
Gray (Andrew), on the Graduation of Galvanometers for the Measurement of Currents and Potentials in Absolute Measure, 32, 105, 319, 339
Gray (Prof. Asa), Natural Selection and Natural Theology, 291, 527
Gray and Milne's Seismographic Apparatus, 547
Greece, Earthquake in, 248
Greek Archipelago, Volcanic Phenomena in, 445
Green (A. H.), Geology, Part i., 98
Green (John Richard), Obituary Notice of, 462
Greenland, New Danish Expedition to, 446; Nordenskjöld's Expedition, 424, 496; Dr. O. Dickson on, 541
Grenfell (J. G.), Geological Traces of Great Tides, 222; Intelligence in Animals, 292; Holothurians, 508
Gresham Funds, the, 292
Grey, (F. W.), Snow Rollers, 507
Griffith (J. W.) and A. Henfrey, Micrographic Dictionary, 603
Griffith (R. W. S.), a Meteor, 434
Grocers' Company, the Scheme of the, for the Encouragement of Original Research in Sanitary Science, 574
Groneman (Dr. H. J. H.): Remarks on and Observations of the Meteoric Auroral Phenomenon of November 17, 1882, 296; the Auroral Meteoric Phenomena of November 27, 1882, 388
Ground, Observations of Periodic Movements of the, 300
Gudvangsören, Landslip at, 463
Guns, Wire, James A. Longridge, 11, 35, 53
Guppy (Surgeon-Major H. B.): Coral-eating Habits of Holothurians, 7; Habits of Scypho-Medusæ, 31; Anthropological Notes in the Solomon Islands, 607
Gwynne (Bertram), Curious Case of Ignition, 580
Gyrostatics, Sir William Thomson, 548

Hagen (Dr. H. A.), Invertebrate Casts, 173
Håkonson-Hansen's (Herr), Observations of the November Auroral Displays, 347
Halo, a Curious, Father Marc Dechevrens, 30; Rev. W. Clement Ley, 53; Rev. Gerard Hopkins, 53; a Lunar Halo, J. Rand Capron, 78; T. P. Barkas, 103; S. A. Good, 180
Hammam R'Irha, an Algerian Winter Resort for Gout and Rheumatic Patients, 113
Hammond, Kinetic Theory of Chemical Actions, 183
Hannay (J. B., F.R.S.), Natural Selection and Natural Theology, 362
Harding (C. W.), "Weather Forecasts," 79
Hare, Baird's, and its Habits, 241; T. Martyr, 266
Harkness (Prof.), on the Transits of Venus, 114
Harnett (W. L.), Meteor, 103
Harrison (J. Park), Projection of the Nasal Bones in Man and the Ape, 266, 294
Hart (Samuel): the Late Transit of Venus, 483; a Remarkable Phenomenon—Natural Snowballs, 483
Harting (J. E.), Incubation of the Ostrich, 480
Hatton (Frank), Death of, 515
Hawk Moth Larva, Surgeon-Major Johnson, 126
Heating by Acetate of Soda, 344
Heen (De), Relations between Physical and Chemical Properties of Simple and Compound Bodies, 422
Heilprin (Prof. Angelo), on the Value of the "Nearctic" as one of the Primary Zoological Regions, 606
Helix pomatia, L., Rev. L. Blomefield, 553
Heloderm, the Sonoran, 153
Hemsley (W. Botting): Botany of the *Challenger* Expedition, 462; on the Relations of the Fig and the Caprifig, 584
Henfrey (A.), J. W. Griffith and, Micrographic Dictionary, 603
Henson (Samuel), on a Fine Specimen of Apatite from Tyrol, lately in the possession of, 608
Herdman (W. A.), a Dredging Implement, 54
Heresies, Scientific, in China, 342
Heroes of Science, Prof. P. Martin Duncan, F.R.S., 76
Herring and Salmon Fisheries, 442
Herschel (Prof. A. S.), The Matter of Space, 458, 504
Herschel (Major J.): the Comet 1882 *b*, 4, 101; Soda Flames in Coal Fires, 78; the Magnetic Storm and Aurora, 87; Ignition by Sunlight, 531
Hertz (Dr.): Pressure of Saturating Vapour of Mercury, 183; Light-Phenomena of Electric Discharges, 403
Hesehus (N.): *Elastische Nachwirkung*, 183; Physics in Russia during the last ten years, 567
Hicks (Henry), Metamorphic Rocks of Ross and Inverness, 474
Hickson (Sydney J.), "Zoological Record," 366
Higgins (C.) and E. W. W. Edwards, "The Electric Lighting Act 1882," 410
Higgins (Henry), Natural Enemies of Butterflies, 338
Higginson (Walter), the Comet 1882 *b*, 29
High Tides, Abnormal, River Thames, J. B. Redman, 6
Hildebrandsson (Prof.), on the Ben Nevis Observatory, 18
Hippopotamus, Death of the old Female, 247
Histology, Vegetable, Tables of D. P. Penhallow, 458
Hofmann (Prof.), Lecture Experiments, 301
Holden (Prof.), Figure of Nucleus of Comet of 1882 (Gould), 246
Holley (G. W.), "Niagara and other famous Cataracts," 146
Holothurians, Coral-eating Habits of, Surgeon Major H. B. Guppy, 7; W. Saville Kent, 433; J. G. Grenfell, 508
Holub's (Dr. Emil), New African Expedition, 542
Home (D. M.), Ben Nevis Observatory, 411
Hong Kong, Geology of, F. W. Eastlake, 177; Observatory Scheme, 565
Hopkins (B. J.), the Comet 1882 *b*, 62, 78
Hopkins (Rev. Gerard), a Curious Halo, 53
Hopkinson (Dr. John, F.R.S.), Some Points in Electric Lighting, 592
Hornblendic and other Schists of the Lizard District, Prof. T. G. Bonney, F.R.S., 71
Hornstein (Dr. Carl), Death of, 247

Horse, Anatomy of the, Prof. Flower's Lecture on, 372
Hosack (James), Waterspout on Land, 79
Houghton (Rev. W.), "The Microscope," 173
Hovering of Birds, 366; Dr. Hubert Airy, 294, 336, 388, 412; Rev. W. C. Ley, 412; Duke of Argyll, 312, 387; H. T. Wharton, 312; Henry Cecil, 388; David Cunningham, 336; Dr. J. Rae, F.R.S., 337, 434; William Galloway, 337; C. T. Middlemiss, 337; W. Larden, 337; the Soaring of Birds, Lord Rayleigh, F.R.S., 534; Dr. Hubert Airy, 590
Hubrecht (Prof. A. A.), Hypothesis of Accelerated Development by Primogeniture and its Place in the Theory of Evolution, 279, 301
Huggins, (Dr. William, F.R.S.), Photographing the Corona, 199
Human Morphology, vol. I., H. A. Reeves, 124
Hungary: Silk Culture in, 209; Earthquake in, 423, 540
Hunterian Oration, the Biennial, 298
Hutton (F. W.), "Catalogues of New Zealand Diptera, Orthoptera, and Hymenoptera," 399
Huxley (Prof. T. H., F.R.S.), on Education, 396
Hydraulic Experiments, Major Allan Cunningham, 1
Hydra, Chlorophyll Corpuscles of, Prof. E. Ray Lankester, F.R.S., 82
Hydrocarbon Flame Spectrum, Origin of, Profs. Liveing and Dewar, 257
Hydrocarbons, Pollution of the Atmosphere by, J. J. Murphy, 241
Hydrogen Whistles, Francis Galton, F.R.S., 491
Hygienic Dress, &c., Exhibition, 443
Hygrometer of Saussure, 616
Hypophysis in Petromyzon Planeri, Genesis of, 91

Ignition, a Curious Case of, 509; Bertram Gwynne, 580
"Im Fernen Osten," A. H. Keane, 170
Implements, Palæolithic, of North-East London, Worthington G. Smith, 270
Incandescent Lamp, Changy, 209
Incandescent Electric Light, the Inventor of, W. M. Williams, 240
Incubation of the Ostrich, J. E. Harting, 480; Geo. J. Romanes, 480
India: The Indian Survey, Major Allan Cunningham, 97; Episodes in the Life of an Indian Chaplain, 99; Destruction of Life by Poisonous Snakes in, Sir J. Fayrer, F.R.S., 205; Destruction of Life by Wild Animals in, Sir J. Fayrer, F.R.S., 268; Indian Archegosaurus, R. Lydekker, 411
Indiarubber, Action of Light on, Prof. H. McLeod, F.R.S., 312
Industrial Education, J. Norman Lockyer, F.R.S., 248
Industrial Society of Berlin, Prizes offered by, 346
Infra-Red of the Spectrum, Work in the, Capt. W. de W. Abney, F.R.S., 15
Infusoria, a Manual of the, &c., W. Saville Kent, Prof. E. Ray Lankester, F.R.S., 601
Ingersoll (Ernest), Oyster Industry of the United States, 39
Ingleby (Miss C. R.), the Magnetic Storm and Aurora, 86
Ingram (William), the Recent Cold Weather, 530
Inhibition (of Structural Functions) and Action of Drugs thereon, Dr. T. L. Brunton, F.R.S., 419, 436, 467, 485
Inland Sea Canal, Lessep's Surveying Expedition for, 516
Insects, Common British, Rev. J. G. Wood, 124
Insects Injurious to Field Crops, Diagrams of, Miss E. A. Ormerod, 146
Insects Visiting Flowers, 498
"Insekten nach ihren Schaden und Nutzen, Die," Prof. Taschenberg, 172
Institution of Civil Engineers, 95, 143, 168, 355, 403, 428, 475, 595
Institution of Mechanical Engineers, 247, 351, 611
Institution of Naval Architects, 469
Intelligence in Animals: J. G. Grenfell, 292; J. Birmingham, 337; Dr. J. Rae, F.R.S., 366; D. Pidgeon, 366
Intermittent Spring in the Jachère, 63
International Electrical Conference, 18
International Fisheries Exhibition, 322, 389, 468, 496, 516, 536
Invertebrate Casts *versus* Algæ in Palæozoic Strata, 46; Dr. H. A. Hagen, 173
Irish Antiquities, Forged, W. J. Knowles, 54
Iron, Schirmwirkung of, Prof. Stephan, 325
Iron and Steel Institute, Annual Meeting of, 587
Irving (Rev. A.): Solar Radiation and Glacier Motion, 553; the Föhn, 605
Iserlohn, Fall of Meteorite at, 423
Isomerism of Albuminous Bodies, on the, Shigetaké Sagiura, 103
Isomerism, Physical, 301
Isanemones, 20
Italy: Exploration of the Mediterranean, Dr. J. Gwyn Jeffreys, F.R.S., 35; Biology in, 46; Earthquake in, 540
Ivens (R.) and H. Capello, Central and West Africa, 391
Izvestia, Caucasian, 517

Jablochkoff's New Element for Electro positive Plate, 114
Jachère, Intermittent Spring in the, 63
Jackson (B. Daydon), Translation of Gorkom's "Cinchona Culture," 287
Jamaica, Annual Report of Public Gardens and Plantations of, 90
Janssen (M.): Papers at the Academy of Science, 62; sent to Oran to observe the Transit of Venus, 89
Japan: Report on the Mint for 1882, 323; Japanese Students, 565; Education in, 617
Jeffreys (Dr. J. Gwyn, F.R.S.): Italian Explorations of the Mediterranean, 35; "Der Norske nord-hass-expedition," 1876–78, Hermann Friele, 457
Jevons Memorial, 113
Jews, Anthropology of the, B. Blechmann, 113
Johns Hopkins University Circulars, 180
Johnson (Surgeon-Major), Hawk Moth Larva, 126
Johnstrup (Prof.), Glacial Phenomena in Denmark, 373
Jones (Prof. T. Rupert), Catalogue of Fossil Foraminifera in British Museum, 173
Jones (W. M.), the Magnetic Storm and Aurora, 87
Joule (Dr., F.R.S.), Lime as a Purifier of Products of Combustion of Coal Gas, 496
Journal of Anatomy and Physiology, 93, 426
Journal of the Asiatic Society of Bengal, 71, 473
Journal of the Franklin Institute, 143, 281, 306, 473
Journal of Physiology, 23
Journal de Physique, 24, 143, 282, 353
Journal of the Royal Microscopical Society, 47, 426
Journal of Royal Society of New South Wales, 93
Journal of the Russian Chemical and Physical Society, 143, 521
Jupiter: Herr Kortazzi's Observations on, 209; Markings on, W. F. Denning, 365

Käfer Westfalens, Die, F. Westhoff, 239
Kayer (Dr.), Death of, 447
Keane (Prof. A. H.): Kreitner's "Im Fernen Osten," 170; Krao, the "Human Monkey," 245; "Wanderings in Baluchistan," Major-General Sir C. M. MacGregor, K.C.B., 359; "Travels and Adventures East of the Caspian during the years 1879-81," Edmond O'Donovan, 359; Aino Ethnology, 389; Dr. G. Nachtigal's "Sahara und Sudan," 408
Keller (Prof.), Animal Migrations due to Suez Canal, 181
Kenia (Mt.), Expedition, Departure of Geographical Society's, 161
Kent (W. Saville), Supposed Coral-eating Habits of Holothurians, 433; "A Manual of the Infusoria," &c., Prof. E. Ray Lankester, F.R.S., 601
Kentucky, Ferns of, John Williamson, 336
Kew Gardens, Visitors to, 230; Report for 1881, 322
Kharkoff, Science at, 213
Kiel Observatory, the Centre for Astronomical Telegrams, 276
Kinematics, Prof. G. M. Minchin, 239
Kinetic Theory of Chemical Actions, Hammond, 183
Kittler, "Normal Element" of Electromotive Force of Galvanic Combination, 325
Knorlein (Josef), Death of, 422
Knowles (W. J.), Forged Irish Antiquities, 54
Kobell (Dr. F. Ritter von), Death of, 88
Koenig (Dr.), on the Leukoscope, 95
Kohlrausch (Prof.), Electric Conductivity of Salts of Silver Haloid, 182
Kortazzi (Herr), Observations of Jupiter, 209
Kraevitch (M.): Electricity of Air, 183; Action of Gravity on Gases, 324
Krao, the "Human Monkey," 579; A. H. Keane, 245
Krause (Drs. Arthur and Aurel), Return of, 92
Kreitner's "Im Fernen Osten," A. H. Keane, 170
Kum-bum, the Sacred Tree of, W. T. Thiselton Dyer, F.R.S. 224

Lakes, Pelagic Fauna of Freshwater, 92

Lake Dwellings, Ancient Scottish, by Dr. Munro, Sir John Lubbock, F.R.S., 145
Lambeth Field Club, Report of, 90
Lamp, Lever's Arc, 274
Lamprey, the Skeleton of the, Prof. W. K. Parker, F.R.S., 330
Lampridæ, the Tracheæ in, 231
Landslip at Gudvangsören, 423
Langdon (Prof. R.), Transit of Venus, 159; Transit of Venus, 1882, British Expeditions, 179
Lankester (Prof. E. Ray, F.R.S.): Chlorophyll Corpuscles in Hydra, 87; "A Manual of the Infusoria," W. Saville Kent, 601
Larden (W.), Hovering of Birds, 337
Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies, on a Relation existing between, F. Trouton, 202
Lavoisier, Priestley, and the Discovery of Oxygen, G. F. Rodwell, 83, 100; C. Tomlinson, F.R.S., 53, 147
Layard (Consul E. L.), a Correction, 78; a Blue Meteor, 531; Lead Ore discovered in China, 588
Leaves, the Shapes of, Grant Allen, 439, 464, 492, 511, 552; F. O. Bower, 552
Leaves and their Environment, Grant Allen, 604
Leaves, Forms of, Sir John Lubbock, Bart., 605
Ledger (Rev. E.), the Sun, its Planets, and their Satellites, 309
Lecky (James), Singing, Speaking, and Stammering, 580
Lemstrom (Prof.), Experiments with Aurora Borealis, 322; Auroral Experiments, 389
Lena, Meteorological Expedition to the Mouth of the, 43, 372, 423, 496
Lenz (Prof. R.): Electric resistance of Mercury, 182; on Cosmical Dust collected by Marx, 422
"Lepidoptera of Ceylon," L. Reeve and Co., 150
Lepidoptera, Porritt's Yorkshire List of, 540
Leprosy in Norway, 423
Lessep's Surveying Expedition for Inland Sea Canal, 516
Leukoscope, Dr. Koenig on the, 95; Prof. Helmholtz's, 277
Lever's Arc Lamp, 274
Ley (Rev. W. Clement): "Weather Forecasts," 29; Magnetic Arrangement of Clouds, 53; A Curious Halo, 53; Hovering of Birds, 412
Libraries, Reference Catalogue of Derby Free, 62
"Light," Lewis Wright, 75
Lighting, Electric, Sir C. W. Siemens, F.R.S., 67
Lightning Conductors, the Efficacy of, 48
Liquid Films, Condensation of, on Wetted Solids, J. W. Clark, 370
Liquids, Arrangement for Measuring the Refractive Index of, Pittschikoff, 325
Lime as a Purifier of Products of Combustion of Coal Gas, Dr. Joule, F.R.S., 496
Lime-water, Protection of Oxygen, &c., from Admixture of Acid Vapour by, Dr. Loewe, 373
Linnean Society, 94, 118, 167, 259, 354, 379, 451, 523, 547, 619
Linnean Society of New South Wales, Proceedings of, 93, 215
Lion at Rest, 584
Listing (Prof. Johann Benedict): Death of, 228; Obituary Notice of, 316
Lithium Lines, on the Reversibility of, Profs. Liveing and Dewar, 499
Liveing (Prof.) and Prof. Dewar, on the Origin of the Hydrocarbon Flame Spectrum, 257; on the Reversibility of the Lithium Lines, 499; Absorption of Ultra-Violet Rays by Various Substances, 521
Lockyer (J. Norman, F.R.S.), Recent and Coming Total Solar Eclipses, 185; Hermann W. Vogel on Lockyer's "Dissociation Theory," 233; the Education of our Industrial Classes, 248
Lodes, Origin of Metalliferous, Prof. Sandberger, 90
Lodge (Prof. Oliver), the Ether and its Functions, 304, 328
Loewe (Dr.), Protection of Oxygen, &c., from Admixture of Acid Vapour by Lime-water, 373
London Mathematical Society (*see* Mathematical Society)
London, Palæolithic Implements of North-East, Worthington G. Smith, 270
London School Board, 346
Longridge (James A.), Wire Guns, 11, 35
Lovett, Lieut.-Col. Beresford, Itinerary Map from Teheran to Astrabad, &c., 323
Lubbock (Sir John, F.R.S.), The Senses of Bees, 46; Munro's "Ancient Scottish Lake Dwellings," 145; Forms of Leaves, 605; on the Sense of Colour amongst some of the Lower Animals, 618
Lunar Halo, A, J. Rand Capron, 78; T. P. Barkas, 103; S. A. Good, 150
Lupton (Sidney), Elementary Chemical Arithmetic, 76
Lydekker (Richard): Siwalik Carnivora, 293; Indian Archegosaurus, 411

Macdonald (Rev. D.), "Africana," 526
McEwen (J. P. M.), Comet 1882 *b*, 52
MacGregor (General Sir C. M.), "Wanderings in Baluchistan," Prof. A. H. Keane, 359
McIntosh (J. G.), Waterspouts on Land, 105
McKendrick (Prof. J. G.), Lectures on Physiological Discovery, 496
McLachlan (R., F.R.S.): Early Coltsfoot, 266; Obituary Notice of Prof. P. C. Zeller, 535
McLeod (Prof. H., F.R.S.), the Aurora, 99; Action of Light on Indiarubber, 312
Madan (H. G.), Complementary Colours at Niagara Falls, 174
Madras, Agriculture in, 607
Madrid, Snow in, 161, 229
Magnetic Arrangement of Clouds, C. H. Romanes, 31
Magnetic Storm and Aurora: W. H. M. Christie, F.R.S. Prof. C. P. Smythe, G. M. Whipple, J. R. Capron, R. H. Tiddeman, J. E. Clark, H. C. Gill, H. Robinson, A. M. Worthington, T. G. Elger, Miss C. R. Ingleby, C. H. Romanes, E. Brown, F. Stapleton, Rev. S. H. Saxby, Dr. H. Airy, Major J. Herschel, A.S.P., H. D. Taylor, T. P. Barkas, W. M. Jones, G. R. Vicars, Prof. O'Reilly, J. L. Dobson, 82 to 87
Magnetical Perturbations, Earth Currents during, 69
Magnetisation of Metals, M. Berson, 183
Main (Dr. J. F.), Practical Mechanics, John Perry, 456
Maklay (M. Mikluho), Russian Imperial Grant to, 92
Malay Archipelago, Meteorology of the, 79
Mammoth Skeleton found at Belgrade, 63
Man, Primitive, Prof. Owen on, Grant Allen, 31
Man and the Ape, Projection of Nasal Bones in, J. Park Harrison, 266, 294
Manchester Literary and Philosophical Society, 234, 524
Manilla, Hurricane at, 63
Manning (B.), the Comet, 29
Manures, Inquiry into Degree of Solubility requisite in, 325
Marcel-Deprez System, Transmission of Energy on, 372
Marcet (Prof. Francis), Death of, 587
Marine Surveying, a Treatise on, Rev. J. L. Robinson, 289
Mars, Rev. T. W. Webb, 203
Marshall (A. Milnes), New Dredging Implement, 11
Marshall (Major) and Nicéville (L. A.), Butterflies of India, Burmah, and Ceylon, 50
Marsipobranch Fishes, the Skeleton, W. K. Parker, F.R.S., 330
Martin (H. Newell) and W. A. Moule, Handbook of Vertebrate Dissections, 335
Martini (Signor), Sound produced by Outflow of Water, 183
Martyr (J.), Baird's Hare, 266
Marx, on Cosmical Dust Collected by, 422
Masheder (Thomas), Meteors, 483
Masters (Dr. M. T.), Double Flowers, 126
Mathematical Society, 71, 190, 282, 379, 499, 595; Annual Meeting, 19
Mathematical Society of Edinburgh, 346, 500, 595
Mathematics in America, J. W. L. Glaisher, F.R.S., 193
Mathematics in Scandinavia, 343
Matter of Space: Charles Morris, 349; Prof. A. S. Herschel, 458, 504
Maxwell (J. Clerk), the Life of, with a Selection from his Correspondence and Occasional Writings, and a Sketch of his Contributions to Science, Lewis Campbell, LL.D., 26
Mechanical Subjects, Papers on, Sir J. Whitworth, 208
Mechanics, Practical, John Perry, Dr. J. F. Main, 456
Mechanics, Teaching of Elementary, 580
Medicine in Russia, Popular, Dr. Slunin, 62
Medioscribed Circle, 607
Mediterranean, Italian Exploration of, Dr. J. Gwyn Jeffreys, F.R.S., 35; New Deep-Sea Fish from, Dr. Giglioli, 198
Megalania prisca, Prof. Owen, F.R.S., 118
Melbourne, Observatory at, 497
Melde (Prof.), Electric Reaction, 183
Meldola (R.), Difficult Cases of Mimicry, 481
Meliola, on the Genus, H. Marshall Ward, 234
Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux, 362

Memory, Diseases of, R. Ribot, Dr. G. J. Romanes, 169
Mendeléeff (Prof.), "Principles of Chemistry," 113
Mensuration, Useful Rules and Tables, W. J. M. Rankine, F.R.S., 431, 483
Mercury, Electric Resistance of, R. Lenz, 182
Mercury, Pressure of Saturating Vapour of, Herr Hertz, 183
Merian (Prof. Peter), Death of, 372
Meridian, Universal, 247, 516
Merrifield (Mary P.), Rabenhorst's "Kryptogamen-Flora von Deutschland, Oesterreich, und der Schweiz," 385
Metalliferous Lodes, Origin of, Prof. Sandberger, 90
Metals, Magnetisation of, M. Berson, 183
Metamorphic Rocks of Ross and Inverness, Henry Hicks, 474
Metamorphic Origin of Granite—Prehistoric "Giants," Duke of Argyll, 578
Metamorphism of Rocks, Recent Researches in, Dr. A. Geikie, F.R.S., 121
Meteorite: Fall of, at Iserlohn, 423; near Brescia, 469; the Alfianello, 511
Meteorology: the Observatory on Ben Nevis, 18, 39, 175, 399, 411, 487; Meteorology of the Malay Archipelago, 79; Meteorological Society, 95, 234, 307, 452, 570, 619; Proposed Exhibition of Meteorological Instruments, 373; Congress of Meteorologists, 539; Meteorological Observations at Mouth of Lena, Yurgens, 423; Meteorological Society of France, 497; Elementary Meteorology, R. H. Scott, F.R.S., 575
Meteors: the November, 43; W. L. Harnett on a, 103; an Extraordinary, B. R. Branfill, 149; Meteoric Auroral Phenomenon of November 17, 1882, 173; Remarks on and Observations of the, Dr. H. J. H. Groneman, 296; Rev. Stephen H. Saxby, 338; H. Dennis Taylor, 365; T. W. Backhouse, 412; A. Batson, 412; H. D. Taylor, 434; Great Meteor in Sweden, 423; a Meteor, R. W. S. Griffith, 434; the Large Meteor of March 2, 1883, W. F. Denning, 461; Meteors, Thos. Masheder, 483; Henry Cecil, 483; Meteor, E. Brown, 508; in Sweden, 517; a Blue, Consul E. L. Layard, 531; at Carlsruhe, 540; Instructions for Observing, 615; Number of, observed at Prossnitz, 617
"Mexico to-day," T. U. Brocklehurst, 503
Meyer (Dr. A. B.), Stockholm Ethnographical Exhibition, 371
"Micrographic Dictionary," J. W. Griffith and A. Henfrey, 160, 603
Microphone, the, 588
Microscope, a Search for "Atlantis" with the, Dr. Arch. Geikie, F.R.S., 25
"Microscope, the," Rev. W. Houghton, 173
Middlemiss (C. S.), Hovering of Birds, 337
Midland Boulders, Rev. W. Tuckwell on the, 346
Mikluho-Maclay (Baron) on New Guinea, 137, 184, 371
Milan, Intemperance in, Prof. Verga on, 347
Millar (W. J.): the Comet, 29; Rankine's "Rules and Tables," 483
Milne (Mr.), Proposed Study of Earthquakes in Japan, 463
Mimicry in Moths: Duke of Argyll, 125; Comm. D. Stewart, 314
Mimicry, Difficult Cases of: Alf. R. Wallace, 481; R. Meldola, 481; Dr. P. H. Stokoe, 508; H. J. Morgan, 531
Minchin (Prof. G. M.), Kinematics, 239
Mineralogical Society, 24, 451
Minor Planets, 148
Mint, Japanese, Report for 1882, 323
Mitchell Library, Glasgow, 615
"Mittheilungen der deutschen Gesellschaft," of Yokohama, 41
Mollusks, Edible, Acclimatisation of, Dr. J. G. Jeffreys, F.R.S., 511
Montgolfier Anniversary, 90, 517
Monuments, Ancient, Worthington G. Smith, 102
Moons, Mock, F. T. Mott, 606
Morgan (Augustus de), Memoir of, R. Tucker, 217
Morgan (C. L.), Suicide of Scorpions, 313, 530
Morgan (H. J.), Mimicry, 531
"Morphologisches Jahrbuch," 94, 473
Morphology Human, Vol. i., H. A. Reeves, 124
Morris, New Method of Producing Aluminium, 183
Morris (Chas.), the Matter of Space, 349
Morse, Memorial in Rome, 445
Moseley (Prof. H. N., F.R.S.): Von Graff's Monograph on the Turbellarians, 227; Incubation of Ostrich, 507
Moths, Mimicry in: Duke of Argyll, 125; Commander Duncan Stewart, 314
Motion, Optical Illusions of, Dr. Bowditch, 183
Motion, Laws of, Prof. P. G. Tait, 283
Mott (F. T.): the Sea Serpent, 293; Mock Moons, 606
Moule (W. A.) and H. Newell Martin, Handbook of Vertebrate Dissection, 335
Mouse-Mill Dynamo (Thomson's), J. T. Bottomley, 78
Mudballs, Formation of, Prof. G. H. Darwin, F.R.S., 507
Muirhead (Dr. H.), Aurora of Nov. 17, 1882, 315
Müller (Dr. Fritz): Animal Intelligence, 240; Two Kinds of Stamens with Different Functions in the same Flower, 364
Müller (Dr. Hermann), Two Kinds of Stamens with Different Functions in the same Flower, 30
Mundella (A. J., M.P.), on Education, 276
Munro (Dr.), "Ancient Scottish Lake Dwellings," by Sir John Lubbock, M.P., F.R.S., 145
Munro (J.), Swan Lamp Spectrum and Aurora, 173
Murcia, Earthquake at, 277
Murphy (J. J.): Complementary Colours, 8; Pollution of the Atmosphere, 241; Aurora, 434
Museum, New Natural History, 54
Museum, Warwick, 539
Muybridge's Zoetrope Pictures of Animals in Motion, 42; New Work on Motion in Man and Animals, 539
Myxinoids, the Skeleton of, Prof. W. K. Parkes, F.R.S., 330

Nachtigal (Dr. G.), "Sahara und Sudan," A. H. Keane, 408
Naphtha Wells at Ferghana, 445
Naples, Zoological Station in, J. T. Cunningham, 453
Napoli on Electricity as a Motive Power for Balloons, 517
Nasal Bones in Man and the Ape, Projection of, J. Park Harrison, 266, 294
Natural History Museum, The New, 54; Removal of Collections, 538
"Natural History, Another Book of Scraps principally relating to," Chas. Murray Adamson, 480
"Natural" Experiment in Complementary Colours, Chas. T. Whitmell, 266
Natural Selection and Natural Theology, Prof. Asa Gray, 291, 529; Dr. Geo. J. Romanes, F.R.S., 362, 528; J. B. Hannay, F.R.S., 364
Natural Science in Civil Service Examinations, 321
Navy, The British, Sir Thomas Brassey, 549
"Nearctic" as one of the Primary Zoological Regions, on the Value of the, Alfred R. Wallace, 482; Prof. Angelo Heilprin, 606
Nebulæ, Meridian Observation of, 324; New, 400, 446
Nervous System, Influence of, on the Regulation of Temperature in Warm-blooded Animals, 469
"Neurologie, Lehrbuch der," Dr. Schwalbe, 196
Newall (H. Frank), Internal Reflections in the Eye, 376
Newcastle-upon-Tyne Free Library, 615
Newcomb (Prof.), "Popular Astronomy," 373
New Guinea, Dr. Otto Finsch's Expedition to, 43; Baron Mikluho-Maclay on, 137, 184
New South Wales, Linnean Society of, 215
New Zealand: Earthworm in, 91; Pott's "Out in the Open," 172; Astronomy in, 276; Catalogues of New Zealand Diptera, Orthoptera, and Hymenoptera, F. W. Hutton, 399
"Niagara and other Famous Cataracts," G. W. Holley, 146
Niagara Falls, Complementary Colours at, H. G. Madan, 174
Nicéville (L. de) and Major Marshall, Butterflies of India, Burmah, and Ceylon, 50
Nicols (Arthur), Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals, Dr. Geo. J. Romanes, F.R.S., 333
Niger: W. A. Forbes's Zoological Expedition up the, 14; Col. Desbordes on Banks of, 424
Nilson, Thorium, 184
"Nomenclator Zoologicus," Sam. H. Scudder, 28
Nordenskjold (Baron): Discovery of North-East Passage, 422; his Proposed Greenland Expedition, 446, 496
Nordland, "Naturen" on the Old Silver Mines of, 347
"Normal Element" of Electromotive Force of Galvanic Combination, Kittler, 325
Norske nord-hass-expedition, 1876–78, Herman Friele, Dr. J. Gwyn Jeffreys, F.R.S., 457
North-East Passage, Discovery of, Baron Nordenskjold, 422
Norway: Leprosy in, 423; Dust Showers, 496; Norwegian Geodetical Operations, 224, 341; Norwegian Science Grants, 444
Nouveaux Mémoires de la Société Helvetique des Sciences Naturelles, 282

November Meteors, 43
Numbers, Theory of, Prof. H. J. S. Smith on, 564
Numerical Estimate of the Rigidity of the Earth, G. H. Darwin, F.R.S., 22

Observatories: Meteorological Observatory on Ben Nevis, 18, 39, 175, 399, 411, 487; Brussels, 445; Melbourne, 497
O'Donovan (Edmond), "Travels and Adventures East of the Caspian," Prof. A. H. Keane, 359
Olsta, Remarkable Mirage seen at, 616
Ommanney (Admiral), Aurora of Nov. 17, 139
Optical Illusions of Motion, Dr. Bowditch, 183
Optics, Physical, R. T. Glazebrook, F.R.S., 361
Opuntia Ficus-Indica, Abnormal Fruit of, Dr. A. Ernst, 77; Dr. M. T. Masters, 126
Ordnance Survey of Scotland, Completion of, 92
O'Reilly (Prof.), the Magnetic Storm and Aurora, 87
Orientalists, Congress of, 565
Ormerod (Miss E. A.), "Diagrams of Insects injurious to Field Crops," 146
Ornithologist in Siberia, 560
"Ornitologia della Papuasia e delle Molucche," Tommaso Salvadori, 577
Ostrich, Incubation of the: J. E. Harting, 480; George J. Romanes, 480; Prof. Moseley, F.R.S., 507
Oswald (Felix L.), "Zoological Sketches," Dr. Geo. J. Romanes, F.R.S., 333
"Out in the Open," T. H. Potts, 172
Owen (Prof. R., F.R.S.), on Primitive Man, Grant Allen, 31; *Megalania Prisca*, 118; on the Affinities of Thylacoleo, 354
Owl, Scream of Young Burrowing, S. Garman, 174
Oxygen, Discovery of, Lavoisier, Priestley, and the, G. F. Rodwell, 8, 100
Oyster Culture, 180
Oyster Fisheries of Denmark, Decline of, 346
Oyster Industry of the United States, 39

Palæolithic Implements of North-East London, Worthington G. Smith, 270
Palæolithic River Gravels: C. Evans, 8; William White, 53; T. K. Callard, 54; Worthington G. Smith, 102
Palæontology, Carboniferous Vertebrate, Notice of some Discoveries recently made in, T. Stock, 22
Palæozoic Strata, Invertebrate Casts *versus* Algæ in, 46
Palestine, Physical History of, Prof. Hull, F.R.S., 520
Palmieri (Prof. Marino), Death of, 41
Panama, Earthquake at, 248
Papuan Ornithology, Tommaso Salvadori, 577
Parallax, Stellar, 210
Paris: Academy of Sciences, 24, 48, 72, 95, 119, 144, 168, 192, 229, 235, 260, 283, 308, 332, 356, 380, 404, 428, 475, 548, 572, 596, 620; Annual Meeting of, 538; Disposal of the Sewage of Paris, 133; Second Inundation at, 247; Compte Rendus of the Geographical Society, 401; Bulletin of, 424; Electric Lighting in, 516
Parker (Rev. Dr. G. W.), Umdhlebi Tree of Zululand, 7
Parker (W. K., F.R.S.), The Skeleton of Marsipobranch Fishes, 330
Parkes Museum, First General Meeting, 19
Parrakeet, The Uvæan, 417
"Pathological Anatomy and Pathogenesis, a Text-Book of," Ernst Ziegler, 477
Patent Bill, the New, 587
Pavy (Dr. F. W., F.R.S.), Physiology of the Carbo-hydrates in the Animal System, 618
Pedara in Sicily, Earthquake at, 567
Pelagic Fauna of Freshwater Lakes, 92
Peripatus Capensis, Prof. F. M. Balfour on, 215
Peronosporæ, New Vine Parasite, 133
Perry (John), Practical Mechanics, Dr. J. F. Main, 456
Persia, Colonel Lovett's Itinerary Map from Teheran to Astrabad, 323
Perugia, Earthquake in, 90
Petermann's "Mittheilungen," 92, 401, 541
Petrie (W. M. F.), Aurora of Nov. 17th, 1882, 139, 315
Petromyzon Planeri, Genesis of Hypophysis in, 91
Pfaundler (Prof.), The Inventor of Continuous-Induction Dynamo-Electric Machine, 517
Philippine Islands, Typhoon in, 181
Phillips (H. A.), Pollution of the Atmosphere, 127, 206
Phosphorus, the Glowing of, 301
Photographing with the Electric Light, Priority in, 276
Photography, Astronomical, Edward C. Pickering, 556
Photometer, Wedge and Diaphragm, 201
Photometric Measurements of Sun, Moon, Cloudy Sky, and Electric and other Artificial Lights, Sir William Thomson, 277
Phylloxera Destruction, Commission of the French Academy, 89
Phylloxera in Chambery, 133
Physical Notes, 324
"Physical Optics," R. T. Glazebrook, F.R.S., 361
Physical Society, 95, 143, 215, 354, 427, 452, 500
Physical and Chemical Properties of Simple and Compound Bodies, Relation between, De Heen, 422
"Physics of the Earth's Crust," Rev. O. Fisher, 76
"Physics in Pictures," &c., 551
Physics in Russia during the last Ten Years, N. Hesehus, 567
Physiological Discovery, McKendrick Lectures on, 496
Pickering (Edward C.), Astronomical Photography, 556
Picou (R. W.), "Manuel d'Électronomie Industrielle," 146
Pictet (Raoul), his "Rapid Vessel," 398
Pidgeon (D.), Intelligence in Animals, 366
Pile-dwellings, Bobenhausen, 160
Piltschikoff, Arrangement for Measuring the Refractive Index of Liquids, 325
Pitt-Rivers Collection, the, 346, 461.
Planet, Inter-Mercurial, A. F. Goddard, 148
Planets, Minor, 248; No. 228, 518
Planté and Faure Accumulators, Chemistry of the, Dr. J. H. Gladstone, F.R.S., and Dr. A. Tribe, F.R.S., 583
Plants, Influence of "Environment" on, W. T. Thiselton Dyer, F.R.S., 82; Howard Fox, 315
Plants, Formic and Acetic Acid in, 91
Plants, Cultivated, Origin of, A. de Candolle, 429; Vilmorin Andrieux, 429
Pogge (Dr.), German African Expedition, 92
Poggendorff's "Fall Machine," 300
Polakoff's Explorations, 424
Polar Exploration, Swedish and Dutch Expeditions, 299
Pollock (E.), Aurora of Nov. 17, 141
Pollution of Rivers, &c., German Society for Prevention of, 89
Pollution of the Atmosphere, H. A. Phillips, 127
Porritt's Yorkshire List of Lepidoptera, 540
Porro (Francis), Professor Schiaparelli on the Great Comet 1882 *b*, 533
Port (Dr. Arnold Dodel-), "Atlas der Physiologischen Botanik," 617
Post-Tertiary, a Possible Cause of the Extinction of the Bones of the, S. Garman, 313
Potato Disease, A. S. Wilson, 5[illegible]3
Potts (T. H.), "Out in the Open," 172
Praxinoscope, Reynaud's New Projection, 60
Preece (W. H., F.R.S.), the Progress of Telegraphy, 390; "Effects of Temperature on Electromotive Force and Resistance of Batteries," 426
Prehistoric Animals, Remains of, discovered at Andernach, 445
Prehistoric "Giants"—Metamorphic Origin of Granite, Duke of Argyll, 578
Prejevalsky (Colonel), Exploration of Central Asia, 133
Preston (S. Tolver), "Ether and its Functions," 579
Priestley, Lavoisier, and the Discovery of Oxygen, G. F. Rodwell, 8, 100; C. Tomlinson, F.R.S., 53, 147
Primitive Man, Professor Owen on, Grant Allen, 31
Primogeniture, the Hypothesis of Accelerated Development by, and its Place in the Theory of Evolution, Prof. A. A. Hubrecht, 279, 301
Prince (E. E.), Threatened Extinction of the Elephant, 509
Princeton School of Science, Longitude of, 248
Princeton Scientific Expedition (No. 3), 323
Pringsheim (Dr. N.), "Jahrbücher fur wissenschaftlich Botanik," Prof. W. R. McNab, 502
Prisms, Direct-vision, 182
Projection Praxinoscope, Reynaud's New, 60
Prossnitz, Number of Meteors observed at, 617
Putnam (Prof. F. W.), on American Antiquities, 277

Quain's Elements of Anatomy, 196
Quarterly Journal of Microscopical Science, 47, 353
Quiberon, Discovery of Dolmen at, 540

Rabenhorst's "Kryptogamen-Flora von Deutschland, Oesterreich, und der Schweiz," Mary P. Merrifield, 385
Rabies, New Facts concerning, 192

Radiation, Terrestrial, Prof. Tyndall's Observations, Dr. A. Woeikof, 460
Radiometer, Prof. Ravelli on Educational Uses of, 444
Rae (Dr. J., F.R.S.): Hovering of Birds, 336; Intelligence in Animals, 366; the Sea Serpent, 366; British Circumpolar Expedition, 508
Railway Geology—a Hint, 8
Railways, Electric, 338; Prof. W. E. Ayrton, 235
Rainfall, British, G. J. Symons, F.R.S., 149
Rains, Heavy, in France and Algeria, 229
Rankine (W. J. M.), Useful Rules and Tables Relating to Mensuration, 431; W. J. Miller, 483
Ransome (Arthur), Fertilisation of the Speedwell, 149, 223
"Rapid Vessel," Pictet's, 398
Rattlesnake, Scream of Young Burrowing Owl like Warning of, S. Garman, 174
Ratzel (Dr. F.), Anthropo-Geographie, 125
Ravelli (Prof.), Educational Uses of Radiometer, 444
Rayleigh (Lord, F.R.S.): the Soaring of Birds, 534; Distribution of Energy in the Spectrum, 559
Rays, Ultra-Violet, Absorption of, by various Substances, Professors Liveing and Dewar, 521
Reaction, Electric, Prof. Melde, 183
Reale Istituto Lombardo di Scienze e Lettere, 282, 354, 473, 521
Redman (J. B.), River Thames Abnormal High Tides, 6
Reeve and Co. (L.), "Lepidoptera of Ceylon," 150
Reeves (H. A.), Human Morphology, vol. i., 124
"Réforme" Telegraphic Communication with London, 469
Refractive Index of Liquids, Arrangement for Measuring, Piltschikoff, 325
Regel (Dr. L. E.), Central Asian Exploration, 446
Rein (Dr. J. J.), Aino Ethnology, 365
Reinhardt (Prof. J. R.), Death of, 41, 61
Rendiconto dell' Accademia delle Scienze di Bologna, 189
Riviera, Unprecedented Cold in the—Absence of Sunspots, C. J. B. Williams, 551
Revue Internationale des Sciences, 426
Revue Internationale des Sciences Biologiques, 189
Reymond (Prof. E. du Bois), Darwin and Copernicus, 557
Reynaud's New Projection Praxinoscope, 60
Rheolyzer, Wartmann's, E. von Fleische, 127
Rhone Glacier Studies, Prof. Forel on Recent, 183
Rhytina Stelleri, Crania of, 347
Ribot's Diseases of Memory, Dr. G. J. Romanes, F.R.S., 169
Rigidity of the Earth, Numerical Estimate of the, G. H. Darwin, F.R.S., 22
Riley (Dr. C. S.), Hibernations of *Aletia xylina* in United States, 214
Rip van Winkle, a Modern, Saltburne, 127
Ristori (E.), Orbit of Comet 1882 *b*, 388
River Gravels, Palæolithic, C. Evans, 8
Rivista Scientifico-Industriale e Giornale del Naturalista, 94, 282, 521
Robert's Tide Tables, 230
Roberts (Prof. W. Chandler, F.R.S.), Hardening and Tempering Steel, 598
Robinson (H.), the Magnetic Storm and Aurora, 85
Robinson (Dr.), Memorial of, 112
Robinson (John), Flora of Essex County, Massachusetts, 173
Robinson (Rev. J. L.), a Treatise on Marine Surveying, 289
Robinson (W. H.), the Zodiacal Light (?), 605
Rocks, Recent Researches in Metamorphism of, Dr. A. Geikie, F.R.S., 121
"Rockies, Camps in the," W. A. Baillie-Grohman, 551
Rodwell (G. F.): Lavoisier, Priestley, and the Discovery of Oxygen, 8, 100; Notes of Travel in Sardinia, 342
Rogers (Prof. James E. T.), Ensilage in America, Prof. J. Wrightson, 479
Romanes (C. H.): Magnetic Arrangement of Clouds, 31; the Magnetic Storm and Aurora, 86
Romanes (Dr. G. J., F.R.S.), Ribot's Diseases of Memory, 169; on Natural Selection and Natural Theology, Prof. Asa Gray, 291, 362, 528; "Zoological Sketches," Felix L. Oswald, 333; "Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals," Arthur Nicols, 333; "The Vampire Bat," 412; Incubation of the Ostrich, 480; Benevolence in Animals, 607
Roorkee Hydraulic Experiments, Major Allan Cunningham, 1
Rowan (Vice-Admiral), Elements of the Great Comet 1882 *b*, 226
Rowan (D. J.), the Zodiacal Light (?), 605
Rowell (G. A.), Experiments on Aurora, 443
Rowland (Prof.), Concave Gratings for giving a Diffraction Spectrum, 95
Royal Asiatic Society (North China Branch), Journal of, 161
Royal Commissioners for Technical Education, visit to Birmingham, 469
Royal Geographical Society, 323
Royal Horticultural Society, 119, 523, 570
Royal Institution, Lecture Arrangements, 112, 495
Royal Society, 118, 143, 189, 215, 234, 257, 330, 354, 376, 426, 450, 473, 499, 521, 568, 618; Names Nominated for the Council, 40; Award of Medals, 61; Address of the President, 134, 162; New Fellows, 615
"Rules and Tables," Rankine's, 431; W. J. Millar, 483
Russell, H. C., the Comet 1882 *b*, 56
Russia, Popular Medicine in, Dr Slunin, 62; Forest Fires in, 113; Russian Chemical and Physical Society, 444; Russian Geographical Society, 424; Glacial Formations of, 497; Physics in, during the last ten years, N. Hesehus, 567
Russow (E.), on Sieve-Tubes, 366
Rye, E. C., "The Zoological Record" for 1881, 310

Sabine's New Wedge and Diaphragm Photometer, 201
Sachs (Julius), "Text-Book of Botany, Morphological and Physiological," Prof. E. P. Wright, 263
Safety Lamp, Prize offered for New, 496
Sagiura (Shigetaké), on the Isomerism of Albuminous Bodies, 103
"Sahara und Sudan," by Dr. G. Nachtigal, Prof. A. H. Keane, 408
Saharunpur Botanical Gardens, Report, 588
St. Petersburg Society of Gardening, 19
Salez, Discovery of Bronze Hatchet at, 540
Salmon and Herring Fisheries, 442
Salvadori (Tommaso), "Ornitologia della Papuasia e delle Molluche," 577
Sampson (Commander), the Comet 1882 *b*, 108, 266
Sandberger (Prof.), Origin of Metalliferous Lodes, 90
Sanitary Associations, Reports of, 423
Sanitary Research, Grocers' Company Scheme, 495, 515
Sanitary Science, the Scheme of the Grocers' Company for the Encouragement of Original Research in, 574
Sap-flow, F. M. Burton, 530
Saporta (Marquis de), Fossil Algæ, 514
Sardinia, Notes of Travel in, G. F. Rodwell, 342
Sarepta, the Stones of, 231
Saturn's Ring, Cassini Division of, 374
Saxby (Rev. S. H.): the Magnetic Storm and Aurora, 86; the Aurora, 100; Meteor of November 17, 338
Scale, Equal Temperament of the, C. B. Clarke, 240
Scandinavia, Mathematics in, 343
Schliemann (Dr.), Proposed Excavations at Athens, 276
Schaeberle (Prof.), Method for Observing Artificial Transits, 67
Schiaparelli (Prof.), on the Great Comet 1882 *b*, Francis Porro, 533
Schirmwirkung of Iron, Prof. Stephan, 325
Schmidt's Cometary Object, 20; Variable Star near Spica, 617
Schriften der Physicalisch-Okonomischen Gesellschaft zu Konigsberg, 521
Schwalbe (Dr.) "Lehrbuch der Neurologie," 196
Schwarz (Herr), Action of Zinc on Sulphur, 184
Science and Theology, 337
Scientific Heresies in China, 342
Scientific Renown, the Thirst for, 285
SCIENTIFIC WORTHIES:—William Spottiswoode, P.R.S. (*with a Portrait*), 597
Sclater (P. L., F.R.S.), the High Springs of 1883, 529
Scorpions, Suicide of, C. L. Morgan, 313, 530
Scotch Universities Bill, 565, 573
Scotland: Completion of Ordnance Survey of, 92; New Appointments of Geological Survey, 246
Scott (R. H., F.R.S.), "Elementary Meteorology," 575
Scott (Major-Gen. H. G. D., F.R.S.), Death of, 587
Scottish Lake Dwellings, Ancient, by Dr. Munro, Sir John Lubbock, F.R.S., 145
Scottish Review, 399
Scottish Meteorological Society, Half-yearly General Meeting, 469
Scudder (Sam. H.), "Nomenclator Zoologicus," 28
Scypho-Medusæ, Habits of, Surgeon-Major H. B. Guppy, 31
Sea Lion, the Cape, 415
Seabroke (Geo. M.), Comet 1882 *b*, 4, 52

Sea Serpent, the, F. T. Mott, 293; Joseph Sidebotham, 315; W. Barfoot, 338; Prof. W. Steadman Aldis, 338; Dr. J. Rae, F.R.S., 366
Seals in the Baltic, 133
Sea-shore, Apparent Bird Tracks by, 91
Secchi (Father), Monument to, 298
Seebohm (Henry), Ornithologist in Siberia, 560
Seismographic Apparatus, Gray and Milne's, 547
Selenka (Prof. E.), Sipunculacea, 133
Serpieri (Prof. A.), "Il Potentiale Elettrico nell' Insegnamento dell' Elettrostatica," 312
Serravallo, Double Action Mercury Air-pump, 324
Sextants, New Apparatus for Testing, 473
Shadows after Sunset, E. D. Archibald, 77; Prof. Dier, 150; J. Rand Capron, 182
Sheep, Blanford's, 415
Sheffield Free-Libraries Report, 19
Shetland, Severe Weather in, 443
Shulachenko's Experiments with Telephones, 445
Siberia, Proposal for Publication of General Description of, 182
Siberian Aborigines, Yadrintseff, 541
Siberian Flora, 445
Siberia in Europe, Henry Seebohm, 560
Sidebotham (Joseph), the Sea Serpent, 315
Siemens (Sir Charles W.), Electric Lighting, the Transmission of Force by Electricity, 67; and Dr. Percy, Presented with the Freedom of the Company of Turners, 276; Electrical Transmission of Force and Storage of Power, 518
Sieve Tubes, E. Russow, 366
Silesia, Earthquake in, 400
Silk Culture in Hungary, 209
Silver, Electric Conductivity of Haloid Salts of, Prof. Kohlrausch, 182
Simondsia paradoxa, Dr. Cobbold, 547
Singing, Speaking, and Stammering, W. H. Stone, 509, 531, 558, 580; James Lecky, 580
Sipunculacea, Prof. E. Selenka, 133
Siwalik Carnivora, Richard Lydekker, 293
"Skin-vision" of Animals, 399
Skobeleff (General), the Weight of his Brain, 347
Slunin (Dr.), Popular Medicine in Russia, 62
Smith (C. S.), Sir G. B. Airy on the Forth Bridge, 99
Smith (F. S.): a Modification of the Gold Leaf Electroscope, 102
Smith (Prof. H. J. S.): Death of, 371; Obituary Notice of, Dr. W. Spottiswoode, F.R.S., 381; his Mathematical Papers and Memoirs, 443; and the Representation of a Number as a Sum of Five Squares, 538, 564, 565, 587
Smith (Leigh), Gift to the Royal Geographical Society, 323
Smith (Robert H.), "Cutting Tools Worked by Hand and Machine," 577
Smith (Worthington G.), Ancient Monuments, 102; Palæolithic Gravels, 102; Palæolithic Implements of North-East London, 270
Smoke Abatement, Dr. E. Frankland, F.R.S., 407
Smoke Abatement Institution, National, 443
Smyth (Prof. C. Piazzi), the Magnetic Storm and Aurora, 83; the Peak of Teneriffe Active again, 315
Snake Bite, Death from, in Bombay, Sir Joseph Fayrer, F.R.S., 556
Snakes, Destruction of Life in India by Poisonous, Sir J. Fayrer, F.R.S., 205
Snowballs, Natural, a Remarkable Phenomenon, S. Hart, 483
Snow Rollers, G. J. Symons, F.R.S., 507; F. W. Grey, 507
Soda Flames in Coal Fires, Major J. Herschel, 78
Soda, Heating by Acetate of, 344
Soda Industry, Present Condition of, Walter Weldon, F.R.S., 401
Sodium as New Element for Electro-positive Plate, Jablochkoff, 114
Solar Corona, on Photographing the, Dr. Wm. Huggins, F.R.S., 199
Solar Eclipses, Recent and Coming Total, J. Norman Lockyer, F.R.S., 185
Solar Eclipse on May 6, Total, 248; English Expedition, 398, 567
Solar Radiation and Glacier Motion, Rev. A. Irving, 553
Solomon Islands, Anthropological Notes in the, H. B. Guppy, 607
Sound-vibrations of Solid Bodies (glass cylinders) in Contact with Liquids, Auerbach, 325
Space, the Matter of, Charles Morris, 349; Prof. A. S. Herschel, 458, 504
Spatzier (Johann), Death of, 422
Speaking, Singing, and Stammering, W. H. Stone, 509, 531, 558, 580: James Lecky, 580
Spectrum Analysis: Work in the Infra-Red of the Spectrum, Capt. Abney, F.R.S., 15; Prof. Rowland's Concave Gratings for giving a Diffraction Spectrum, 95; Profs. Liveing and Dewar on the Origin of the Hydrocarbon Flame Spectrum, 257; Distribution of Energy in the Spectrum, Lord Rayleigh, F.R.S., 559
Speedwell, Fertilisation of, A. M. Stanley, 127, 174; A. Ransom, 149, 223
Speke and Grant's Zebra, Sir J. Fayrer, F.R.S., 604
Spencer's (Herbert) Philosophy, Section founded for Study of, at Birmingham, 587
Spica, Schmidt's Variable Star near, 617
Spider, New Species of African, 348
Spitzbergen, Swedish Expedition to, 243
Spitzbergen Geological and Palæontological Collections, 588
Sponges, Australian Freshwater, 91
Spörer (Prof.) on the Transit of Venus, 284
Sportsman's Handbook to Practical Collecting, &c., Rowland Ward, 146
Spottiswoode (Dr. W., P.R.S.): Address to Royal Society, 134, 162; Obituary Notice of Prof. Henry Smith, 381; "Scientific Worthies," 597
Spring in the Jachère, Intermittent, 63
Spring (M. W.), Thunderstorms, 133
Springs, High, of 1883, P. L. Sclater, F.R.S., 529
Squares, Prof. H. J. S. Smith and the Representation of a Number as a Sum of Five, 538, 564, 565, 587
Stamens, Two Kinds of, with Different Functions in the same Flower, Dr. Hermann Müller, 30; Dr. Fritz Müller, 364
Stammering, Singing, Speaking and: W. H. Stone, 509, 531, 558, 580; James Lecky, 580
Stanley (A. M.), Fertilisation of Speedwell, 127
Stanley (W. F.), Causes of Elevation and Subsidence of Earth's Surface, 523
Stapleton (F.): the Magnetic Storm and Aurora, 86; the Comet, 151
Stapley (A. M.), Fertilisation of Speedwell, 174
Stars: Star Maps in *Glasgow Evening Times*, 89; Measures of Double, 182; Binary, 518; ζ Cancri, 424; *p* Eridani, 589; Variable, 324, 400, 540; S Virginis, 424; U Cephei, Ceraski's, 424, 445; Supposed Variable μ Doradûs, 498; Schmidt's Variable near Spica, 617
Steel Plate Manufacture, 405
Steel, Hardening and Tempering, Prof. W. Chandler Roberts, F.R.S., 594
Stellar Parallax, 210
Stephan (Prof.), Schirmwirkung of Iron, 325
Stevenson (Thos.), Observations of Increase of Velocity of Wind with Altitude, 432; E. D. Archibald, 506
Stewart (Comm. D.), Mimicry in Moths, 314
Stock (T.), Notice of some Discoveries recently made in Carboniferous Vertebrate Palæontology, 22
Stockholm Ethnographical Exhibition, Dr. A. B. Meyer, 371
Stokoe (Dr. P. H.), Mimicry, 508
Stone Age in Japan, Weapons and Implements from the, 616
Stone (Prof. E. J.), Transit of Venus, 1882, British Expeditions, 177
Stone (W. H.), Singing, Speaking, and Stammering, 509, 531, 558, 580
Storage Batteries, Chemistry of, Dr. E. Frankland, F.R.S., 568
Strahan (A.), Movements of Air in Fissures and the Barometer 375, 461
Stress, Clerk Maxwell on, 314
Strong (George), Improved Feed-water Heater and Purifier, 90
Suez Canal, Animal Migrations due to, Prof. Keller, 181
Sulphur, Action of Zinc on, Schwarz, 184
"Sun: its Planets and their Satellites," Rev. E. Ledger, 309; Eclipse of the, 346; Ignition by Sunlight, Major Herschel; E. H. Verney, 531; Shadows after Sunset, E. D. Archibald, 77; J. Rand Capron, 102; Prof. Dier, 150; a Mock Sunset, 78; Wolf and Faye on Periodicity of Sunspots, 235; Absence of Sunspots—Unprecedented Cold in the Riviera, C. J. B. Williams, 551
Sunflowers at Night, the Reversion of, C. A. White, 241
Surveying, Marine, a Treatise on, Rev. J. L. Robinson, 289
Svanberg (Dr. Gustav), Death of, 132

Swan Lamp Spectrum and the Aurora, J. Rand Capron, 149; J. Munro, 173
Sweden, Auroræ in, 113, 496; Darwin Memorial and the People of, 275; Great Meteor in, 423, 517; Swedish Expedition to Spitzbergen, 1882, 243; New Swedish Arctic Expedition, 400
Sweden and Norway, Electrical Disturbances in, 89
Switzerland: Swiss Geological Society, 132; Avalanche in Western, 181; Earthslip in, 209; Heavy Rain in, 209; Fishes of Switzerland, Dr. Victor Fatio, 220; Electric Lighting in, 497
Sydney: Linnean Society of New South Wales, 308, 355, 475, 571, 619
Symons (G. J., F.R.S.), British Rainfall, 149; Snow Rollers, 507
Szechenyi's Travels, A. H. Keane, 170

Tacchini (Prof.), Aurora of November 17, 139
Tait (Prof. P. G.), Paper on the Laws of Motion, 283
Talisman Deep-Sea Expedition, 587
Tapetum, the, in the Retina of Mammals, 216
Tapir, the Malayan, 151
Taschenberg (Prof.), "Die Insekten nach ihren Schaden und Nutzen," 172
Tashkend, Earthquake at, 617
Tawney (E. B.): Death of, 247; Obituary Notice of, 295
Taylor (Rev. C. J.), the Aurora, 99
Taylor (H. D.): the Magnetic Storm and Aurora, 87; Aurora of November 17, 140; Meteor of November 17, 365
Taylor (J. E.), Origin of our Vernal Flora, 7
Teaching of Elementary Mechanics, 580
Technical College, the Finsbury, 181, 318, 425, 495
Technical Education Commission, 516
Telegraph Extension in China, 588
Telegraphy, Advance in Use of, for French Newspapers, 372; the Progress of, W. H. Preece, F.R.S., 390
Telephones, Shulachenko's Experiments with, 445
Telephonic Communication, Novelty in, 515
Temperament, Equal, of the Scale, C. B. Clarke, 240
Teneriffe, the Peak of, active again, Prof. C. Piazzi Smyth, 315
Terrestrial Radiation, Note on, John Tyndall, F.R.S., 377; Dr. A. Woeikof, 460
Tertiary History of the Grand Cañon District, Dr. Arch. Geikie, F.R.S., 357
Thames, River—Abnormal High Tides, J. B. Redman, 6
Thames, Monthly Means of the Temperature of the Water of, Sir G. B. Airy, F.R.S., 189
Theology, Natural, Selection and Natural, Prof. Asa Gray, 291, 529; Dr. Geo. J. Romanes, F.R.S., 362; J. B. Hannay, F.R.S., 364
Theology, Science and, 337
Thermometer, an Improved Air, 43
Thermometers, Liquid, Expansion of Bulbs of, Prof. Govi, 209
Thibet, Native Exploration of, Sir H. Rawlinson, 323
Thomson's Mouse-Mill Dynamo, J. T. Bottomley, 78
Thomson (Sir William, F.R.S.): Photometric Measurements of Sun, Moon, Cloudy Sky, and Electric and other Artificial Lights, 277; Gyrostatics, 548
Thorium, Nilson, 184
Thunderstorms, M. W. Spring, 133
Thylacoleo, on the Affinities of, Prof. Owen, F.R.S., 354
Ticks, 531; Dr. T. Spencer Cobbold, F.R.S., 552; Rev. L. Blomefield, 552
Tiddeman (R. H.), the Magnetic Storm and Aurora, 84
Tides, Abnormal High—in the Thames, J. B. Redman, 6
Tides, Great, Prof. R. S. Ball, F.R.S., 201
Tides, Geological Traces of Great, J. G. Grenfell, 222
Timehri, 539
Times, the, on Science, 566
Timor-Laut, H. O. Forbes's Visit to, 159
Tissandier's Electromagnetic Engine for directing Balloons, 343
Tomlinson (C., F.R.S.), Priestley and Lavoisier, 53, 147
Torpedo, Prof. Fritsch on the, 453
Traill (D.), Transit of Venus, 159
Tram-car, Electrically moved, Successful Trial of a, 470
Transactions of the New York Academy of Sciences, 330
Transit of Venus, 112, 154, 253, 540; Prof. Harkness on the, 114; British Expeditions, E. J. Stone, F.R.S., Prof. Langley, John Birmingham, 177; C. J. B. Williams, F.R.S., 197; in Algiers, 208; French Expedition, 208; German Expedition, 208; Prof. Edgar Frisby, 266; Samuel Hart, 483
Transits, Method for Observing Artificial, Prof. Schaeberle, 67
Transmission of Energy on the Marcel-Deprez System, 372
"Travels in India," 21
Tresca (M.), Papers on Electrical Measures, 62
Tribe (Dr. A., F.R.S.), Chemistry of the Planté and Faure Accumulators, 583
Tricycles propelled by Electricity, 19
Tromholt (Sophus), on the Aurora Borealis, 394
Trotting Horse, Evolution of the American, W. H. Brewer, 609
Trouton (F.), on a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies, 292
Trouvé's Batteries, 41, 42
Tubercle, Bacillus of, 492, 563
Tucker (R.), Memoir of Augustus de Morgan, 217
Tuckwell (Rev. W.), on the Midland Boulders, 346
Tunnel, Channel, Prof. W. Boyd Dawkins, F.R.S., 338
Turbellarians, Prof. von Graff's Monograph on the, Prof. H. N. Moseley, F.R.S., 227
Tylor (Dr. E. B., F.R.S.), "The Burman," 6
Tympanic Membrane, Function of Membrana Flaccida of, J. M. Crombie, 129
Tyndall (Prof. John, F.R.S.), "Note on Terrestrial Radiation," 377; Dr. A. Woeikof on, 460
Typhoon in Philippine Islands, 181

Ultra-Violet Rays, Absorption of, by various Substances, Professors Liveing and Dewar, 521
Umdhlebi Tree of Zululand, W. T. Thiselton Dyer, F.R.S., 7; Rev. Dr. G. W. Parker, 7, 32
United States, Oyster Industry of the, 39; New Aquarium at Wood's Hole, 347
Universities Bill, Scotch, 565, 573
University and Educational Intelligence, 47, 71, 93, 117, 142, 167, 214, 233, 281, 306, 330, 353, 402, 449, 472, 498, 547, 594, 618
Ural Mountains, Russian Exploration of, 446; Severe Weather in, 539
Uric and Hippuric Acid, Dr. A. B. Garrod, F.R.S., 451
Uvæan Parrakeet, 417

Valais, Earthquake in the, 181
"Vampire Bat": Thos. Workman, 411; A. W. Auden, 411; G. J. Romanes, F.R.S., 412
Variable Stars, 324, 400, 540; S Virginis, 424; U Cephei, Ceraski's, 424, 446; Supposed Variable μ Doradûs—a Spurious Star, 498; Schmidt's Variable Star near Spica, 617
Varna, Alleged Wreck of the, 114
Vega Expedition, the Fossil Plants collected by, 347
Vega Gold Medal conferred on Mr. Stanley, 422
Vegetable History, Tables of, D. P. Penhallow, 458
Velocity of the Wind, Diurnal Variations in the, E. Douglas Archibald, 461
Venus, Transit of, 112, 154, 253, 541; Observations in Paris of the, 113; Prof. Harkness, 114; W. de Fonvielle's "Periodical," 132; Duke of Argyll, Dr. R. S. Ball, F.R.S., Dr. W. Doberck, J. L. E. Dreyer, C. L. Wragge, W. F. Denning, D. Traill, H. Cecil, R. Langdon, 154 to 159; British Expedition, E. J. Stone, F.R.S., 177; Prof. Langley, 179; John Birmingham, 180; Prof. Cacciatore, 180; C. J. B. Williams, 197; German Expedition, 208; French Expedition, 208; United States Expedition, 246, 539; Prof. Edgar Frisby, 266; Prof. Spörer on the, 284; Samuel Hart, 483
Verhandlungen der Naturforschenden Gesellschaft in Basel, 71
Verhandlungen der Naturhistorischen Vereines der Preussischen Rheinlande und Westfalens, 521
Vernal Flora, Origin of our, J. E. Taylor, 7
Verney (E. H.), Ignition by Sunlight, 531
Vertebrate Dissection, Handbook of, H. Newell Martin and William A. Moule, 335
Vertebrate Palæontology, Carboniferous, Notice of some Discoveries recently made in, T. Stock, 22
Vicars (G. Rayleigh), the Magnetic Storm and Aurora, 87
Victoria (Philosophical) Institute, 143, 260, 283
Vienna Geographical Society, 229, 401
Vienna Imperial Academy of Sciences, 72, 95, 120, 260, 284, 524, 572
Vienna International Electrical Exhibition, 373, 444
Vilmorin-Andrieux, "Les Plantes Potagères," 429
Vine Parasite, New, 133
Vivisection, Facts and Considerations relating to, 542

Vivisection Bill, 549
Vogel (Hermann W.), Lockyer's Dissociation Theory, 233
Volatile Bodies, on a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of, F. Trouton, 292
Volcanic Ashes, Showers of, 516
Volta Prize, 89
Von Graff's Monograph on the Turbellarians, Prof. H. N. Moseley, F.R.S., 227
Volcanic Eruption in the Caucasus, 63
Volcanic Phenomena in Greek Archipelago, 445
Von Siebold, Memorial to, 41

Walker (C. V., F.R.S.), Death of, 228
Wallace (A. R.): Difficult Cases of Mimicry, 481; on the Value of the "Nearctic" as one of the Primary Zoological Regions, 482
Ward (H. Marshall), on the Genus *Meliola*, 234
Ward (Rowland), the Sportsman's Handbook to Practical Collecting, &c., 146
Warmbrunn, Fulgurite found near, 540
Wartmann's Rheolyzer, E. von Fleische, 127
Warwick Museum, 539
Washington Observatory, U.S., 300
Waste Paper in China, 588
Watch and Clockmaker's Handbook F. J. Britten, H. Dent Gardner, 76
Water, Sounds produced by Outflow of, Signor Martini, 183
Water-Analysis, American Researches on, 211, 231
Waterspouts on Land: James Hosack, 79; J. G. McIntosh, 103
Watson (Sir Thos.), Death of, 160
Watts (Arthur), the Comet, 5
Waves, Mountainous, in Channel in Calm Weather, 540
Weather, the: J. M. Fountain, 32; J. R. Capron, 198; Altitude and, Dr. Woeikoff, 223; the Recent Cold Weather, W. Ingram, 530; "Weather Forecasts," the Bishop of Carlisle, 4, 51; Rev. W. Clement Ley, 29; C. W. Harding, 79
Webb (Rev. T. W.), "Anomalous" Tail to the Comet 1882 *b*, 89; Mars, 203
Weights and Measures, 131
Weldon (Walter, F.R.S.), Present Condition of Soda Industry, 401
Westhoff (F.), "Die Kafer Westfalens," 239
Wetted Solids, Condensation of Liquid Films on, J. W. Clark, 370
Wharton (H. J.), Hovering of Birds, 312
Whipple (G. M.): the Magnetic Storm and Aurora, 83; New Apparatus for Testing Sextants, 473
Whistles, Hydrogen, Francis Galton, F.R.S., 491
White (Wm.), Palæolithic River Gravels, 53
White (W. H.), "The British Navy: its Strength, Resources, and Administration," Sir Thomas Brassey, 549
Whitehouse (F. Cope), "Is Fingal's Cave Artificial?" 285
Whitmell (Chas. T.), "Natural" Experiment in Complementary Colours, 266
Whitworth (Sir Joseph), Papers on Mechanical Subjects, 208
Whitworth (W. A.), the Churchman's Almanac for Eight Centuries, 386
Wiedmann (E.), Pre-fusional Chemical Transposition of Heated Water containing Salts, 183
Williams (Dr. C. J. B.): the Comet, 29, 110; Transit of Venus, 197; the Comet 1882 *b* during last Month, 197; Unprecedented Cold in the Riviera—Absence of Sunspots, 551
Williams (W. M.), the Inventor of the Incandescent Electric Light, 240
Williamson (John), Ferns of Kentucky, 336
Wilson (A. S.), Potato Disease, 523
Wind, Increase in Velocity of, with Altitude, E. D. Archibald, 243, 506; Thos. Stevenson, 432; Diurnal Variation of, on Open Seas and near and on Land, A. Buchan, 413; E. Douglas Archibald, 461
Winlock (W. C.), the Comet, 128
"Winners in Life's Race," Arabella Buckley, 51
Winter (Karl), Death of, 228
Wire Guns, James A. Longridge, 11, 35, 53
Wissmann and Pogge's African Expedition, 92, 341, 401
Woeikoff (Dr.): Altitude and Weather, 223; Terrestrial Radiation and Prof. Tyndall's Observations, 460
Wolf on Methods employed in Astronomical Physics, 372
Wolga, Discovery of Remains of Diluvial Mammals on, 373
Women Students, College Hall and Residence for, 229
Wood (Rev. J. G.), Common British Insects, 124
Workman (Thos.), The "Vampire Bat," 411
Worms, Charles Darwin on, 28
Worthington (A. M.), the Magnetic Storm and Aurora, 85; Influence of Vacuum on Electricity, 434
Wragge (Clement L.): Ben Nevis Observatory, 39, 487; Aurora, 54; Transit of Venus, 158
Wright (Prof. E. P.), "Text-Book of Botany, Morphological and Physiological," Julius Sachs, 263
Wright (Lewis), "Light," 75
Wrightson (Prof. J.), Ensilage in America, Prof. James E. T. Rogers, 479

Yadrintseff, Siberian Aborigines, 541
Yarrell (Wm.), A History of British Birds, Part xv., 98
Year-Book of Pharmacy, 1882, 361
Yoe (Shway), "The Burman," 5
Yorkshire College Students' Association, 19
Yorkshire List of Lepidoptera, Porritt's, 540
Yurgens' Meteorological Observations at Mouth of Lena, 423

Zebra, Speke and Grant's, Sir J. Fayrer, F.R.S., 604
Zeitschrift fur wissenschaftliche Zoologie, 94, 189, 426
Zeller (Philip Christoph), Obituary Notice of, R. McLachlan, F.R.S., 535
Ziegler (Ernst), "A Text-Book of Pathological Anatomy and Pathogenesis," 477; Death of, 566
Zinc on Sulphur, Action of, Schwarz, 184
Zodiacal Light (?), 580; W. H. Robinson, 605: Robert Dwarris Gibney, 605; E. Brown, 605; D. J. Rowan, 605
Zoological Expedition up the Niger, W. A. Forbes's, 14
Zoological Gardens, Additions to, 20, 63, 78, 91, 114, 133, 161, 182, 210, 230, 248, 277, 300, 324, 348, 374, 400, 424, 445, 470, 497, 517, 540, 589, 617
"Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals," Arthur Nicols, Dr. Geo. J. Romanes, F.R.S., 333
"Zoological Record for 1881," the, E. C. Rye, 310; Sydney J. Hickson, 366
Zoological Regions, Primary, on the Value of the "Nearctic" as one of the, Prof. Angelo Heilprin, 606
"Zoological Sketches," Felix L. Oswald, Dr. Geo. J. Romanes, F.R.S., 333
Zoological Society, 94, 190, 282, 307, 379, 451, 499, 570, 619
Zoological Society's Living Collection, Illustrations of New and Rare Animals of, 151, 415
Zoological Station in Naples, J. T. Cunningham, 453
Zoology in Japan, 614
Zululand, Umdhlebi Tree of, W. T. Thiselton Dyer, F.R.S., 7; Rev. G. W. Parker, 7, 32

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, NOVEMBER 2, 1882

HYDRAULIC EXPERIMENTS

Roorkee Hydraulic Experiments. By Major Allan Cunningham, R.E. Three vols. (Roorkee: Thomasen College Press, 1880-81.)

UNDER the direction of the Indian Government there have been constructed a number of canals, which, while reaching in transverse dimensions a size not much inferior to the Suez or North Sea Canal, have a far greater length and ramify into smaller channels of enormous total extent. Besides these, reservoir and river works have been carried out of the greatest magnitude. Hence the Indian Government has a most direct interest in the advancement of the knowledge of hydraulics. Not only must hydraulic formulæ be used in the design of hydraulic works, but also in regulating the distribution of a valuable commodity—irrigation water—on which large revenues depend. Yet down to a recent period the Indian Government has been content to avail itself of researches carried out in Europe and chiefly in France, and has made no use of its splendid opportunities for scientific hydraulic experiments. When at last hydraulic experiments on a large scale were sanctioned, involving a large expenditure it was very fortunate that the direction of them was intrusted to so very competent an officer as Major Cunningham. "Beaucoup de personnes croient que tout homme intelligent et instruit peut faire, sans grand travail de bonnes expériences; c'est une erreur qui a fait perdre beaucoup de temps et d'argent." So says M. Boileau, who is himself one of the most careful of hydraulic experimenters. Major Cunningham certainly does not think lightly of his work. He has enormous industry; he repeats his observations again and again; he studies every detail of his methods; he notes the opinions of all his predecessors in work of a similar kind, and discusses his results with great lucidity. If his experiments have furnished no strikingly novel laws, the fault is not his.

"The general result of this work may perhaps be considered in some ways disappointing, in that there are no brilliant results, no simple laws of fluid motion discovered, not even a new formula for mean velocity proposed" (p. 4).

It is certainly true that when Major Cunningham passes from discussing the details of practical methods, where he is always instructive, to purely scientific questions, to generalising laws from the results obtained on verifying accepted rules, he has a rather exceptional number of purely negative conclusions to state. It is almost amusing to find caution carried to the extent involved in printing as a general result of a considerable discussion that the value of the coefficient in the formula for the discharge of a stream "depends *probably* on the nature of the banks and bed, as well as on the hydraulic mean depth and slope." But nevertheless we believe the practical objects of the experiments have been obtained, and the outlay usefully incurred. Less of thoroughness at all events would have rendered the experiments useless, and although considering the scale of the experiments they seem at present rather less fruitful of definite results than might have been expected, yet it may be hoped that Major Cunningham has not made the most that can be made of his results. In time the new suggestion will come which will reduce to order the discordant observations. In the establishment of any new general conclusions or formulæ in the hydraulics of streams, this store of data will certainly be of the greatest value.

Of the magnitude of the work undertaken by Major Cunningham, it is difficult to give an adequate idea. It lasted over four years. The results include 565 sets of vertical velocity curve observations, each set including velocities taken three times at each foot of depth; 545 sets of rod float observations, each including six measurements of velocity; 581 sets of mean velocity observations, each including three measurements of velocity at ten to twenty points; 440 measurements of surface slope; besides many others. In addition to all this, the tabulation and computation of the results involved enormous labour. The printing of the results at Roorkee, whilst it must have involved greater trouble and responsibility than similar work in this country, seems to have been most efficiently and accurately done.

From the practical point of view Major Cunningham's book may be regarded as an exhaustive treatise on Float Gauging. All the more important observations were

VOL. XXVII.—No. 679

made by floats, and he has used these simple instruments in all their known forms, as surface floats, sub-surface floats, twin floats, and rod-floats. Every detail of the construction and use of these floats has been studied, their form, the length of run, the mode of marking the sections and float paths, and the precautions in taking the time. The sources of error are weighed, and in some degree the limits assigned beyond which the methods become unreliable. There will always be cases where the methods of float-gauging must be used, and no one who has work of this kind to do can afford to neglect Major Cunningham's directions. A few observations were, in fact, made with current meters. But the instruments used were of a type which must now be regarded as antiquated, and as to these Major Cunningham suggests no improvement which has not already been tried by the German engineers, who have, in fact, converted the current meter into a new instrument of precision.

It is not at all to be regretted that Major Cunningham adopted floats in his experiments. Even from the scientific point of view, if floats are at best a rough means of determining velocities, yet they are not liable as more complicated instruments are, if used without sufficient care or knowledge, to large and concealed errors. Hence float observations may always be used advantageously to check observations made in other ways. The progress of hydraulics suggests questions, for the solution of which float methods are inadequate, and the results obtained by Harlacher and Wagner seem to show that floats will be superseded by instrumental means of greater complication, but of far greater delicacy. But in truth in hydraulics no one method is free from objections and researches carried out by all methods, when sufficient care is exercised, will prove useful.

We may now pass to consider briefly the bearing of these experiments on some points of theory. Major Cunningham devotes Chapter VI. to a discussion of the unsteadiness of the motion of the water in ordinary streams. At each point the velocity varies in direction, and magnitude from instant to instant. The float-velocities taken on 50-feet runs, which are themselves mean velocities for a certain time and distance, vary from 10 to 30 per cent., so that to obtain the true mean velocity over any given float-path, something like fifty float observations are necessary. Recent current-meter observations show this variation of velocity still more clearly. The essential unsteadiness of the motion of water in streams was pointed out with the greatest clearness by M. St. Venant (1872), and the still more important fact that the motion is periodically unsteady, that is, that the variations occur periodically about a constant mean value, so that the average velocity for a sufficient but not very great length of time is sensibly constant. It is only this last fact which has rendered it possible, to apply the equations for steady motion to the actual motion of streams, and it is a pity that Major Cunningham has not adopted St. Venant's convenient term, mean local velocity, for the sensibly constant average velocity at each point of a stream. It is not the "interlacing of the stream lines" (p. 107), but the destruction of stream-line motion by eddying motions of quite another character, to which the unsteadiness seems to be due.

In Chapter VII. the observations of the surface-slope at different periods of the experiments are discussed, and it is here that we think may be discovered the one matter in which the conditions of the experiments were unsatisfactory, and in which they are markedly inferior to Bazin's small-scale experiments. Taking the Solani embankment and Solani aqueduct sites, at which the largest amount of work was done, we find that the experiments were made at about the centre of a ten-mile reach, terminated at the upper end by a regulator controlling the water-supply, and at the lower end by a fall where, by artificial means, the water-level was kept up to any desired height. The bed of the canal between these limits had originally the uniform slope of about a foot per mile. This original level is maintained at five points by masonry works, but between these the bed is irregularly scoured out to an extent which must have made very sensible variations of velocity within distances of a mile. At the tail of the reach is a weir standing five feet above the level of the bed, the crest of which was further raised by temporary obstructions of a height sometimes reaching five feet more. Hence the whole height of obstruction was often greater than the whole depth of water at the site of the gaugings. Under these circumstances the slope of the water surface varied, being generally quite different in the part of the reach above the site of the experiments from that in the part below, where the influence of the tail weir was felt. Further, the difference of slope in the parts of the reach above and below the site of the experiments differed widely in different conditions of the water supply. The local surface slope, that is the slope of the water surface in the neighbourhood of the gauging site, varied irregularly with the variation of the slopes above and below, being apparently, as might be expected, most affected by the obstruction at the tail of the reach. Now as the velocity at a given site does not exclusively depend on the surface slope at the site, but to a certain extent on the slope above and below, the conditions of the site were initially to some extent unfavourable, and that in a degree which, although it may be small, is difficult to appreciate. The local surface slope itself can only be measured on a considerable length of stream (1000 to 4000 feet). But in that length the surface slope appeared to vary, the slope in 2000 feet being as much as 25 per cent. different from that in 4000 feet, and the slope at one bank being 50 per cent. different from the slope at the other. It is obvious, therefore, that the local surface slope is a quantity which, under the conditions of these experiments, was not ascertainable with any great accuracy. But the whole comparison of the experimental results with formulæ of discharge involves the accurate knowledge of this quantity. All inferences from these experiments as to the reliability of formulæ must be weakened in proportion as the slope measurement is doubtful.

It is not in Major Cunningham's experiments alone that this difficulty in determining the surface-slope has been found. It is to the uncertainty of this quantity mainly, to this *fons et origo malorum*, that the discordances of large-scale experiments are due. The roughest small-scale experiments, those, for instance, discussed by Eytelwein and Prony, have furnished coefficients more useful in practice and more generally applied than any large-scale experiments hitherto carried out. The advantage of regular canals over natural rivers for hydraulic experiments almost disappears when the canal bed is scoured out to an irregularity similar to that of a natural stream, and the canals are at a disadvantage when artificial control at the tail of the reach modifies the conditions of flow to an extent sensibly felt at the site of the experiments. It is, of course, in the lower states of the water in the canal in the Roorkee aqueduct reach, that the effect of the tail control is most sensible, but then experiments made in these conditions are an essential part of the data necessary for generalisation.

Major Cunningham spent a good deal of time in verifying a supposed theory that the surface of a stream should be convex. The theory is probably a capital instance of the frequent mistake of importing the principles of theoretical hydrodynamics into practical hydraulics. In a stream flowing from a reservoir, in such a way that the tangential forces on the surface of the elementary streams are absent or negligible, the energy per pound of fluid is uniformly distributed. It follows that in parts where the velocity is greater, the pressure is less. A stream may be regarded as a bundle of horizontal filaments coming from a common reservoir. If in such a stream the central filaments have a greater velocity than those nearer the sides, their pressure will be less. Consequently, for equilibrium there must be a greater depth of stream towards the centre, and the transverse water-line will be convex upwards. Such is the theory which Major Cunningham has taken a great deal of trouble to test, and to which he attaches weight in spite of his observations. From preliminary calculations he shows that the known differences of velocity would give a difference of level, between the centre and sides of the Ganges Canal, of 3 inches. After the most careful measurements, it was found that the difference of level varied from +0·018 foot to −0·095 foot, the average difference being almost exactly zero. Obviously the theory is outrageously wide of the truth, and the reason is not far to seek. The differences of velocity to which the supposed differences of pressure are due, are created by exactly those tangential actions of the filaments which the theory neglects. There is no reason for assuming equal distribution of energy along a filament, part of the energy of which is being destroyed by lateral frictional actions between the filaments. As to the observations in Chapter V., with a guage giving still water-level, it is not clear that the small difference of level observed was not due to the position of the mouth of the tube which communicated with the canal.

The discussion of the vertical velocity parabolas in Chapter XI. is extremely interesting, and the method adopted for finding the most probable curve by the method of least squares, is laborious and conscientious. The method of weighting the observations seems, it is true, rather artificial, especially as the observations at great depth best define the form of the parabola. The general conclusion arrived at is, that while all the observations can be fairly well expressed by parabolic curves, no formula can be found expressing the dependence of the variation of velocity on the slope, and dimensions of the channel. It would be interesting to see if a parabola with axis on the water-line would not agree better with the results, the observations above the line of maximum velocity being of course discarded. So far as there is any theory of the mutual action of the filaments, it leads to the result that the parabolic axis should be at the surface; and that is not inconsistent with one possible explanation of the reduction of velocity near the surface.

In ordinary streams, the velocity is greater towards the surface and centre, and less towards the bottom and sides. But the greatest velocity is not found at the surface, but at a variable depth below it, amounting very often to one-fourth of the whole depth. The Mississippi observers attributed this to the friction of the water against the air. In accordance with this they found the depression of the line of maximum velocity to depend quite directly on the direction of the wind, and they logically introduced into their formulæ of flow, the free surface, as forming part of the frictional wetted border. Major Cunningham retains the Mississippi observers' explanation, while his experiments disagree with theirs on all the points which directly support the explanation. He finds, for instance, that the depression of the line of maximum velocity is entirely independent of the direction and force of the wind. Now excepting one suggestion to be referred to presently, no kind of retarding action between the air and water has been stated which is not of the nature of a frictional resistance. The Mississippi observers and some others who adopt the explanation of the depression of the line of maximum velocity we are now criticising, state explicitly that they consider the resistance between the air and water to be of the same nature as the resistance between the water and its solid bed. If so, since the line of maximum velocity is ordinarily depressed to one-fourth the depth at the centre, and generally still more towards the sides, the friction between the water and air must be something like one fourth as great as the friction between the water and solid bed. But is it conceivable that the friction between the level water surface and mobile air should have anything like one-fourth the value of the resistance of the water impinging on all the immovable roughnesses of the stream bed? Further, any resistance of this kind must depend on the relative velocity of the water and air. But the air is most commonly in motion, and on the average must as often and as long blow down stream as up stream. Blowing down stream, it should accelerate the stream to the same extent as blowing up stream it retards it. But it is known from Boileau's experiments and others that the depression is still persistent with a wind blowing down-stream at a velocity greater than that of the water. To this Major Cunningham's only answer is that "the time required for the penetration of change of velocity of the surface current caused by wind to any considerable depth appears to be very great. It has been estimated that it would take one week for half change of surface velocity to penetrate three feet." The evidence for this is not given, but if it is so, is it not because the friction between air and water is extremely small, and it is only in those cases where the persistence of the wind action for a long time allows an accumulation of effect, that that effect is sensible.

A wind blowing on the surface of a lake is long in producing a current merely because the friction is small, but it does produce a current in time, because the action is cumulative. On a river it produces no sensible effect at all, as Major Cunningham's experiments show. But if the friction between air and water is as great as he supposes, it ought to produce a sensible effect, and since winds blow as often and as long down-stream as up-stream, the water-surface should as often be accelerated as retarded, and the vertical velocity parabola should as often have its axis above the water-surface as below it. Boileau does indeed suggest that the absorption of air by the water and the evaporation of the water cause a loss of energy near the surface, but here again the cause seems as inadequate as air-friction. The experiment of Francis, quoted on p. 107, is admitted by Major Cunningham, to prove that "there is a continual transfer of water from the bed towards the surface, even in water in apparently tranquil motion," and his own float-observations (p. 269) show that "near the edge of a stream there is a persistent flow of the water at and near the surface from the edge towards the centre." Now the flow from the bottom and sides towards the top and centre brings water, stilled by impinging on roughnesses of the bed, to replace the quick moving surface-water. It is not true that the water so rising must acquire the velocity of the layers through which it passes, for it may rise in eddying masses, which are but little affected by the friction on their surface, or the motion of the water may be in horizontal spiral paths, which allow the bottom water to reach the surface without passing through the quicker moving central parts of the stream. At all events the transfer of the bottom water to the surface is a known phenomenon, and it is adequate as an explanation of the diminution of surface velocity.

In Chapter XVI. is given a somewhat elaborate theory of the motion of a rod-float, which leads to the result that the rod-velocity is slightly less than the true mean velocity of the water past the immersed portion of the rod. Quite apart from the question of the general unsteadiness of the motion of the water, it may be pointed out that the relative velocity $v-u$ of the streams impinging on the rod must for the most part fall below the limit for which the pressure due to impact or friction can be assumed to vary as the square of the velocity. Hence the calculation that the rod-length should be 0·94 of the depth of the water to give a true mean velocity, seems an extremely doubtful one.

In criticising thus two or three points of theory, it must be pointed out that these matters do in fact lie somewhat outside the main objects of the experiments, and an error on these points detracts nothing from the practical value of Major Cunningham's work. W. C. U.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

"Weather Forecasts"

WILL you permit me to call attention to the apparently complete failure of the Forecasts of Weather given in the daily papers with respect to the storm of Tuesday, October 24? The matter seems to me to be one of much practical moment. Here is an extract from the "Weather" article in the *Times*, which I presume agrees with that given in each of the daily papers:—

Forecasts of Weather for Tuesday, October 24 (*issued at* 8.30 *p.m. on the previous day*).

0. SCOTLAND, N.—South-westerly breezes, fresh or moderate; showery.
1. SCOTLAND, E.—South-westerly breezes, moderate; some showers, with bright intervals.
2. ENGLAND, N.E.—Same as No. 1.
3. ENGLAND, E.—Same as No. 5.
4. MIDLAND COUNTIES.—Same as No. 1.
5. ENGLAND, S. (London and Channel).—Westerly and south-westerly breezes, light to fresh; fine and cold at first, some local showers later.
6. SCOTLAND, W.—Same as No. 0.
7. ENGLAND, N.W. (and N. Wales)—Same as No. 0.
8. ENGLAND, S.W. (and S. Wales).—South-westerly winds, fresh to strong; showery.
9. IRELAND, N.—Wind returning to south-west, and freshening; weather showery.
10. IRELAND, S.—Same as No. 9.

Warnings.—None issued.

By order,
ROBERT H. SCOTT, Secretary.

Notice particularly the concluding words: "Warnings; none issued;" and then remember what took place. It is curious to compare in this respect the *Times* of October 24 with that of October 25. In the latter issue we read as follows:—

"Yesterday morning a violent gale of wind, accompanied by a heavy downpour of rain, visited London. The previous night was beautiful, but at three o'clock yesterday morning the sky became overcast, and from half-past four o'clock up to ten o'clock there was an incessant downpour of rain. At half-past nine o'clock the upper part of 19, Windmill Street, King Street, New Cut, was stripped off, and the occupiers of the upper floors had a narrow escape. At ten o'clock a sign-board was carried away from the frontage of a house in Jewry Street, Aldersgate Street. Although the street was crowded, no one was reported hurt. At Five Fields, Dulwich, the grass was strewn with broken arms from the trees, and a large elm at Norwood was blown down. A portion of a large shed situated near the Surrey Gardens Estate was unroofed. The trees in the various metropolitan parks have suffered severely from the gale. The River Thames at ten o'clock resembled a small sea, and much damage was done to the shipping below London Bridge."

And much more to the same purpose.

I feel desirous of knowing, both on general and scientific grounds, and also for obvious practical reasons, whether any explanation can be given of this absolute breakdown of weather science. It would seem to be possible that a storm can visit our coasts, and do immense destruction both by sea and land, and yet not give the faintest notice to our weather prophets of the impending danger; and it really almost makes one smile to perceive that on the day of the storm no warnings were issued, and that on the day after "the South Cone was hoisted this morning in Nos. 2, 3, 5, 7, and 8."

If no mistake has been made in the observations, and a mistake seems scarcely possible, we seem to be driven to the conclusion that a storm of the first magnitude can come upon us unawares; and if this be so, the conclusion is discouraging and very strange as regards science, and it is very serious as affecting the value of forecasts of the weather to fishermen and others.

I write this letter with the hope that some light may be thrown upon the subject to which it refers. H. CARLISLE

Rose Castle, Carlisle, October 26

The Comet

I BEG that you will allow me space for a few lines of comment upon the letters and drawings of the comet in your last issue, my own included. While thanking the engraver for the generally accurate reproduction of my sketch, it is clear that wood-engraving scarcely admits of a perfect rendering of stumped shading. A few words of correction will serve all the purpose of preserving for possible future use the evidence which I wished to put on record. The chief defect is in the *isolation* given to the "wisp," described by another correspondent as a "horn." It seemed rather to be an inclined elongation of the brightest part. The inclination too is exaggerated: its prolongation should have passed *within* the star on the northern[1] border, but clear of the head. The only other alteration I should desire would be the strengthening of the brightness all along the middle or axis of the tail, and the smoothing away of all other features such as now seem indicated in the body of it. Trivial as these changes may seem, the ultimate value of the drawing, if it should ever have any, must depend on its accuracy. The feebleness of the feature which attracted my attention may at the same time be inferred from its absence in the adjoining contemporaneous sketch accompanying Mr. Seabroke's letter, while its reality is proved by the descriptions in the two letters which follow. As regards the "rift" or "shadow," on which stress is laid by Mr. Williams at Cannes, one cannot help suspecting that this impression was the effect of contrast *only*—contrast between the complete absence of tail in that quarter, and the unrecognised presence of exceedingly feeble luminosity due to the extension and diffusion of cometic matter roundabout. It would require very strong evidence indeed to establish the real presence of *shadow* in the ordinary sense of that term.

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[1] The tail lies nearly along a parallel of declination.

One other point deserves notice. You have three contemporaneous accounts, from Rugby, Hawkhurst (Kent), and Cheltenham, all referring to the morning of October 23. Considering how rude and unsettled the weather has been for weeks past, so extensive a clearance was rather remarkable.

The brightness of this comet's tail may be inferred from an observation which I made during the current week, and which will perhaps excite as much surprise, if not incredulity, in others as it did in me. Sunday night was clear and bright, with a moon four days past the full. I was at an hotel in London, and on the stroke of three I stole into a vacant room in the third floor, the window of which looked south-east. Here I stood for a full hour looking for the comet, scarcely able to credit my senses, as the morning drew on without my seeing it. With the naked eye I could see stars of the 5th, and with a binocular, stars in Hydra of the 7th or even 8th magnitude; but no comet. At first I was uncertain, for this very reason, as to the identity of α Hydræ, although if I had not been seeing the comet flaring below it so frequently during the last three or four weeks, no such doubt would have occurred to me. At last, as all the small stars of Hydra gradually settled themselves in my recollection in their right places, and I knew *exactly* where the whole length of the comet *must* be, and the whole being then well above the opposite roof, I fancied at times that I could make out a faint illumination in the proper place; but not even then, with the binocular, could I find the head; nor could I, without previous knowledge, have been able to testify confidently to the presence of the tail.

I regret that I cannot condense this account without sacrificing some of the conditions which help to make so strange a disappearance credible. If anyone had told me on the 23rd that the object I was then drawing would be invisible to me a week later, in London, *by reason of moonlight only*—for the visibility of small stars proves the clearness of the atmosphere—how could I have credited it? I feel, therefore, that I cannot expect to be believed unless the whole circumstances are told, even though they betray my uncertainty about stellar configurations when deprived of the aid of a map. J. HERSCHEL

ON Wednesday, the 25th instant, at 6.10 a.m., Mr. Hodges and I again obtained two measures of position of the nucleus with the equatorial, after correcting for instrumental errors and refraction, the mean of the readings comes out R.A. 10h. 6m. 48s.; Dec. 17° 2′ 55″. But owing to flexure of the instrument and to the fact that the circles read only to 20″ and 2s. respectively, these figures are open to correction. Daylight, with a little haze, had so far advanced when the measures were completed, that only the nucleus was distinguishable in the telescope; but with the filar micrometer I measured its length; the mean of two readings came out to 41″·5, but owing to the gradual shading off of the nucleus, one's readings might vary 5″ according to its assumed limits. The width I made about 10″. I was rather surprised at these results, as I had estimated its length two days before at about 10″ only; but I had then used an eye-piece to which I am not accustomed, and my estimate was probably an error. The position angle of the major axis of the nucleus was 108° 7′.

Though the comet was fainter by reason of the bright moon, still we could trace the tail as far as on Monday, the 23rd.

We viewed the comet at 5 a.m., but owing to buildings in the line of sight, we got no reliable readings until 6 a.m.

In my sketch of the nucleus in your last issue, the engraver has made it round, with a fainter elongation. It appeared of nearly the same brightness throughout. GEO. M. SEABROKE

Temple Observatory, Rugby, October 30

I SEND herewith two sketches of the comet made by me on the mornings of October 23 and 31, and a few brief particulars which may be of some value.

October 23, 1882, at 4.30 a.m., the first sketch was made. At 4 o'clock the atmosphere was exceptionally clear, and the sky continued cloudless until 5 o'clock, when a few light clouds appeared. The comet was not brilliant, although clearly seen. Nucleus with coma presented an indistinctly outlined disc a few degrees above the horizon, and obliquely upwards was a tail which stretched more than 15° across the sky. I compared the extent of tail at the time with the distance between α and β Orionis, and the tail had decidedly the best of it. Whilst glancing from the comet to Orion, I saw in the intervening sky-space, in little over three minutes, no less than *five* meteors, one of which left a long luminous trail visible several seconds. The extremity of the tail was broad. Its *under* boundary was a well-defined line about 40° from the horizontal, and was slightly convex downwards. The *upper* boundary was about 45° from the horizontal, was nearly straight, but very ill-defined, the light fading away into darkness very gradually upwards. The fanning out of the tail was very rapid towards the far end. The termination was somewhat fish-tail shaped, since there was centrally a deepish concavity between the extreme limits, which projected horn-like. The light of the tail was broken into two unequal areas by an obscure streak. The inclosed lower area was the smaller and decidedly brighter, and on its lower side contained a still brighter area, that, starting from the upper part of the coma, gradually passed into the lower boundary.

October 31, 1882, at 5.30 a.m., the second sketch was made. The atmosphere was again very clear, but the moon's light dimmed the comet greatly, and exactly at 6 o'clock it and the coming dawn rendered it indistinguishable. The naked eye could distinguish none of the features observed on the 23rd, but the general outline had somewhat changed, and the comet had changed its position relatively to the stars. ARTHUR WATTS

Manor House, Shincliffe, Durham, October 31

MAY I beg the readers of NATURE, who possess good measures of the course of the great comet, kindly to publish them in NATURE? I would also be very much obliged for good measures of the distances of different envelopes of the head from the nucleus. The measures are desirable in two directions—*towards* the sun, and *perpendicularly* to this direction. Of the greatest scientific interest would be a complete series of measures during the whole period of visibility of the comet, and especially in the first and last days of this period. B.

"The Burman"

MR. E. B. TYLOR, in his review of "The Burman" in NATURE (vol. xxvi. p. 593), has fallen into an error which it may be well to correct. He says that the tattooing on the body of the "Greek nobleman," Georgios Konstantinos, "was evidently done by Burmese tattooers, and is a masterpiece of their unpleasant craft." This is a mistake into which even a man who had seen many specimens of Burmese tattooing, might fall. But it could never be made by a Burman. The general resemblance to the decorations on the Burman's thighs is close enough, but each separate figure, when done by the Burmese Sayah, is surrounded by a border of Burmese letters, in many cases as a mere ornament, but in not a few with a special cabalistic meaning. Still, however blurred with age, they can always be recognised as Burmese characters. I went down and examined the "tattooed nobleman," which he was good-natured enough to allow me to do very closely, and the result was to convince me that it was no native of Burma who so cruelly victimised the poor man. The frames of the figures might have been letters, but if so, they were of some language with which I am unacquainted. Moreover, many of the figures themselves were such as a Burman Sayah never uses; such as especially the birds and serpentine creatures, while the elephants were of a very inferior character. The Beeloos (ogres) and Kyah-Beeloos (tiger-ogres), moreover, which appear on every Burman's legs, were absent, and, most conclusive of all, there was not a single inn, not one cabalistic square. No Say-Sayah I ever knew would have had self-control enough to have omitted these signs of his wisdom in magic. Mr. Tylor says the story of Konstantinos is "mostly fictitious." That may be, but if he was not tattooed in Central Asia, it is difficult to say where it could have been done. I may also mention that the "nobleman" did not understand a single word of Burmese, and did not recognise a Burman, which could hardly have been the case if he had suffered his "punishment" in Burma. The pain, by the way, is not nearly so great as it is represented to be, and even when a man is tattooed all over the head, I cannot understand his dying or going mad, as Konstantinos's companions are said to have done. When I was tattooed, I had nearly twenty figures done at a sitting, and felt no particular inconvenience, though the actual operation is no doubt "unpleasant."

SHWAY YOE

THE opinion that the "tattooed man" was decorated in Burma has been generally received by anthropologists, and so far as I know, not hitherto contradicted. In addition to Mr. Franks' paper I may now refer to the *Transactions* of the Berlin Anthropological Society, in the *Zeitschrift für Ethnologie*, vol. iv. 1872 p. 201, for an account of an examination of him by Prof. Bastian, who, as an authority on Burmese matters, has been already mentioned in connection with "Shway Yoe's" book. Prof. Bastian says, "as to the Burmese character of the tattooing there can be no doubt. The letters rather point to the Shans, to whose district many treasure-diggers resorted," &c. It appears, also, that Konstantinos, when questioned as to the mode in which he was operated on, described the instrument as a split point carried in a heavy metal handle, which agrees with the Burmese method.

As the "tattooed man" is in part inscribed with actual letters, a copy of these would probably settle the question at once. It is a pity that for some reason photographs of him, which one would think were profitable articles from the exhibitor's point of view, are not (or lately were not) to be had.

E. B. TYLOR

River Thames—Abnormal High Tides

THE normal high water in the Pool, or the average of all the tides of the year, is a constant quantity, and is the same now as half a century back, the mean level being 12 inches below the Metropolitan datum of high water of spring tides called "Trinity standard." High water of spring tides averages 12 inches above, and high water of neaps 3 feet 6 inches below that datum. Whilst, however, the ordinary high water is a constant quantity, exceptional tides rise now very much higher than they did a quarter of a century back; on October 18, 1841, a tide occurred which rose 3 feet 6 inches above Trinity, and it was the highest recorded for half a century; eleven years afterwards, on November 12, 1852, 3 feet 7 inches were marked. The land flood of that year is popularly known as the Duke of Wellington's flood, from the demise of the great captain having occurred at that period; no such tide recurred for seventeen years nearly, until March 28, 1869, when 3 feet 7 inches was again reached. Five years afterwards the tide rose, on March 20, 1874, higher than ever before recorded, reaching an excess of 4 feet 4 inches.

These exceptional metropolitan tides arise from the rare concurrence of three causes, viz. an exceptionally heavy land flood meeting an equinoctial spring tide, and these accompanied by a great westerly gale heaping up the Channel sea, suddenly veering to north-west, and driving the tidal wave before it from the North Sea up the Thames estuary. Four reasons may be specified for these results. The first is the greatly increased rate of discharge of floods from the catchment basin. This, however, is questioned by many; but we find Stevenson giving the ordinary discharge as 102,000 cubic feet per minute; Beardmore 100,000 as the annual mean at Staines, and 400,000 as the maximum, whilst O'Connel, in the "Encyclopædia Metropolitana," states it at from 475,000 to 600,000 and Prof. Unwin, of Cooper's Hill College, obtained results during the winter of 1875, at the Albert Bridge, Windsor Home Park, equivalent to from 701,280 to 845,640, or one-third more than any previous estimate.

Secondly, the low-water *régime* of the river has been greatly developed by increased scour and removal of shoals by dredging, so that the head of the low-water prism ascending from seaward, with 20 feet minimum depth, which a quarter of a century back was below the Arsenal at Woolwich, is now above the Dock Yard, two miles higher. Thirdly, the removal of old London, Blackfriars, and Westminster bridges, by raising high water above-bridge 6 to 12 inches, and lowering low water 3 to 4 feet, brings up about 33 per cent. more tidal water above-bridge than half a century back.

Fourthly, the Thames Embankments have added a few inches to the range, by narrowing, straightening, and regulating the channel by which the tidal momentum has been increased. Now, assuming that the high water of a spring tide is raised from 4 to 6 inches, this, from London Bridge to Twickenham, would amount to 700,000 tons of water, but the additional quantity, due to the removal of the old bridges within the same limits, would amount to six times that quantity, or to 4,200,000 tons.

In an essay by me, recently published by the Institution of Civil Engineers, the proportion of land water as compared with tidal water was estimated at 1-18th of the latter, and that of the 14 inches excess of range over any previously recorded tide in November, 1875, only from 3 to 3½ inches might be due to land water. The Embankment Commissioners of 1861 took the hitherto standard maximum height for quays of 4 feet above Trinity, and this proved a safe elevation until March, 1874; but the tide on November 15, 1875, was 6 inches higher, and forcibly directed public attention to the question, and again on January 2, 1877, the tide rose as high as in March, 1874, and in January, 1881, reached a height of 4 feet 8 inches at the London Docks, and 5 feet here in Westminster, the maximum yet experienced.

The Admiralty Tide Tables of the last twenty years show that 2 feet and 2 feet 1 inch are the maxima to be expected during the equinoxes, but the computors direct attention to the fact that gales of wind will add at times materially to the estimated heights; indeed north-north-west gales will add 1 yard vertically to the computed heights in the Port of London, as the surface of the water at high water will be at times 5 feet higher than at sea with a good spring tide, the tidal column rising upwards at a tolerably uniform rate of 1¼ inch per mile in the forty-eight miles from Sheerness to London.

From 1860 to 1863, 6 inches was the calculated maximum above Trinity standard and that observed 3 feet and 6 inches in December 1863.

From 1864 to 1866, 6 inches was again the estimated excess, and 3 feet and 6 inches again the actual result in November 1866.

For 1867–1868 they were relatively 4 inches and 3 feet, the last in February 1868.

After this due to the altered condition of the river brought about by the causes just referred to, we have the following results as regards maxima, viz.:—

	Estimated height above Trinity.	Observed height above Trinity.	
	′ ″	′ ″	
1869—March	—	3 7	
October	1 8	—	
1870—February	—	3 0	
March	2 0	—	
1871—April	1 8	—	
1872—April	—	2 10	
September	1 7	—	
1873—February	—	3 3	
October	2 0	—	
1874—March	2 1	4 4	Westminster.
1875—April	1 10	—	
November	—	4 9	,,
1876—September	1 5	—	
June–Dec.	—	1 11	
1877—January	—	4 4	,,
March–Sept.	1 11	—	
1878—March	2 1	—	
November	—	3 1	,,
1879—March	1 10	—	
April	—	3 6	
1880—March	1 6	—	
November	—	2 9	,,
1881—January	—	5 0	,,
September	1 11	—	
1882—February	—	4 6	,,
Aug.–Sept.	2 1	—	

During the recent springs we have the following results (at Westminster):—

1882.		Estimated excess.	Actual excess.	Excess.	Wind.
		′ ″	′ ″	′ ″	
Tuesday,	Sept. 26, p.m.	0 5	0 12	0 7	E.S.E.
Wednesday,	,, 27, ,,	1 4	1 9	0 5	W.
Thursday,	,, 28, ,,	1 11	2 0	0 1	W.N.W.
Friday,	,, 29, ,,	2 0	2 6	0 6	W.N.W.

The early morning tide marked about 2 inches higher. During the past springs we have

		″	″	″	
Tuesday,	Oct. 10, p.m.	11 below ...	6 below ...	5 ...	E.
Wednesday	,, 11, ,,	5 ,, ..	6 above ...	11 ...	S.S.E.
Thursday	,, 12, ,,	1 ,, ...	[illegible] ,, ...	13 ..	W.N.W.
Friday	,, 13, ,,	3 above ...	9 ,, ...	6 ...	N.N.W.

The comparatively quiet autumnal weather sufficiently accounts for the slight variations.

The tide ebbed as low as 23 feet 6 inches below Trinity in October last year at the London Docks Shadwell entrance, yielding a total tidal vertical oscillation of fully 28 feet in the Port of London.

J. B. REDMAN

6, Queen Anne's Gate, Westminster, S.W., October 19

P.S.—The springs succeeding those described in my letter show a greater difference, influenced doubtless by the great gale of Tuesday, October 24, when the barometer fell as low as in the gales of October 28 and November 16, 1880, on these three occasions reading a tenth under 29 inches. The tide of October 28, 1880, was a low neap, but on November 19, 1880, at the top of the springs estimated at 6 inches under Trinity high water it was 2 feet 9 inches above, or 3 feet 3 inches excess three days after the gale.

The excessive amount of land water now meeting the tide adds to the increase, together with the northerly gales.

		Estimated.	Observed.	Excess.	
		′ ″	′ ″	′ ″	
Tues.	Oct. 24, noon	0 9 below ...	0 6 below ...	0 3	S.S.W. gale.
Wed.	,, 25, p.m.	0 5 above ...	0 12 above ..	0 7	W.S.W.
Thurs.	,, 26, ,,	1 1 ,, ...	2 9 ,, ..	1 8	S.
Fri.	,, 27, ,,	1 6 ,, ...	4 3 ,, ...	2 9	E.N.E.[1]
Sat.	,, 28, ,,	1 7 ,, ...	5 0 ,, ...	3 5	N.N.E.[1]

In effect the last tide is identical with that of January 18, 1881.—J. B. R.

Note.—The estimate of excess due to wind over and above the forecasts is somewhat overstated in this letter, as the Admiralty heights are for London Bridge and those observed are for Westminster, where the reading will be quite 2 inches higher.

Umdhlebi Tree of Zululand

THE following note has been communicated to us by the Rev. Dr. Parker, a well-known missionary in Madagascar. The story reminds one of the old myth about the Upas in Java. No light can be thrown upon it at Kew, but perhaps in the pages of NATURE it might meet the eye of some person who could give some more information about it. W. T. THISELTON DYER

There are two species, in both the leaf is lanceolate, dark green, glossy, hard, and brittle, and from both a thick milky juice exudes, while the fruit is like a long black pod, red at the end. One species is a tree with large leaves, and peculiar looking stem, the bark hanging down in large flakes, showing a fresh growth of bark underneath: in the words of my informant, "What a villainous-looking tree! nasty, rough, ugly!" The other species is a shrub, with smaller leaves, and the bark not peeling off the stem. Both species are said to possess the power of poisoning any living creature which approaches it; the symptoms of poisoning by it being severe headache, blood-shot eyes, and delirium, ending in death. The person affected dies either in delirium, or instantaneously without any delirium. A superstition is connected with this plant. Only a few persons in Zululand are supposed to be able to collect the fruits of the Umdhlebi, and these dare not approach the tree except from the windward side. They also sacrifice a goat or a sheep to the demon of the tree, tying the animal to, or near the tree. The fruit is collected for the purpose of being used as the antidote to the poisonous effects of the tree from which they fall—for only the fallen fruit may be collected. As regards habitat, these trees grow on all kinds of soil, not specially on that which exudes carbonic acid gas, but the tree-like species prefers barren and rocky ground. In consequence of this superstition, the country around one of these trees is always uninhabited, although often fertile.

G. W. PARKER

The Origin of our Vernal Flora

IT is usual to assign an Arctic origin to our mountain flora, and floral comparisons and statistics fully bear out this brilliant generalisation. It is formulated that height above the sea-level is climatally equivalent to northern latitude. This is an assumption that flowering plants are largely conditioned by heat. Thus latitude and oreographical habitats are more or less equal.

Might I introduce another element into this question? Seeing that temperature is so largely influential in explaining the distribution of flowering plants, it occurs to me that not only may height above the sea-level answer to northern distribution, but *seasonal occurrence* as well.

All botanists must have been struck by the fact that the earliest plants to bloom among our vernal flora are genera peculiarly Arctic and Alpine. In some instances (as with *Chrysosplenium oppositifolium* and *C. alternifolium*) the species are identical. These latter plants blossom with us in March or April; within the Arctic circle not until June and July, and even so late as August. Thus, with them, seasonal blossoming is equivalent to northern latitude, as regards the thermal conditions under which they flower. The generic names of all our early flowering plants are those pre-eminently Alpine and Arctic in their distribution—*Potentilla*, *Stellaria*, *Saxifraga*, *Chrysosplenium*, *Draba*, *Ranunculus*, *Cardamine*, *Alsine*, &c. I contend, therefore, that our vernal flora is explained by the fact that their seasonal occurrence, as regards temperature, is equivalent both to height above the sea-level and northern latitude. In every instance it will be found that the blossoming of the species of the above genera necessarily takes place in Great Britain two or three months earlier than within the polar circle. May we not therefore contend that we owe our English vernal flora to the same causes as distributed our English Alpine plants; and that they are as much protected by being able to flower earlier in the year, as if they had been located on the tops of high hills and mountains?

The power to endure cold and wet displayed by many members of our vernal flora is very remarkable. Thus *Ranunculus bulbosus* and *R. acris*, *Stellaria media*, &c., are frequently found in flower all through the winter, unless the season be extra cold. Many other early bloomers among our common flowers are also remarkable for their durability, whilst the late flowering plants are equally noticeable for the short space during which they bloom. This indicates a hardihood on the part of our vernal flora which cannot be explained except by reference to the climatal experience of the species. Some of them, as the groundsel and chickweed, may have exchanged an entomophilous for an anemophilous habit, or have become self-fertilised by the change.

Again, it must have been observed that many of our early flowering plants display a tendency towards a seasonal division of labour. All of them either flower before they leaf, or show a tendency to do so, as with the Coltsfoot (*Tussilago farfara*), the Crocus (*C. vernus*), the Snow-drop (*Galanthus nivalis*), &c. Even the violets (*Viola odorata* and *V. canina*), the Daffodil, Primrose, Cowslip, &c., although they in part leaf when they flower, develop leaves much more abundantly after flowering than before, thus showing an inclination towards dividing the period of active life into two distinct stages—the reproductive and the vegetative. Everyone knows how completely this has been effected by the Meadow Saffron (*Colchicum autumnale*). My impression is that this early flowering tendency is a survival of the habit these plants had to blossom under more rigorous climatal conditions. In short, that our vernal flora must have the same origin assigned to it as an Alpine; that it has survived through being able to bloom at an early period of the year at low levels, instead of flowering at a later season higher up, above the sea-level; protection and advantage being secured in both instances.

J. E. TAYLOR

Ipswich

On Coral-eating Habits of Holothurians

BEING struck with a remark of Mr. Darwin in his work on "Coral Reefs," where it is stated on the authority of Dr. J. Allan, of Forres, that the Holothuriæ subsist on living coral, and that by these and other creatures which swarm on coral reefs, an immense amount of coral must be yearly consumed and ground down into mud (p. 14), I determined to commence a series of observations on this subject, in order to ascertain the rate at which these animals void the coral sand from their intestinal canal, and "*ergo*" the amount of coral an individual would yearly transform into sand.

I have by no means satisfied myself that the Holothuriæ do subsist on living coral. This may be due, however, to my field of observation being confined to the fringing reefs around Santa Anna, and the neighbouring coast of the large island of St. Christoval—where living coral occurs only in scanty patches, the greater portion of the coral "flats" being formed of coral detritu cemented into a more compact rock. I carefully watched the habits of the two species most numerous on the "flats," and in no case did I observe a single individual browsing on the patches of living coral. In truth it was on the dead coral rock forming the "flats" of these reefs that these two species of Holothuriæ subsisted; and it appeared to me that they selected those feeding-grounds where the attachment of molluscs, zoophytes, and stony algæ had to some degree loosened the surface of the rock.

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[1] Gales.

The particular species, on which my observations were made to determine the amount of coral sand daily discharged, possessed a bluish-black body, from 12 to 15 inches in length when undisturbed, and with a circle of 20 pelate tentacles around the mouth. Without going into all the details of my methods of investigation, it will be sufficient to state that from three independent observations on this species of Holothuria I have placed the amount of coral sand daily voided by each individual at not less than two-fifths of a pound (avoirdupois). At this rate some fifteen or sixteen of these animals would discharge a ton of sand from their intestinal canals in the course of a year, which represents about 18 cubic feet of the coral rock forming the "flat" on which these creatures live. In order to illustrate this point more clearly, I will assume that every rood of the surface of the "flat" supports some fifteen or sixteen Holothuriæ, a number which errs rather on the side of deficiency than of excess. In the course of a year 18 cubic feet of coral rock will be removed in the form of sand from the surface of each rood, which is equal to the removal of 1·605th of a foot per annum, or 1 foot in about 600 years.

Although this estimate can be only regarded as of a tentative character and as applicable to but one species of the Holothuriæ, it nevertheless throws some light on what I may term the "organic denudation" of coral reefs, and it is not unreasonable to suppose that where a fringing reef is undergoing a very gradual up-heaval, the combined operation of the fish, the mollusc, the annelid, and the echinoderm, may prevent it from ever attaining an elevation above the level of the sea at high water. H. B. GUPPY

H.M.S. *Lark*, St. Christoval, Solomon Islands, June 30

Railway Geology—a Hint

IT must often have occurred to others as well as to myself when making a long journey by rail, and being whirled along all too fast through section after section of the greatest interest to the eye that can see in them something more than mere railway "cuttings," how valuable would be some handbook giving the geological features of the country traversed by the principal railway lines, and illustrated by clearly drawn maps and sections.

To give an instance—I have occasion pretty often to travel by the South Western line from Waterloo Station to Exeter, a route along which my untrained eye can take note of a succession of instructive pictures, in the course of a five hours' journey—the recent gravels, &c., covered by pine wood in the neighbourhood of Woking, broken abruptly at Basingstoke station by a section of the chalk, to be succeeded from here onwards to Salisbury by undulating downs of the same formation, bare of trees, and but sparsely inhabited; next, at the Yeovil junction, a sandstone quarry, riddled by martin's nests, presumably of oolitic age; then, between Axminster and Honiton the greyish blue of a cutting through the lias; to be finally succeeded, as I approach the term of my journey, by the rich red earths and loams of the new red sandstone.

Any other line, for instance, the Great Western, which runs parallel to that just instanced, would give equally varied pictures; and a copiously illustrated handbook, with notes explanatory, but as brief as possible—not only of the ground immediately bordering the line of rail, but of the general features of the neighbouring country within the range of the eye of the traveller, should surely, I venture to think, have a large circulation.

Will no geologist—a member of the Government Survey, for instance—undertake the task? J. C. G.

New University Club, October 27

[We noticed a Guide of this kind for American railways in vol. xix. p. 287, and then suggested the utility of a similar handbook for England.—ED.]

Complementary Colours

I HAVE often noticed the complementary purple on the foam of the bluish-green waters of Alpine rivers. The waters of the Lake of Geneva, and of the Rhone at Geneva, as is well known, are not bluish-green, but greenish-blue; but there also I have noticed what to my eye is exactly the same tint of purple on the foam. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, October 28

Palæolithic River Gravels

THE recent articles and reports in your columns on the subject of Palæolithic river gravels bring three points strongly forward, viz.:—

1. The great number of "flint implements" and "flint flakes" found in the river gravels.
2. The presence in the same deposits of bones of recent and extinct Mammalia.
3. The entire absence of the bones of man.

Such being the uniform results of persevering researches extending now for more than twenty-four years, it is surely time to request anthropologists to give (1) some explanation of the remarkable absence of human remains in deposits containing so many objects considered to be of human manufacture, and (2) some proof that it is absolutely impossible for these so-called "flint implements" and "flint flakes" to have been formed by natural causes. C. EVANS

Hampstead, October 18

LAVOISIER, PRIESTLEY, AND THE DISCOVERY OF OXYGEN.

IT is a matter of very little importance whether Lavoisier actually obtained oxygen gas a few weeks or days before Priestley. The bare bald discovery of the gas is a very minor matter when placed in juxtaposition with the astounding revolution produced in chemistry by Lavoisier; with the admirable series of experiments, the acute reasoning, the elegant logical penetration, which enabled him to overthrow the theory of Phlogiston when literally all Europe supported it. The discovery of oxygen dims and pales before the development of the theory of combustion, the theories of acidification, of calcination, of respiration, and the introduction of exact quantitative processes and instruments of precision into chemistry.

But it matters much whether the fair fame of one of the noblest and wisest men in the long roll of illustrious natural philosophers is to remain with a grievous slur cast upon it. It matters much whether his reputation is to be blasted by the reproach that he claimed the discovery of oxygen, knowing well that Priestley had preceded him.

It is with a view of removing this slur upon the memory of the founder of modern chemistry, and certainly not with any thought of adding one iota to his long list of greater triumphs, that we have examined into the true bearings of the question.

First as to the accusations. Dr. Thomas Thomson, in his "History of Chemistry," 2nd edit., 1830, vol. ii. p. 19, writes: "Lavoisier, likewise, laid claim to the discovery of oxygen gas, but his claim is entitled to no attention whatever, as Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774, which is a considerable time before the date assigned by Lavoisier for his pretended discovery." Again, p. 106: "Yet in the whole of this paper the name of Dr. Priestley never occurs, nor is the least hint given that he had already obtained oxygen gas by heating red oxide of mercury. So far from it, that it is obviously the intention of the author of the paper to induce his readers to infer that he himself was the discoverer of oxygen gas. For after describing the process by which oxygen gas was obtained by him, he says nothing further remained but to determine its nature, and 'I discovered with *much surprise* that it was not capable of combination with water by agitation,' &c. Now why the expression of surprise in describing phenomena which had been already shown? And why the omission of all mention of Dr. Priestley's name? I confess that this seems to me capable of no other explanation than a wish to claim for himself the discovery of oxygen gas, though he knew well that that discovery had been previously made by another."

Had Dr. Thomson been better acquainted with the character of Lavoisier; had he known what manner of man he was in all his dealings with his contemporaries and with the work of those who had gone before, he would never have made such an assertion as the above.

Prof. Huxley in his Birmingham address on Priestley (August 1, 1874) also accuses Lavoisier of unfairness: "though Lavoisier," he writes, "undoubtedly treated Priestley very ill, and pretended to have discovered dephlogisticated air, or oxygen, as he called it, independently, we can almost forgive him, when we reflect how different were the ideas which the great French chemist attached to the body which Priestley discovered."

Starting, as we confess, with the complete belief that Lavoisier did not discover oxygen, we are compelled to assert that a careful perusal of the various memoirs bearing upon the subject and the consistent attitude of Lavoisier throughout, has led us to the firm conviction that he has as much right to be regarded as the discoverer as either Priestley or Scheele.

Let us examine Dr. Thomson's statements. The year 1774 he asserts "is a considerable time before the date assigned by Lavoisier to his pretended discovery."

Lavoisier ("Traité élémentaire de Chimie," 1789, part 1, Chap. III.) says in speaking of oxygen: "Cette air que nous avons découvert presque en même temps, M. Priestley, M. Scheele, et moi, a été nommé, par le premier air déphlogistiqué; par le second, air empyréal. Je lui avais d'abord donné le nom d'*air éminemment respirable;* depuis on y a substitué celui d'*air vital.*" Evidently "presque en même temps" is a very loose statement. Scheele's treatise, "Chemische Abhandlungen von der Luft und Feuer," was published in Upsala in 1777, and he certainly did not discover oxygen before 1775. Lavoisier is therefore speaking in quite general terms when he says that oxygen was discovered almost at the same time by Priestley, Scheele, and himself. He at least puts himself on a level with Scheele as to date, and it is universally admitted that Scheele procured the gas after Priestley. And this general expression is the only *claim* to the discovery we can anywhere find in the writings of Lavoisier.

Now what are the facts in favour of Lavoisier? On November 1, 1772, he deposited with the secretary of the Academy a note, which was opened on May 1 following, in which he stated that he had discovered that sulphur and phosphorus, instead of losing weight when burnt, actually gained it, without taking into account the humidity of the atmosphere. He traced this to the fixation of air during the combustion, and surmised that the gain of weight by metals during calcination was due to the same cause. He reduced litharge in close vessels "avec l'appareil de Hales," and observed the disengagement of a great quantity of air. "This note leaves no doubt," says Dr. Thomson, "that Lavoisier had conceived his theory, and confirmed it by experiment, at least as early as November, 1772. . . . "Il est aisé de voir," writes Lavoisier, just before his death, "que j'avais conçu, dès 1772, tout l'ensemble du système que j'ai publié depuis sur le combustion."

Early in 1774 he published experiments in his "Opuscules physiques et chimiques," to prove that lead and tin, when heated in closed vessels, gain weight, and cause a diminution in the volume of air. "J'ai cru pouvoir conclure," he writes, "de ces expériences, qu'une portion de l'air lui-même, ou d'une matière quelconque, contenue dans l'air, et qui y existe dans un état d'élasticité, se combinait avec les metaux pendant leur calcination, et que c'etait à cette cause qu'était due l'augmentation de poids des chaux métalliques." Later in the year he read before the Academy ("à la rentrée publique de la Saint Martin, 1774"); a memoir "On the calcination of tin in closed vessels," in which he proved that when tin was calcined in hermetically sealed vessels, it absorbed a portion of the air equal in weight to that which entered the retort when it was unsealed, so as to admit air. He states as his conclusion that only a part of the air can combine with metals or be used for purposes of respiration, and that hence the air is not a simple body as generally believed, but composed of different substances; and he adds that his experiments on the calcination of mercury, and the revivification of the calx, singularly confirm him in this opinion.

At the Easter Meeting of the Academy in 1775, Lavoisier read a memoir, "Sur la nature du principe qui se combine avec les métaux pendant leur calcination et qui en augmente en poids." In a footnote we are informed that the first experiments described in the memoir were made more than a year previously, while those relating to the mercury *precipitatus per se*, "ont d'abord été tentées au verre ardent dans le mois de Novembre, 1774." Having heated calx of mercury with carbon, he found that fixed air soluble in water was given off, while when he heated it alone he observed *avec beaucoup de surprise* that an air was produced insoluble in water, readily supporting combustion, serving for the calcination of metals; incapable of precipitating lime water, and incapable of being absorbed by alkalies.

Priestley obtained a gas from mercury, *calcinatus per se*, on August 1, 1774, and finding it insoluble in water, and capable of readily supporting combustion, concluded that the mercury during calcination had absorbed *nitrous particles* from the air. He did not discover the real nature of the gas till March, 1775. In October, 1774, Priestley visited Paris, and mentioned to Lavoisier, Leroy, and others the prodction of gas from the mercury *calcinatus per se.* Probably the properties were not demonstrated. Lavoisier says he observed "with much surprise" that the gas was not absorbed by water, &c., was not in fact fixed air. He had expected to find the air given off by calx of mercury when heated alone, the same as that evolved when he tested it with charcoal, and was surprised to find it a different air. He enumerates the principal properties of the new gas as we know it. He burns it in a candle, charcoal, and phosphorus. He calls it air *eminemment respirable*, and *air pur;* and says it alone is concerned in respiration, combustion, and the calcination of metals.

Lavoisier constantly quotes Priestley and Scheele in connection with oxygen; again and again he speaks of that air which Mr. Priestley calls *dephlogisticated*, M. Scheele *empyreal*, and I *highly-respirable*," but we can find no distinct claim to its discovery save the sentence quoted above, in which he states that it was discovered *almost at this same time* by Priestley, Scheele, and himself.

In his next memoir, "On the Existence of Air in Nitrous Acid" (read April 20, 1776), he says: "Je commencerai, avant d'entrer en matière, par prévenir le public qu'une partie des expériences contenues dans ce mémoire ne m'appartiennent point en propre; peut-être même, rigoureusement parlant, n'en est-il aucune dont M. Priestley ne puisse réclamer la prèmière idée." And again: "Je terminerai ce mémoire comme je l'ai commencé, en rendant hommage à M. Priestley de la plus grande partie de ce qu'il peut contenir d'interessant." Moreover, in giving an account of ammonia, sulphurous acid, and several other gases, he writes: "Les expériences dont je vais rendre compte appartiennent presque toutes au docteur Priestley; je n'ai d'autre mérite que de les avoir répétées avec soin, et surtout de les avoir rangées dans un ordre propre à presenter des consequences." Thus it must be admitted that Lavoisier was always ready to acknowledge the merits of Priestley.

Even supposing that Priestley had demonstrated the production of oxygen to Lavoisier before he had himself obtained it, which, however, does not appear probable, Lavoisier investigated its chief properties before Priestley knew any more of it, than it was a gas containing nitrous particles. "Till this first of March, 1775," writes Priestley, "I had so little suspicion of the air from *mercurius calcinatus* being wholesome, that I had not even thought of applying to it the test of nitrous air." Again, in speaking of an experiment made on March 8, 1775, he says: "By this I was confirmed in my conclusion that the air extracted from *mercurius calcinatus*, &c., was *at least as good* as common air; but I did not certainly conclude that it was any *better*." At this time Lavoisier had proved the principal properties of the new gas, as we now know them. No wonder he expresses surprise. Did Paracelsus discover hydrogen? or did Boyle? or Mayow? or Cavendish? Lavoisier saw *with much surprise*, not that a gas was produced by heating calx of mercury, but that the gas was different from fixed air.

Let us finally examine Dr. Thomson's criticism of the "Opuscules Physiques et Chimiques":—

"Nothing in these essays," he writes, "indicates the smallest suspicion that air was a mixture of two distinct fluids, and that only one of them was concerned in combustion and calcination; although this had been already deduced by Scheele from his own experiments, and though Priestley had already discovered the existence and peculiar properties of oxygen gas. It is obvious, however, that Lavoisier was on the way to make these discoveries, and had neither Scheele nor Priestley been fortunate enough to hit upon oxygen gas, it is exceedingly likely that he would himself have been able to have made that discovery."

Now these essays were published "*au commencement de* 1774," at which time we have abundant evidence from other memoirs that Lavoisier *had* more than suspicion "that air was a mixture of two distinct fluids, and that only one of them was concerned in combination and calcination." Moreover, this had *not* "been already deduced by Scheele from his own experiments; neither had Priestley "already discovered the existence and peculiar properties of oxygen gas."

We do not the least press the following point. We trust we have made out our case without the necessity of resorting to it; but we venture to ask upon what authority Dr. Thomson asserts that "Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774." In our edition of Priestley's works (3 vols. 8vo. "Being the former six volumes abridged and methodised with many additions." Birmingham: Thomas Pearson, 1790), Priestley, after telling us that he visited Paris in October, 1774, says, "I frequently mentioned my surprise at the kind of air which I had got from this preparation to M. Lavoisier, Mr. Le Roy, and several other philosophers, who honoured me with their notice in that city" (p. 109). And again, "as I never make the least secret of anything I observe, I mentioned this experiment also, as well as those with the *mercurius calcinatus*, and the red precipitate to all my philosophical acquaintances at Paris and elsewhere; having no idea at that time, to what these remarkable facts would lead." It is of course a very different thing to mention an experiment to an acquaintance, and to actually perform it before him. But suppose, as Dr. Thomson asserts, that Priestley had prepared the gas from *mercurius calcinatus* in Lavoisier's house in October 1774, it is abundantly manifest by his own confession that he had no idea at that time of the nature of the gas; and more than five months afterwards that he had "so little suspicion of the air from *mercurius calcinatus* being wholesome, that I had not even thought of applying to it the test of nitrous gas"; and even so late as March 8, 1775, he did not conclude that the new gas was any better than common air!

Who is the discoverer? Is it the man who obtains a new body for the first time without recognising that it is different from anything else, or is it the man who demonstrates its true nature and properties? If the former Eck de Sulzbach discovered oxygen in 1489, and Boyle in 1672 not only procured hydrogen but proved its inflammability. If the latter, assuredly Lavoisier discovered oxygen.

But whatever the verdict may be, the memory of Lavoisier shall be saved from any imputation of unfairness. He was the most generous of men. His noble character stands out clearly and luminously in all his actions. He was incapable of any meanness.

We cannot for one moment compare the work of Priestley with that of Lavoisier. The elegant methods and admirable diction of the latter contrast strangely with the clumsy manipulation and prosy phlogistianism of the former. "From an ounce of red lead," writes Priestley, "heated in a gun-barrel, I got about an ounce measure of air, which altogether was worse than common air, an effect which I attribute in great measure to phlogiston discharged from the iron. The production of air in this case was very slow." Then he heated, without method or reason, as Hales had done before him, "flowers of zinc, chalk, quicklime, slacked lime, tobacco-pipe clay, flint, and muscovy talck, with other similar substances, which will be found to comprise almost all the kinds of earth that are essentially distinct from each other, according to their chemical properties," in the hope of getting some phlogisticated air from them. What a farrago! John Mayow, a century earlier, wrote more scientifically: "Si ad flammæ naturam serio attendamus, et nobiscum cogitemus, qualem demum mutationem particulæ igneæ subeunt, dum eædem accenduntur: nihil aliud certe concipere possumus, quam particularum ignearum accensionem in motu earum pernicissimo consistere. Quidni ergo arbitremur, particulas salinas ad ignem constandum præcipue idoneas esse? Quæ cum maxime solidæ, subtiles, agilesque sint, motui velocissimo, igneoque obeundo multo aptiores esse videntur, quam particulæ sulphureæ, crassiores mollissimæque."

Priestley's observations read like the writings of the seventeenth century, Lavoisier's like those of the nineteenth. Compare with the extract given above about the "phlogiston discharged from the iron" the following, "I have," writes Lavoisier, "a salt of unknown composition: I put a known weight in a retort, add vitriolic acid and distil. I obtain acid of nitre in the receiver, and find vitriolated tartar in the retort, and I conclude that the substance was nitre. I am obliged in this reasoning to suppose that the weight of the bodies employed was the same after the operation as before, and that the operation has only effected a change." "J'ai donc fait mentalement une équation dans laquelle les matières existantes avant l'opération formaient le premier membre, et celles obtenues après l'opération formaient le second, et c'est réellement par la résolution de cette équation que je suis parvenu au résultat. Ainsi, dans l'exemple cité, l'acide du sel que je me proposais d'examiner était une inconnue, et je pouvais appeler x. Sa base m'était egalement inconnue, et je pourvais l'appeler y; et puisque la quantité de matière a du être la même avant et après l'opération, j'ai pu dire $x + y +$ acide vitriolique $=$ acide nitreux $+$ tartre vitriolé $=$ acide nitreux $+$ acide vitriolique $+$ alcali fixe; d'où je conclus que $x =$ acide nitreux, $y =$ acide fixe, et que le sel en question est du nitre."

There is nothing in Priestley's scientific writings which exhibits so masterly a treatment as this. Priestley ignored Lavoisier's brilliant conclusions. He died defending the theory of Phlogiston. He denied the decomposition of water. He worked without method or order, and without the balance; and reasoned upon facts which lacked verification by quantitative means.

His conclusions were frequently hasty and ill founded. Lavoisier's work requires no praise in this place. Priestley's discoveries may be compared to the mingled chaos of ὁμοιομερεῖαι of Anaxagoras; Lavoisier was the Νοῦς, the designing intelligence which set them in order, and put each in its appointed place. Not without reason, said M. Wurtz, "La Chimie est une science française. Elle fut instituée par Lavoisier d'immortelle mémoire."

G. F. RODWELL

A NEW DREDGING IMPLEMENT

HAVING recently visited Oban, in company with a friend for the express purpose of obtaining living specimens of Pennatulida, and of testing the powers of an instrument devised for their capture, I send you a note of our experiences which may perhaps be of interest to your readers.

The ordinary dredge, though well adapted to obtaining most animals that dwell on the sea-bottom, will clearly not do for all, and for no animal form is it less suited than for the one we were most anxious to obtain—*Funiculina quadrangularis*. This giant Pennatulid consists of a tall fleshy rod-like axis, three to five feet or more in length, and about half an inch in diameter, which bears along its sides the individual polypes of the colony, and is traversed throughout its entire length by a flexible calcified stem. *Funiculina* lives erect, with the lowermost six or eight inches planted as a stalk in the mud of the sea-bottom, and the major portion of its length projecting up freely into the water.

For such a form the dredge is clearly very unsuitable. Indeed unless the dredge be of very great size it must be a pure accident if specimens ever get into it at all. The tangles give a better chance, and yet for such a purpose they are but a clumsy and haphazard contrivance; and even should they by chance entangle and draw out a *Funiculina* there is a danger, amounting almost to certainty that it will drop off again during the process of hauling in.

The instrument we employed was a modification of one originally devised by Dr. Malm of Goteborg, and used by him with considerable success in dredging for *Funiculina* in Gullmarn Fiord, Bohuslan. Dr. Malm's apparatus, of which he has kindly furnished us with a description and drawings, consisted of three poles, each nine feet long, connected together at their ends, so as to form a triangle; the poles were armed with large-sized fish-hooks, and the dredging-rope attached at one angle, the whole apparatus strongly resembling that used by the Philippine Islanders for dredging *Euplectella*, as described and figured by Moseley (Naturalist on the *Challenger*, p. 407).

Our instrument, as we first used it, consisted of two poles six feet long, connected together in the form of a letter A by a cross-bar four feet long. The rope was fastened to the apex of the A, and lead weights to the lower ends of the side poles. Attached along the cross-bar at intervals of six inches were cords four feet in length, each armed with five or six fish-hooks and having a small lead weight tied to its lower end. The theory of the machine was that the whole instrument would be dragged along at an angle of about 30° to the sea-bottom, steadied by the weights at the ends of the side poles; the cross-bar being a foot or so above the ground, and the cords armed with fish-hooks trailing behind, with their ends kept on the bottom by the small weights attached to them.

The machine was subsequently modified by lengthening the cross-bar to nine feet, and attaching the fish-hooks not singly, but in threes, like grappling irons. We also connected the cords together by horizontal strings, in order to obviate their tendency to become entangled with one another.

The instrument yielded excellent results: a large number of specimens of *Funiculina quadrangularis* were obtained, four or five, and in one case as many as seven being brought up at a single haul; the specimens were also in perfect condition, the injury inflicted by the hook being quite imperceptible. Several of the specimens were of large size; and one dredged in Ardmucknish Bay, and measuring no less than sixty-five inches in length, appears to be the largest specimen hitherto obtained alive from any locality, being a foot longer than the largest recorded by Kölliker in his monograph on the the Pennatulida. Even this, however, does not appear to be the limit of growth, for a dead stem obtained at Glaesvae, in the Bergen Fiord, and now in the Hamburg Museum, is more than seven feet in length.

Funiculina quadrangularis is generally considered a rare species. It is certainly a very local one; but our Oban experience would lead us to infer that where it does occur it is to be found in quantity, an inference borne out by Sir Wyville Thomson, who speaks of passing over a "forest of *Funiculina*" when dredging in Raasay Sound during the *Porcupine* expedition. It appears to have been hitherto obtained at Oban only in small numbers, a result we believe to be due entirely to the use of instruments ill-adapted to its capture.

Four or five specimens of *Pennatula phosphorea* were obtained with the same instrument, which further proved its utility by bringing up several fine specimens of Hydrozoa. The instrument in its present form is clearly capable of improvement; still the results of a first trial have been so good, that we may possibly be rendering a service to other naturalists by making them known through your columns.

A. MILNES MARSHALL

Owens College, October 27

WIRE GUNS

IT will no doubt surprise many of our readers to be told that after nearly a quarter of a century of experiment and investigation, and the expenditure of millions upon millions of money, the nation is so imperfectly armed that we are again entering upon a period of reconstruction of our heavy ordnance, the outcome of which it is not easy to foresee. From the old cast-iron 68 pounder, weighing from 4 to 5 tons, we have arrived at the 80 ton gun of Woolwich, but only to learn that such guns are already obsolete, and must give place to others of a new type developing greater power with less weight. Till very recently we have been constantly told by the highest authorities in this department of the Government that the English guns were the finest, the strongest, and the most powerful in the world, and it is no doubt somewhat startling to learn that all this has been a delusion.

It is not our intention to dwell upon the causes of this, nor to inquire whether it has been due to departmental conservatism or to the uncertainty incidental to the progress of an art carried on by a tentative method, and modified from time to time by new discoveries in physical science. Our purpose is rather to give some information about a system of gun making, which is at last obtaining the attention of gunmakers, we allude to what is termed the wire system of construction.

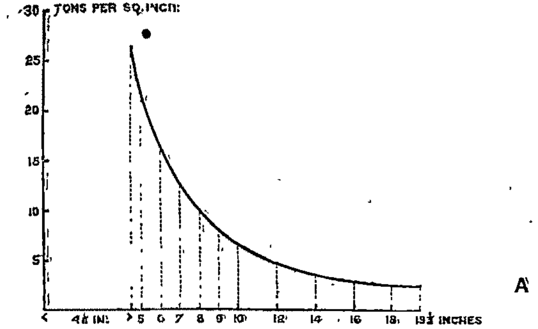
Twenty-seven years ago this system was brought before the then existing Ordnance Committee by the writer who has from that time to this persistently advocated its merits, proving, not only by the construction of guns but also by mathematical analysis, its great advantage over other systems; but it is only within the last two or three years that it has been regarded with tolerance by practical gun makers.

In France the system has been applied under the superintendence of Capt. Schultz, of the École Polytechnique, and in this country Sir Wm. Armstrong and Co. have made one or two guns, the latest and largest of which is now under trial at Woolwich. So far as these guns have been tried they have given very exceptionally good results, both in France and England, and they promise to excel all others in strength, facility of construction, and economy as regards cost. Let us then attempt to explain in a popular manner the principles and methods of this system of construction.

A gun is a machine the object of which is to send heavy bodies to a great distance at a very high velocity. The motive power acts on the body for a very short time, a fraction of a second only, it must therefore be of great intensity, and consequently the machine must have very great strength. Formerly all guns were made of cast-iron or bronze; after this wrought iron and steel came into use or a combination of the two. Krupp and Whitworth adopted steel, Armstrong and Woolwich a combination of wrought-iron and steel, Palliser again, a combination of cast and wrought-iron.

In making a vessel to resist great internal pressure, it was natural to conclude that by increasing the thickness of the vessel, its resisting strength could be proportionately increased, but as was first pointed out by the late Prof. Barlow, it was found that the limit in this direction was very soon reached, and that no vessel, whatever the thickness, could resist an internal pressure greater than the tensile strength of the material of which it was made.

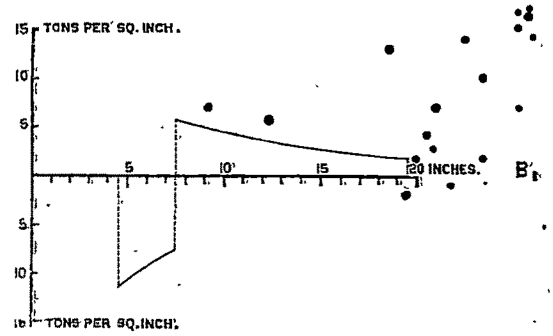
If the cylinder be composed of a material whose tensile strength is 10 tons per square inch, and if the internal pressure be 10 tons per square inch, and if the cylinder be conceived as to be divided into a great number of successive indefinitely thin layers, then, whatever be its thickness, the first of these layers will be strained to 10 tons, its maximum strength, the next layer will be strained less, and the strains will go on decreasing according to a fixed law as we proceed outwards. Now these outer layers cannot exert any more force, except it be transmitted from the innermost one, and consequently any further assistance can only be got from them by increasing the strain of the innermost layer, which, being already strained to its maximum strength, must necessarily give way.

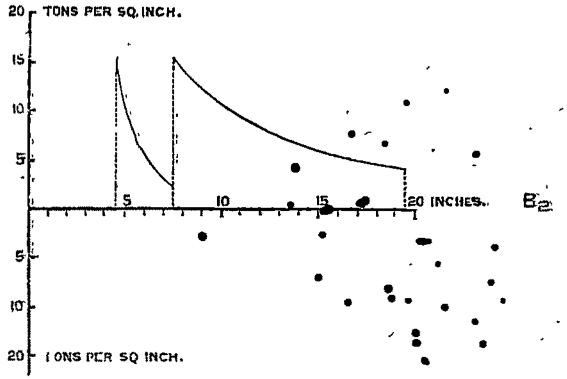


In order to meet this radical defect in all homogeneous cylinders the principle of initial tension was adopted. This was done by building up the cylinder of several concentric rings, or hoops, each of which was put on the one below it with an initial strain, thus compressing all those below. If now, by this method, the innermost hoop or tube be put into a state of compression of, say, 5 tons per square inch, it is evident that the first thing the internal pressure has to do, is to remove this compression to zero. This will absorb 5 tons per square inch of pressure. It has then to overcome the tensile strength of the material, or 10 tons per square inch, which requires an additional pressure of 10 tons per square inch. Thus the resisting force of the cylinder has been increased from 10 to 15 tons per square inch.

Now the greater the number of the hoops in a given thickness of cylinder, the greater is the additional strength imparted, *provided that each hoop is put on with the proper initial strain*, and if the hoops were infinite in number and therefore infinitely small in thickness, we could obtain the maximum strength for the thickness of cylinder, and each ring would, at the moment of rupture, be strained to its maximum tensile force. In such a cylinder the strength would increase in the exact ratio of the increase of thickness, and when it burst every layer would give way at the same time, but as there is no limit to the possible increase of thickness, there is also no limit to the possible increase of the internal pressure. Of course this theoretical construction is practically impossible, but we can approach to it very closely by making the hoops very numerous and very thin. The limit of the number of hoops is however very soon reached in the system of hoop construction.

Sir Wm. Armstrong's 100-ton gun is built up of a steel tube and three wrought-iron hoops on it. The Woolwich 81-ton gun has a steel tube and two wrought-iron hoops. Sir Wm. Armstrong's gun is therefore a better gun than the Woolwich, assuming in both cases that the initial tensions are correctly adjusted, but if in either case the number of hoops had been doubled, the total thickness remaining the same, both guns would have been greatly increased in strength. The practical difficulties of increasing the number of rings are, however, very great, and the expense would be enormous. The proper initial tension, or *shrinkage* as it is called, depending on extreme accuracy of workmanship, would be extremely difficult of attainment, and Sir Wm. Armstrong has probably gone nearly as far as is practically possible in this direction.

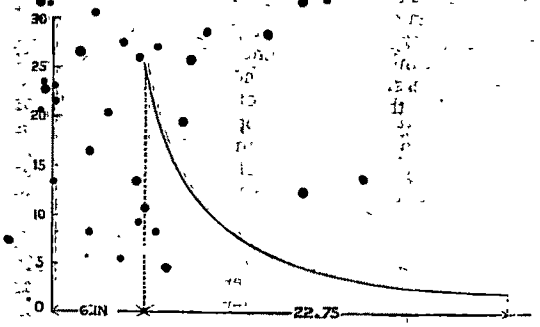




The regulation of the initial tension in guns of the hoop construction is so important that it is necessary to go somewhat more into detail, in order that our readers may thoroughly understand its importance, and be in a position to appreciate the advantages attendant on the use of wire.

We therefore introduce to their notice a series of diagrams showing the distribution of the strains throughout the thickness of a gun. The first is the case of a homogeneous gun, such for instance as a solid cast-steel gun as formerly made by Krupp, and we will assume it to be 9 inches calibre, and 15 inches thick at the breech end, and that it is subjected to an internal pressure of 24 tons per square inch. Now it is evident that the total strain to be resisted is 9 times 24 tons, or 216 tons, one half of which, or 108 tons must be borne by each side of the gun, or by a thickness of 15 inches of steel. If therefore the strain could be uniformly distributed, it would not exceed $\frac{108}{15}$, or 7·2 tons per square inch, but in reality the strain at the inside circumference would be nearly 27 tons per square inch, whilst at the exterior of the gun it would be only 2½ tons per square inch.

The subjoined diagram (A) represents the condition of



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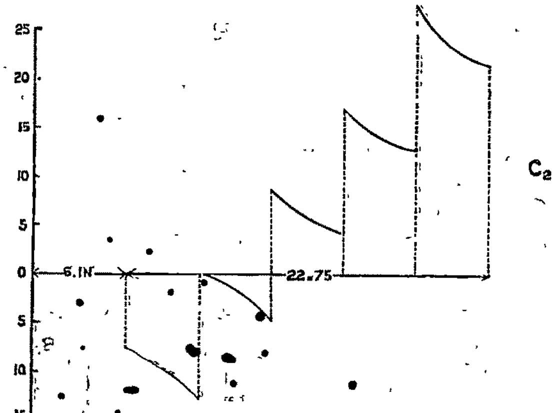

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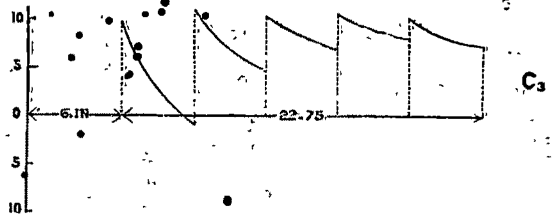


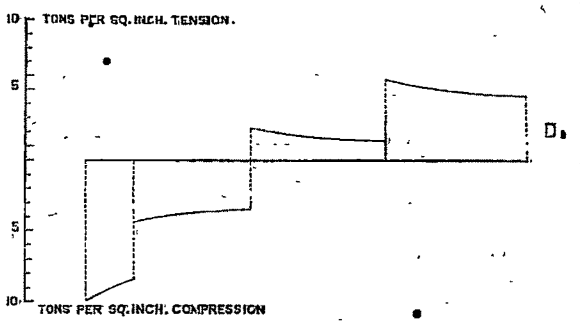
C3

strain of such a gun under these circumstances. The abscissæ denote the distances from the centre of the bore, whilst the corresponding ordinates denote the strains in tons per square inch at these distances.

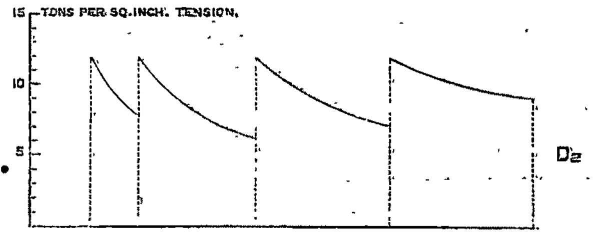
In the next place let us examine the condition of strain of a gun of the same calibre, but composed of an internal steel tube 3⅜ inches thick upon which is shrunk a wrought-iron hoop 12⅞ inches thick with a shrinking of 1 in a thousand. This was the Woolwich construction for all guns up to 9-inch calibre up to 1869.

Subjected to an internal pressure of 24 tons per square inch, the diagram B_2 shows the induced strains. Previous to the internal pressure being applied, diagram B_1 shows that the steel tube would be compressed by the outer wrought-iron hoop. The compression would be 11·7 tons per square inch at the inner and 7·86 tons at the outer circumference; on the other hand the wrought-iron hoop would be in a state of tension, 5·19 tons per square inch at the inner and 1·38 tons at the outer circumference. When the internal pressure of 24 tons per square inch is applied, the diagram B_2 shows the condition of strain. The steel tube would be strained to 15·53 tons per square inch at the inner, but only to 2·67 tons at the outer circumference, whilst for the wrought iron hoop the strains would be 15·09 and 4 tons respectively per square inch. Thus it appears that comparing this gun with the homogeneous gun of the same size and under the same conditions the maximum strain has been reduced from 27 tons to 15·83 tons per square inch.

Pursuing the matter further let us examine the conditions of Sir Joseph Whitworth's 12-inch gun, built up of a steel tube 4·35 inches thick, on which are placed four successive steel hoops, each of 5·55 inches thick, the total thickness of the gun being thus 26½ inches. Before proceeding to the examination of the strains in this gun, it is desirable to devote a moment or two to the very important question of the amount of initial strains with which hoops should be put on. The Woolwich practice is to adopt a uniform shrinkage of 1 in a 1000, that is to say, the internal diameter of each hoop is 999/1000ths of the external diameter of the hoop below it. The outer hoop is expanded by heat, placed over the inner one, and then in cooling grips it with the force due to a contraction of 1/1000th of its size. This is a fundamental error in the Woolwich practice, and it is mainly from their persistence in this error that so many Woolwich guns have failed. The proper amount of shrinkage is not a fixed amount. It depends on the thickness of the rings, their position in the structure, and the modulus of elasticity of the material, and it is only by a due regard to these



D1



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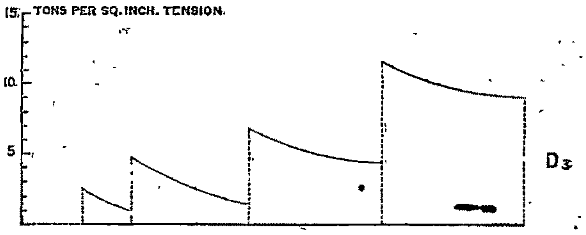

D3

elements of the problem that the advantages of the hoop system can be properly developed.

In illustration of this we refer to three diagrams of Sir Joseph Whitworth's 12-inch steel gun. The first, C_1, shows the strains, if the hoops are put in with no initial strain, that is to say, if each hoop is an exact fit to the one below it, which is Sir Joseph's present practice. The gun in this state is in the same condition under internal pressure as a homogeneous or solid gun of steel. The tensions with an initial pressure of 24 tons per square inch would be 28·18 tons and 2·3 tons per square inch at the inner and outer circumference respectively. The second diagram, C_2, would be the state of the strains, if the Woolwich rule of a uniform shrinkage of 1 in 1000 were adopted. The inner tube and the first hoop would never be out of compression, the second hoop would be strained to 8·44 tons and 3·85 tons, the third ring to 17·40 tons and 12·84 tons, and the fourth ring to 27·64 tons and 22·82 tons at the inner and outer circumferences respectively.

The third diagram, C_3, shows the gun as it would be strained if the initial shrinkages had been properly calculated and applied. For every hoop the tension of the inner circumference would be 10 tons per square inch, whilst that of the outer circumferences would be 1 ton compression for the tube, 4·11 tons, 6·51 tons, 7·72 tons, and 8·82 tons for the hoops respectively.

Thus it is seen that by a multiplication of hoops with initial strains properly applied the maximum strain is reduced from 28 tons to 10 tons per square inch. But on the other hand, by the Woolwich rule of a uniform shrinkage of 1 in 1000, some of the hoops would be always under compression, whilst others would be more or less strained, and the maximum would attain nearly the same as in the homogeneous gun—28 tons per square inch. Another remark must here be made. Referring to diagram C_3 it is seen that in the case of each hoop the strain decreases rapidly from the inner to the outer circumference. Thus in the first hoop the strain decreases from 10 tons to 4 tons, in the next from 10 tons to 6½ tons, and so on. Now by greatly increasing the number of hoops and consequently decreasing the thickness of each, the strains on the outer circumference may be brought very nearly up to the same strain as the inner circumference, and this is what is attained by the use of *wire*. A coil of wire is but a very thin hoop, and if, instead of a hoop of 4½ inches of steel, 36 coils of wire of ⅛th inch had been used, the difference of strain between the inner and outer circumference of each coil would be inappreciable, and the whole thickness of the gun would have been uniformly strained, and the maximum strain would not have exceeded 6 tons per square inch, or if the wire were strained to 10 tons per square inch the thickness of the gun might be reduced from 22¾ to 13½ inches.

But this is not all the advantage of the use of wire. Wire of small section is greatly stronger than the same material in mass. It is within the truth to say that steel which in mass might be safely strained to 15 tons per square inch, might in the form of wire be strained to 30 tons per square inch. Consequently the wire gun would be as safe under a strain of 20 tons as the hoops under 10 tons, and therefore the thickness of a wire gun of equivalent strength to that represented in diagram C_3 might be reduced to 6¾ inches instead of 22¾ inches.

From the preceding remarks and the diagram of Whitworth's 12-inch gun, it will be seen how very important is the question of the degree of shrinkage in built up guns. It is worth while to dwell a little longer upon this question, and to illustrate it we now give diagrams showing how the strength of a gun may be reduced by a small difference in the shrinking such as would be caused by a slight error in the dimensions of one of the hoops, due either to miscalculation, imperfect workmanship, or irregular contraction in cooling. The diagrams D_1 and D_2 represent the strains on the hoops of an 8-inch gun, built up of an inner tube and three concentric hoops of iron having an elastic limit of 12 tons per square inch. D_1 shows the strains when the gun is completed and free from internal pressure, on the hypothesis that the shrinkages are correctly calculated and accurately worked too. The tube and first hoop are in compression, the two outer rings in tension. D_2 represents the strain when subjected to internal pressure, so as to make the maximum strain 12 tons per square inch, and it is seen that all the hoops are equally strained up to the elastic limit. D_3 shows the strain in the same gun on the hypothesis that either from miscalculation or inaccurate workmanship the outer hoop has been made 1/500th of an inch too small, and when by internal pressure the maximum strain reaches 12 tons per square inch.

It is apparent at a glance what a great difference this error has made in the distribution of the strains. Without going into detail, it may be stated that the strength of the gun has been reduced 40 per cent. by the small error of 1/500th of an inch in one of the hoops. Accurate workmanship is, however, only one of the difficulties to be encountered in shrinking on hoops. Different qualities of iron shrink differently in cooling from the same temperature; moreover they do not shrink back in all cases to the size from which they were expanded, but to a somewhat smaller size. This depends on the temperature to which they have been heated. Moreover the shrinkage varies according to the number of times they have been heated. For instance, a wheel tier 7 feet diameter was heated red-hot, and cooled thirteen times in succession with the following results:—

1st	time	it contracted	⅝	in.	in length.
2nd	,,	,,	$\frac{11}{16}$	,,	,,
3rd	,,	,,	$\frac{7}{16}$	,,	,,
4th	,,	,,	⅝	,,	,,
5th	,,	,,	⅜	,,	,,
6th	,,	,,	¼	,,	,,
7th	,,	,,	¼	,,	,,
8th	,,	,,	⅝	,,	,,
9th	,,	,,	¼	,,	,,
10th	,,	,,	¼	,,	,,
11th	,,	,,	⅝	,,	,,
12th	,,	,,	¼	,,	,,
13th	,,	,,	⅛	,,	,,

Thus altogether it contracted 5⅜ inches from its original length of 22 inches.

It is clear therefore that however accurate the calculation and workmanship, there must be great difficulty in ensuring the exact amount of tension in this system of gun construction, and if guns are made without regard to calculation, without regard to the peculiar idiosyncracy of the iron, and without regard to the temperature from which the shrinking is made (and such is pretty much the case at Woolwich), it is no wonder that they split their tubes or shift their hoops in action. Many Woolwich guns have done this even under trial, and it is not improbable that in the late operations at Alexandria two of the guns of the *Alexandra* were injured in this way.

Another objection to this method of gunmaking is the possibility of latent defects in the hoops. It is impossible always to detect a flaw, even of considerable magnitude, in a hoop of iron or steel 10 to 18 inches thick such as are used in the large Woolwich guns, and such latent flaws may prove fatal to the gun even if in other respects it were properly constructed.

JAMES A. LONGRIDGE

(*To be continued.*)

MR. FORBES' ZOOLOGICAL EXPEDITION UP THE NIGER

MR. W. A. FORBES writes from Lokoja, on the Niger, at the confluence with the Binué (September 9) as follows:—I have been here on and off about a fortnight, and have been up the Binué as far as Loko, about 100 miles, where I got some birds. Altogether up to the present I have seen or got about 80 species of birds, including *Scopus, Plotus, Indicator*, and *Rynchops*; as yet no *Podica, Irrisor*, or Musephagidæ. Of Hornbill I have seen 3 or 4 species, but they are very shy, and as yet I have not shot one. Ploceine birds are the feature here; about 1-3rd of the species are of that family, and some I have are good ones, especially *Estrelda nigricollis* and *E. rara*, both of them discovered by Heuglin. These and other things make me fancy that we are out of the true West African region here; the antelopes seem also eastern. There are 4—5 here, including a brown *Hippotragus*, and what I fancy is *Alcelaphus tora*. I have skins and horns of these, and shall get others. *Bos brachyceros* is common here, but as yet I have only seen spoor, not the beast itself. We saw lots of Hippopotamuses coming up, and I killed the second I shot at, but could not recover the body.

I have also killed a large crocodile, 15 feet long, apparently *C. acutus*. I have also a few fishes and reptiles, and shall get more I hope. Butterflies are not very numerous at present, and the country is too open for them, being, generally speaking, a large grassy plain, with lots of isolated trees, not very big, and bushes. There is no regular thick forest up here at all, and even in the lower river, in the delta, it is nothing like the Neotropical forests. The weather has been very dry, and the river is still rising. After leaving Bidda our plans are uncertain. Mr. M. talks of going on to Sokoto, if he can get away from his stock-taking, and if he goes I shall probably go too. If not, I shall try and stay some time at Ischunga, a station a little off the river above Egga."

We are happy to be able to add that Mr. Forbes was in excellent health at the date of his letter.

WORK IN THE INFRA-RED OF THE SPECTRUM

IT is with a certain amount of dread of boring the readers of NATURE, that I have taken up my pen to write on the method of photographing with rays of very low refrangibility, since it ought to have passed the limits of novelty. And yet I suppose it has not altogether done so, since almost weekly, I have inquiries made as to where the method is described, and am questioned as to how to succeed with it, when my correspondents know where to find its description. The Editor, also, has asked me to write on the subject, so I propose to put as concisely as I can what plan to adopt. It is almost too well worn a scientific adage to repeat that unless you can obtain a sensitive salt which will absorb the rays to be used photographically, you cannot hope for success; and the method which I shall describe presently fully secures this desideratum. To photograph the red and dark rays then a sensitive salt must be procured which shall absorb the red and ultra-red rays. The colour of the salt to aim at then is a bluish green, which gives a continuous absorption at the least refrangible end of the spectrum. The salt employed is bromide of silver in a modified molecular state, a state I may say which is very easy to obtain when the formula below is strictly carried out, but very easily missed if the experimenter is self-inspired to make improvements in the method of procedure. I don't know whether it is something peculiar to photographic minds that there is in them such a large amount of self-assurance, but my frequent experience is that those who try a formula for a photographic preparation invariably try to improve on it before giving the original one a chance of success; and then when failure occurs they blame everything and everybody except their own conceptions. May I ask those who read this and endeavour to prepare the sensitive compound alluded to, to follow out strictly the directions as I described them in the Bakerian Lecture for 1880.

The following is the mode of preparation. A normal collodion is first made according to the formula below:—

Pyroxyline (any ordinary kind) . .	16 grains
Ether (·725 Sp.)	4 oz.
Alcohol (·820)	2 oz.

This is mixed some days before it is required for use, and any undissolved particles are allowed to settle, and the top portion is decanted off. 320 grains of pure zinc bromide are dissolved in ½ oz. to 1 oz. of alcohol (·820) together with 1 drachm of nitric acid. This is added to 3 ozs of the above normal collodion, which is subsequently filtered. 500 grains of silver nitrate are next dissolved in the smallest quantity of hot distilled water, and 1 oz. of boiling alcohol ·820 added. This solution is gradually poured into the bromized collodion, stirring briskly while the addition is being made. Silver bromide is now partially suspended in a fine state of division in the collodion, and if a drop of the fluid be examined by transmitted light it will be found to be of an orange colour.

Besides the suspended silver bromide, the collodion contains zinc nitrate, a little silver nitrate, and nitric acid, and these have to be eliminated. The collodion emulsion is turned out into a glass flask, and the solvents carefully distilled over with the aid of a water bath, stopping the operation when the whole solids deposit at the bottom of the flask. Any liquid remaining is carefully drained off, and the flask filled with distilled water. After remaining a quarter-of-an-hour the contents of the flask are poured into a well-washed linen bag, and the solids squeezed as dry as possible. The bag with the solids is again immersed in water, all lumps being crushed previously, and after half-an-hour the squeezing is repeated. This operation is continued till the wash water contains no trace of acid when tested by litmus paper. The squeezed solids are then immersed in alcohol ·820 for half-an-hour to eliminate almost every trace of water, when after wringing out as much of the alcohol as possible the contents of the bag are transferred to a bottle, and 2 ozs. of ether (·720) and 2 ozs. of alcohol (·805) are added. This dissolves the pyroxyline and leaves an emulsion of silver bromide, which when viewed in a film is essentially green-blue by transmitted light.

All these operations must be conducted in very weak red light—such a light, for instance, as is thrown by a candle shaded by ruby glass, at a distance of twenty feet. If a green light of the refrangibility of about half way between E and D could be obtained it would be better than the faint red light transmitted by ruby glass, since the bromide is less sensitive to it than to the latter. The light coming through green glass after being filtered through stained red glass is almost the best light to use. It is most important that the final washing should be conducted almost in darkness. It is also essential to eliminate all traces of nitric acid, as it retards the action of light on the bromide, and may destroy it if present in any appreciable quantities. To prepare the plate with this silver bromide emulsion all that is necessary is to pour it over a clean glass plate, as in ordinary photographic processes, and to allow it to dry in a dark cupboard.

It has been found advantageous to coat the plate in red light, and then to wash the plate and immerse it in a dilute solution of HCl, and again wash, and finally dry. These last operations can be done in dishes in absolute darkness; the hydrochloric acid renders innocuous any silver sub-bromide which may have been formed by the action of the red light, and which would otherwise cause a heated image.

Let me here give warning, that the emulsion formed will be very grainy in appearance, and requires vigorous shaking to cause it to emulsify proper. If it requires a little plain pyroxyline, say about two grains to the fluid ounce should be added to give greater consistency. One thing is certain, if it be not coarse-grained under the microscope it will not be sensitive to the required region, and moreover it will be found that on an average it should be about twice as coarse as the average form of bromide which is generally obtained in collodion emulsion. Here let me interpolate a remark. It has been assumed that because an emulsion in gelatine has a bluish colour after it has been boiled, that in this case we have the same form of bromide as that described above. It is a very different form: let me show how. Suppose we throw a spectrum on a gelatine plate it will be found that G requires about $\frac{1}{5}$ of a second with a very narrow slit, whereas to obtain B it will require the best part of a minute, and to obtain rays of lower refrangibility very much more; and that any amount of exposure will not make an impression much below A. With the blue-green bromide in collodion to obtain an impression about G will take some eight or ten seconds, and it will be found that at the same time we have an impression of B. A minute's exposure to the prismatic spectrum will under similar circumstances give an impression as much below A as D is above it, measured not in wave-lengths but along the photograph. I point out this because a leading continental photographic experimentalist has expressed himself satisfied as to the identity of the two forms of sensitive salt. They are totally distinct as if he tried to work with a gelatine plate in the infra-red region he will soon own. Now in reference to the coarseness of grain it is right to call attention to its disadvantages. Its advantage we know. In spectrum work we often come across close pairs of lines. Now suppose each pair happened not to be separated by a larger interval than the grain of the sensitive salt, we shall be unable to resolve such a pair, for the action of either component of the pair, and much more both, if they fell on it would be to cause, on development, a reduction to metallic silver of the whole grain. Thus evidently such a close pair would be unresolved.

When a photograph of the spectrum on the finest grained plate is examined under the microscope it will be found that the metallic image is composed of grains of silver and nothing else; and that instead of the lines having sharp edges as seen by the eye that they shade. Part of this shading is due to the grain, though the greater part is due to proper absorption, which the eye is incapable of distinguishing. The fineness of grain given by the different processes we may class as follows, in the order of coarseness, the coarsest grain being first:—

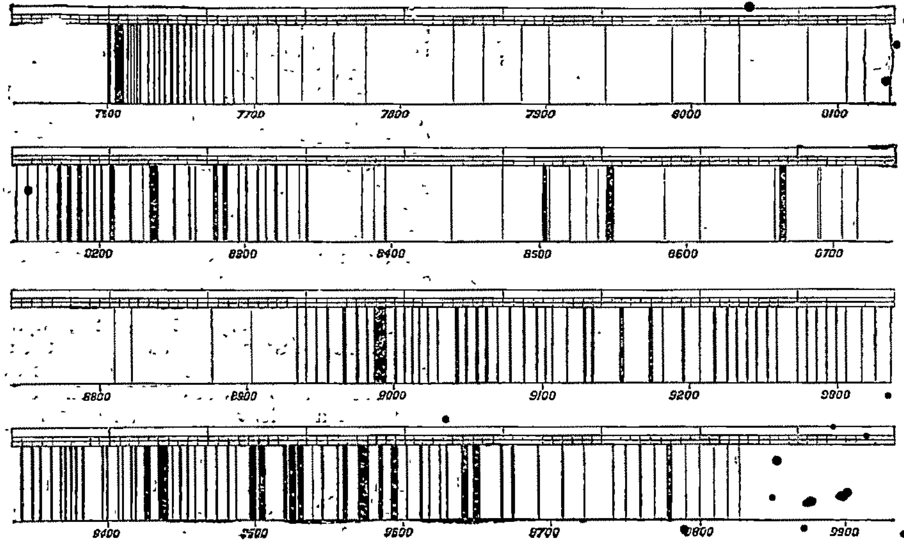

Fig. 1.

1. Wet plate developed by iron.
2. Special bromide emulsion, as before described.
3. { Ordinary collodion emulsion.
   { Wet-plate developed by pyrogalic acid.
4. Gelatino-bromide plates.

It will thus be seen that for delicate work the dispersion with the wet plate process and the special bromide emulsion must be larger than when using a gelatine plate if equal resolving power be wished for. The above plate is an instance of this. In it we have the solar spectrum in approximate wave-lengths from λλ (7,600) to about λ 10,500. The general impression to the eye is the extraordinary width of the lines compared with those in the visible spectrum. No doubt they are as a rule broader, but their breadth is also to be accounted for in other ways. First, the slit used was not quite as fine as might have been when the photographs were taken. Secondly, the dispersion used was the first order of a Rutherford grating 17,200 lines (about) to the inch, and a camera lens of a focus of about fifteen inches. In later photographs nearly all the broad lines have been resolved into pairs or triplets, as have also some of the lines of medium breadth. There are lines, however, like the 3 broad lines between 8500 and 8700 which remain unchanged whatever dispersion was used. This resolution was effected by using a finer slit and dispersion of the second order, the fine slit alone will not give it. If we take an example in the visible spectrum, and examine the B line with the eye, it will be found to be made up of a series of doublet flutings, each component being apparently of equal intensity. These pairs it is impossible to secure on the photographic plate, unless the second order of the grating spectrum is used; but when secured it will be found that the more refrangible component is more intense, as is the case in certain hydro-carbon flutings. The sole reason why the first order is useless to cause resolution is that the pairs are so close they can both fall on the diameter of the grain of the sensitive compound. On the other hand, with a gelatine plate I have been able to see on one inch and a half every line and more than given in Angström's map from G to F. In this case the grain is almost invisible.

The development of the plate is greatly more difficult than the preparation of the emulsion. A strong developer it will not stand, and I may say also that a very new one is also inadmissible when using the ferrous oxalate development. To make the developer a saturated solution of neutral oxalate of potash is saturated in the cold, with ferrous oxalate: and then the deep red solution decanted off. When freshly prepared it is useless to attempt to develop a plate with it unless the precaution be taken of adding to it an equal part of a saturated solution of ferric oxalate in the oxalate of potash. Such a mixture may be employed by adding to it immediately before all an equal volume of a solution of potassium bromide (twenty grains of the salt to thirteen of water). The plate may then develop without fog or it may not: if it does fog, the developer must have more bromide solution added to it, and another trial made. On some days a clean picture seems an impossibility, whilst on others, every one will be perfect. It is not the emulsion that is in fault, since, on a "clear day" and on a "foggy day" the identical emulsion may be used, showing that the developer is at fault. This year this trouble seems to have increased, and I can only lay it down to the different preparations of the oxalates. Of one thing care should be taken, viz., that the developer never shows alkalinity; a drop of dilute sulphuric acid or nitric acid may be added to the developing cup just before development with advantage.

With prisms the photography with the rays of low refrangibility is simple, with one great drawback, and that is the difficulty of obtaining a true focus for the plate. This must all be done by guess-work, and plates exposed till the focus is obtained. When once obtained it is a good plan to mark the camera to show the focus, and at the same time accurately to mark the table on which it stands, so that the *same portion of the lens receives the same rays.* This is more particularly necessary to attend to when using an achromatic lens. I believe it to be easier to use the uncorrected lens than a corrected one, provided always that the camera has a proper horizontal swing back, which can be shifted through a very large angle at least 30° when using three prisms. If a spherical mirror be used in the collimator and in the camera instead of the lenses, the same difficulties of focusing do not present themselves. The disadvantage of this method is that the edges of the spectrum based are diffused and not straight, and this is awkward when making comparison of different spectra. With a grating nearly the same difficulty arises when using lenses, but not quite to such a degree. If "a" and A be got in focus at the end of the plate, the swing back being used till this results, and if the lens be placed close to the grating, the whole of the infra-red region will be fairly in focus. This of course only applies to my own grating which may have a slight curvature. In using the grating we must not forget that the second order overlaps the first order, and the third order the second order and so on: and if a plate were exposed without any artifice being adopted to get rid of this overlap the plate would show two or three spectra. There are several methods of accomplishing this separation, the simplest being to use the absorbing medium in front of the slit. At first I used stained red glass which cuts off all radiation above the green, leaving thus the tails of the different spectra intact. At present, when wishing to go no further down the scale than λ 10,000, I have found that a deep coloured solution of iodine of potassium in water about one-tenth of an inch in thickness is very excellent. The objection to the red glass is that it exercises a certain amount of general absorption in the infra-red region, but with the white glass of the cell holding the solution, and the solution itself, this general absorption is minimized. To get down still further, very thin stratum of a blue dye in tetrachloride of carbon is efficacious in conjunction with the iodine solution. With the above solutions λ 13,000 can be reached. Beyond this limit it is necessary to use other means of eliminating the higher orders of the spectrum. The simplest plan is to place behind the collimator a couple of prisms, and, some two feet from the prisms, the grating so that it only receives those rays which it may be desired to impress. Thus one side of the grating may catch the limit of the red whilst the rest will be filled with the dark rays. The most difficult plan is to place a prism according (as Frauenhofer did) in front of the grating, in such a way that the axis of the prism is at right angles to the ruling and parallel to the plane of the grating; this causes a complete separation of all the different orders of spectra. But the resulting photographs are inconvenient to measure, since they are curved, and the position of the camera is also awkward. Another plan is to use a prism in front of the slit, but this too, I have found inconvenient for the same reasons as given above. For ordinary work the absorption method is decidedly the most elegant, but then it limits the operations with the spectrum. It was from photographs obtained in this manner that the above map of the solar spectrum was obtained, and as it is before us, it may be well to make a few remarks on it. As to what the lines are due to we are at present absolutely uninformed, except as to some very few. A notable exception to this is the line lettered about 8600, which is one of the strongest lines in this part of the spectrum. Colonel Festing and myself found that this line coincided absolutely in position with what we call the radical absorption band of benzene, that is to say, that by diminishing a thin layer of benzene placed between the slit of the spectroscope and a source of light giving a continuous spectrum, this absorption-band, amongst many others, was the last to disappear, and that it also was the key-note as it were of the absorptions of all benzene derivatives.

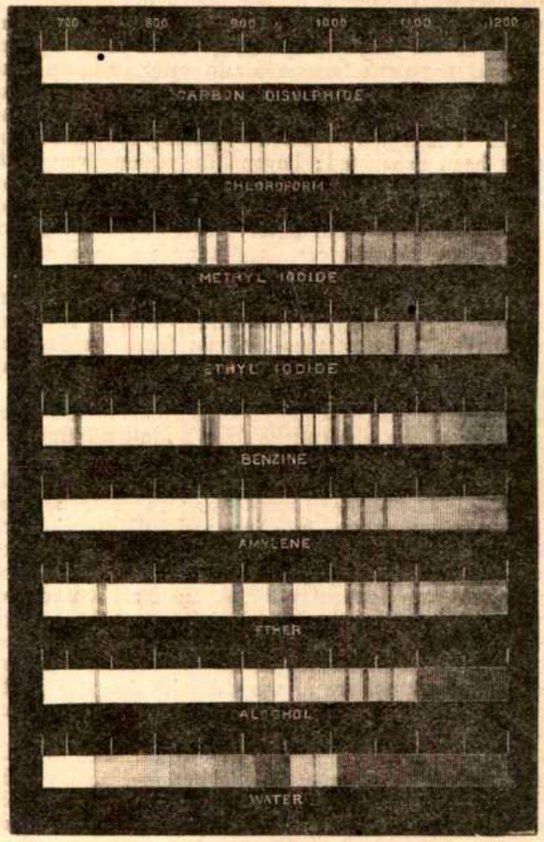

FIG. 2.

A coincidence of this kind would not be fortuitous any more than that the vapour of sodium gives lines coincident with the D lines; and hence we were forced to ascribe this line to benzene or some of its derivations. When first we made this announcement it was facetiously remarked that we had been photographing London smoke; and no doubt had not other localities for photographing the spectrum been chosen, the reproach (for such it was) might have been just. My visit last June to the Riffel, 8,500 feet high, showed that not only was this said line present, but that it was more intense even than at the level of the sea. There was more unfolding of the spectrum at that high altitude, and lines faint indeed, which had almost escaped registration below, were well marked on the photographs obtained there. The brilliancy of this infra-red spectrum can scarcely be surpassed. When examined at an elevation of 10,000 feet, the general absorption due to water almost vanishes, and with the exception of two congeries of lines which lie beyond those given in the diagram, the whole of the lines shown are stronger than I have ever had them before.

Colonel Festing and myself have also shown the presence of some alcohol derivative, somewhere between ourselves and the sun, and the presence of the absorption lines at a high altitude place it outside our atmosphere. This I was not wholly prepared for, since lately we have been told that alcohol exists in rain water, and rain water can only derive it from the air. The fact, however, remains that it probably exists beyond the limits of our atmosphere. The region disclosed by photography has by no means been exhausted; beyond the region given in the diagram lies one in which we have a breadth of continuous spectrum, and beyond that again beautiful groups of lines, all of which require and deserve careful study. Of one thing we may be fairly certain, that none of them are due to metallic vapours, but are probably due to vapours of non-metallic compounds in some form or another, and these at a comparatively low temperature. It is not unlikely that amongst these will be found oxygen compounds, and if so it would be interesting in more ways than one.

As a suggestion in which direction to look, I have annexed a diagram of the absorptions (Fig. 2), in the infra-red of a few liquids, by which it will be seen, that by a study of these we may perhaps throw some light on the solar spectrum. The bands in some instances where the liquid is vaporized are split up into lines and flutings, whilst the radical bands, to which I have already drawn attention, seem to remain constant. When it is remembered that one-tenth of an inch of a liquid, such as benzene, will give a definite absorption, it will be seen that a manageable length of vapour may be placed between the slit and the source of light, for its proper investigation. Colonel Festing and myself are at work at it at the present, but the field of investigation is so large that it requires more workers before any general theory can be brought to bear on the subject. It is partly to aid such would-be workers that I have penned the above, and shall be glad if it stirs up some few to aid in this research, which not only has a bearing on solar physics, but even still more largely on physical chemistry.

W. DE W. ABNEY

NOTES

WE have received a communication from Prof. Hildebrandsson, director of the Meteorological Observatory, Upsala, so well known for his researches into the upper currents of the atmosphere, in which, with reference to the proposed observatory on Ben Nevis, he remarks that "the erection on Ben Nevis of a permanent meteorological observatory is of *the utmost importance* for the development of modern meteorology. No better situation for a mountain observatory can be imagined. I have for a special purpose discussed the few observations published from Puy de Dôme. They are of great importance, but unfortunately this mountain, as well as the station of Gen. Nansouty on the Pic-du-Midi, has a bad situation in relation to storm tracks, being almost constantly placed on the north-westerly or south-easterly slope of a high pressure. On the contrary, Ben Nevis is situated almost in the middle track of the depressions or storms of north-western Europe. Hence observations made there must be of far greater importance in their relation to the theory of cyclones than the mountain observations in the south of France. I hope the Scottish Meteorological Society will find the means of carrying on this work." With these views of Prof. Hildebrandsson we heartily concur, and hope that the Council of the Scottish Meteorological Society will succeed in the patriotic effort we understand that they are now making to raise the necessary funds, viz. 5000*l.*, for the erection and partial endowment of this truly national observatory.

WE are glad to learn that Sir Edward Reed is so far recovered that he may be able in the course of a few days to give occasional attendance in Parliament.

THE International Electrical Conference which has been sitting in Paris for the last fortnight, has, after passing several resolutions, adjourned to the first Monday of October, 1883. In regard to electrical units it was resolved that at present there is not a sufficient concord of view to enable the numerical value of the "ohm" in the mercurial column to be definitely fixed, and that all governments be appealed to by France to encourage further research on the subject. The section for "Earth Currents and Lightning Conductors" resolved that Government should be requested to favour regular and systematic observations of atmospheric electricity upon their telegraphic systems; that it is important for the study of storms to be extended to every country; that wires independent of the telegraphic system should be provided for the special study of earth currents; and that, so far as possible, the great subterranean telegraphic lines, particularly those running north and west, should be utilised for the same purpose, observations being instituted on the same day in the various countries. The section for fixing a standard of light expressed the opinion that the light emitted by a square centimetre of melting platinum would furnish an absolute standard. In closing the Conference M. Cochery, the Postal Minister, assured the Members that the French Government would endeavour to give effect to their Resolutions by representations to the various Governments concerned. It is hoped that the twelve months for which the Conference is adjourned will be sufficient for the searches in the various departments in question to be completed. England is indebted solely to the private enterprise and spirit of Sir William Thomson for being represented at all. Between the French Government, the Foreign Office, and the Science and Art Department a sad mess has been made. The Post Office Telegraph Department was never asked to send a representative, nor have any of those who took such an active part in the Conference last year been asked to take any part in this. A more disgraceful muddle has never previously distinguished our "how not to do it" system.

M. MIGNET, Perpetual Secretary of the Academy of Moral Sciences, has just resigned the office held by him from the reorganisation of the Academy in 1835, up to the present time. Having been born at the end of the last century, his plea of old age may be said to be fully justified. It is stated on good authority that he will be succeeded by M. Jules Simon, who is now temporarily filling the office of *secrétaire perpetuel*.

THE annual meeting of the five French Academies, sitting as one body in the capacity of the French Institute, was held on October 25. M. Dumas, as director of the Académie Française, was in the chair. He opened the proceedings by an address, which quite fulfilled the expectations that had been raised. M. Dumas gave an elaborate history of the several academies of Paris, of their suppression in 1793, and their reopening in 1795 as the five classes of the Institute. The regulations adopted at the time were altered by the several monarchical governments, but have gradually resumed their former provisions, so that the present Institute may be said practically to exist as it was at the end of the last century. The subject was treated with wonderful eloquence and expression. M. Dumas derived the origin of modern scientific societies from the Academia di Lincei; he showed that the Academy of Sciences of Paris and the Royal Society of London came into existence about the same period, their meetings having been foreshadowed or instigated by the *conversazioni* held by the friends or followers of Descartes. M. Dumas insisted most on the grand spectacle exhibited by these institutions surviving monarchy, nobility, established churches, and finding in political revolutions a new field for their activity. He might have added, that even under the disorderly reign of the Commune, the sittings of the Academy were unmolested, and the editor of the *Journal Officiel de la Commune* did his best to report the sittings. The Academy of Sciences was represented in the addresses delivered by M. Alphonse Milne-Edwards, who gave a graphic account of the really good work done by the *Travailleur* in the Mediterranean and Atlantic. The large hall was crowded, and the whole proceedings were of high interest.

THE anniversary meeting of the London Mathematical Society will be held on the evening of Thursday, November 9, at 8 p.m., at 22, Albemarle Street. Mr. S. Roberts, F.R.S., has chosen as the subject of his valedictory address, "Some Remarks on Mathematical Terminology and the Philosophical Bearing of Recent Mathematical Speculations concerning the Realities of Space"; his principal aim will be to show that mathematics are neutral in philosophy. *Inter alia* he will report to the Society the fact of the establishment of the De Morgan memorial medal and the conditions of its being awarded. The following changes are proposed to be made in the Council for the ensuing session: Prof. Henrici, F.R.S., President, Sir J. Cockle, F.R.S., and Mr. Roberts, F.R.S., Vice-Presidents, and Messrs. E. B. Elliott and Dr. J. Hopkinson, F.R.S., to be new members in the place of Prof. Rowe and Mr. H. W. Lloyd Tanner, who retire.

THE *Japan Gazette* of August 21 contains a long and curious description of a bear festival among the Ainos. The writer, Dr. B. Scheube, is, we believe, the only European who has ever been actually present at this ceremony, the descriptions of it given by Miss Bird and other writers being derived from hearsay. The bear receives the title of *Kimui-Kamui*. The true derivation of this latter title—which is generally and incorrectly said to come from the Japanese *Kami*, a divinity—has been explained by Mr. Keane in NATURE (vol. xxvi. p. 525). The festival is now rarely held, and there is small reason to regret this, as it has degenerated to a brutal orgy. It commences with drink, every change in ceremony begins and concludes with drink, until finally every one in the village is intoxicated, while their hands, faces, and clothes are smeared with the gore of the sacrifice. Dr. Scheube says: "I had much difficulty in keeping off the drunken crowd that wanted me to partake of the blood and liver (the latter is eaten raw); and I can say that though hardened in these things by the practice of my profession, the sight of these drunken people with their bodies smeared over with blood filled me with a loathing that made me feel glad that the day and the feast were coming to an end together." Dances, many of them of an obscene nature, also form part of the ceremony.

A VERY business-like Annual Report from the Sheffield Free Libraries and Museum Committee has been sent us. Though complaint is made of the heavy cost of two of the branches, it is satisfactory to find that one of these rivals the Central Library in its number of volumes circulated. A new catalogue of the Central Library, which has been issued lately, we shall hope to notice more fully shortly. Besides the new branch of a musical department we may call attention also to the Observatory and the Museum of Natural History, with the hope that, in all our large towns eventually, the Free Library will become the centre of instruction in all knowledge.

THE *Electrician* learns that the improvements in the storage of electric energy and in electromotors have so far advanced, that tricycles can not only be lighted, but also propelled solely by electricity, as was seen from the tricycle ridden last week by Prof. Ayrton in the city. The Faure accumulators in which the energy was stored for the lighting and drawing, were placed on the footboard of the tricycle, and the motion was produced by one of Professors Ayrton and Perry's newly-patented electromotors placed under the seat of the rider. Using one of these specially-made tricycle electromotors and the newest type of the Faure accumulators, the total dead weight to be added to a tricycle to light and propel it electrically, is only one and a half hundredweight, a little more than that of one additional person.

WE wish to call the attention of our readers to the "Feuille des Jeunes Naturalistes," published monthly in Paris, with a London agency at 110, Leadenhall Street. Founded at Mulhouse in Alsace in 1870, the young journal was hardly launched before the national troubles began; the publication was removed to Paris, where the two first editors both perished during the war at the age of about twenty. The object of the journal is to establish a medium of communication between young naturalists, to encourage them to publish their earliest essays in a serial where they will be sure to find readers to be instructed and competent judges to guide them in their future studies. Every kind of trustworthy observation is welcomed; and the editors undertake to translate communications sent to them in English. The Journal is believed to have been instrumental in the formation of several local natural history societies.

THE St. Petersburg Society of Gardening is taking the necessary steps to prepare the International Botanical and Gardening Exhibition and Congress, which will take place in the Russian capital. Professors Beketoff, Borodin, Famintzin, Marklin, and Maximowitsch, and Messrs. Annenkoff, Gobi, Iversen, Semenoff, and Wolkenstein are elected members of the scientific committee; three other committees—for the Exhibition, for the erection of buildings, and for the reception of guests—were appointed at the last meeting of the Society.

WE have received a copy of the syllabus of the Yorkshire College Students' Association. The society was founded in 1877, and is now in its sixth session. The number of members is large, and the meetings have hitherto been very successful Attention is devoted to literature as well as to science. An excellent programme of papers is down for the present session which began on October 24 with an address by the president, Prof. Thorpe, on "The Story of the Origin of the Metric System."

THE German Ornithological Society held its annual meeting at Berlin recently under the presidency of Baron Homeyer. Mr. Schalow (Berlin) read a paper on the progress of ornithology during the last five years; Prof. Landois (Münster) on egg shells considered from a histological and a genetic point of view; Mr. Mutzel (Berlin) on the call of the Tragopan; and Prof. Blasius the report of the stations for observing the migrations of birds in Germany.

TELEGRAMS from the south-east of Europe report that there was an earthquake in the northern part of the Balkan Peninsula on October 25. At 1.26 p.m. the shocks were felt severely at Preboi, in Bosnia. They lasted fully three seconds, the direction of the vibrations being from west to east.

THE first General Meeting of the Members of the Parkes Museum, since the incorporation of the Museum, was held on

Saturday last. Capt. Douglas Galton, C.B., was voted to the chair. It was unanimously resolved that H.R.H. Prince Leopold, Duke of Albany, who had graciously consented to accept the presidency, be formally elected to that office. Capt. Douglas Galton, in replying to a vote of thanks for presiding, said that the Museum had now entered on a fresh phase of existence, and had established itself as an independent institution in premises which, after necessary alterations had been completed, bid fair to serve its purpose, for the present at least, admirably. The Council contemplated making the sanitary arrangements necessary for the Museum itself as perfect as possible, and it was intended that all such arrangements should be useful for teaching purposes; the drainage, for instance, had been carefully considered by Prof. Corfield and Mr. Rogers Field, M. Inst. C.E., and the latter gentleman had generously undertaken to bear the whole expense of carrying it out. Mr. Twining had undertaken the whole trouble and cost of arranging, and for the most part of providing the Food Collection; the Warming, Lighting, and Ventilating, have been referred to a Special Committee, whose endeavour it would be to insure that every appliance was the best of its kind. The general collection was to be carefully weeded and re arranged, and it was hoped that the Museum would be opened to the public soon after Christmas.

The name *isanemones* has been recently applied by M. Brault to curves of equal velocity of wind, and he has made a drawing of such curves for the North Atlantic in summer, using for the purpose 240,000 observations on board ship. It is shown that an approximate numerical value may be attached to each of the ordinary terms used in ship's logs to denote the wind's force. M. Brault's map, which appears in *Comptes Rendus*, is remarkable in that it reproduces almost exactly the map of *mean isobars*. Thus, during summer, that is to say, when the atmosphere is most stable over the great North Atlantic basin, the mean *isanemones* and the mean *isobars* are the same, presenting only differences that are nearly equal to possible errors of observation and of construction. It remains to be seen in what measure this important law is general; M. Brault believes it to be so for every surface of the globe which is under what he calls fundamental maxima and minima (such as the maximum and minimum of Asia, the maximum of the Azores), the fixity and permanence of which are such that they form together, and at six months' interval two distinct systems which suffice to define the two great phases of the annual circulation. (*Ephemeral* maxima and minima are such as appear and disappear daily in our latitudes; while *mobile* or *tempestuous* minima such as cyclones or squalls, are grouped as a third class.)

In his work on worms, Darwin has described some tower-like dejections which he never saw constructed in England, but which are attributed to an exotic species of *Perichæta*, from Eastern Asia, naturalised in the environs of Nice. M. Trouessart has lately observed similar dejections in gardens near Angers. Having collected a large number of worms from where the towers were made, he found no species of *Perichæta*, nor of any other exotic genus. In two or three cases he surprised the worms at work, and they were *Lumbricus agricola*. It was the anterior part of the body that was lodged in the tower. After the rainy period at the end of September all the tubular interior of each tower (forming a continuation of the subterranean gallery) was quite free; but a few days later it was obstructed by recent dejections. M. Trouessart supposes that the calotte or cap of the tower, getting hard in air, a time comes when the worm can no longer burst the upper wall as before, to place its dejections outside (so increasing the height of the tower), but deposits them within. Thus a long period of rain is necessary for these towers to rise regularly. The towers probably serve to protect the galleries from rain, and to afford a breathing place for the worms, where they are not seen by birds.

We learn from the *Rivista Scientifico-Industriale* that Baron V. Cesati has resolved to sell his botanical collection. This consists of a herbarium of about 32,000 phanerogamic species, also a special cryptogamic herbarium containing at least 17,000 species; altogether more than 350,000 plants. There is also a collection of autographs of 2500 botanists. Any one wishing to purchase is desired to apply to the owner, at the Botanical Gardens of Naples. Full particulars of the herbaria will be given.

In the construction of a railway bridge recently over the Ticino, electric illumination has been used instead of that with stearine candles (previously preferred for the compressed air caissons). The hygienic conditions of the workmen in the caissons is thus greatly improved; as stearine candles impregnate the atmosphere with smoke. Eight lamps of the small Swan type are used to light the working chamber; a Siemens' dynamo of about 30 lamp-power supplying the current. A second dynamo is kept in reserve, to be used in case of breakdown or excessive heating. The additional cost of the system is regarded as largely compensated by the increased comfort in working.

The additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii* ♂) from South Africa, presented by Mr. G. H. Jones; nine Hairy-footed Jerboas (*Dipus hirtipes*), twenty-four —— Gerbilles (*Gerbillus* ——) from Arabia, presented by Lieutenant Paget, R.N.; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. H. G. Austin; a Ceylon Jungle Fowl (*Gallus stanleyi* ♂) from Ceylon, presented by Mrs. Dick Lauder; a Spinose Land Emys (*Geomyda spinosa*) from Borneo, presented by Miss C. G. Robson; two Sharp-headed Lizards (*Lacerta oxycephala*) from Madeira, presented by Mr. H. J. Clements; three European Tree Frogs (*Hyla arborea*), European, presented by Miss L. Burness; a Rhesus Monkey (*Macacus erythraeus* ♂) from India, a Malbrouck Monkey (*Cercopithecus cynosurus*) from East Africa, deposited; two Canadian Beavers (*Castor canadensis*) from Canada, an Eyra (*Felis eyra* ♂), two Sun Bitterns (*Eurypyga helias*), a Brown Gannet (*Sula leucogastra*) from South America, two Globose Curassows (*Crax globicera* ♂ ♀) from Central America, a Razor-billed Curassow (*Mitua tomentosa*) from Guiana, a Greater Shearwater (*Puffinus cinereus*) from Lincolnshire, six Knots (*Tringa canutus*), a Lapwing (*Vanellus cristatus*), British, a Matamata Terrapin (*Chelys matamata*) from the Amazons, purchased; a Muscovy Duck (*Cairina moschata*) from South America, received in exchange.

OUR ASTRONOMICAL COLUMN

Schmidt's Cometary Object.—We have received a circular (No. 48) of the Imperial Academy of Sciences of Vienna, containing a letter from Dr. Julius Schmidt, dated Athens, October 14, in which he notifies his discovery of a nebulous object not far from the head of the great comet, which will be best given in his exact words. He writes:—"Seit October 9, 16·5h. liegt in S.W. neben dem Kometen eine der Form nach stark variable cosmische Nebelmaterie, welche die scheinbare Geschwindigkeit des grossen Kometen zwar etwas übertrifft, doch im Ganzen der Bewegung desselben entspricht." Dr. Schmidt appends the following places, the first and last being from measures, the second deduced from a star-chart.—

1882.	M.T. at Athens.	Apparent R.A.	Apparent Decl.	Dist. from nucleus of principal comet.
	h. m.	h. m. s.	° ′	° ′
Oct. 9	16 54	10 15 53	−12 53	3 24
10	16 36	10 10 26	−12 43	4 25
11	16 37	10 5 51	−14 33	5 21

On submitting these positions to calculation by the ordinary method of Olbers for a parabolic orbit, Mr. Hind has found the following elements, the second set being the result of the corrected value for the ratio of the curtate distances at the extreme observations, though the representation of the middle place is not sensibly improved thereby :—

Perihelion passage,	Sept. 24·2778 G.M.T.	Sept. 24·0912
Long. of perihelion	234 42·6	232 21·5
,, ascending node ...	350 2·4	354 50·9
Inclination	29 11·5	29 41·9
Log. perihelion distance ...	8·11394	8·26678
Motion—retrograde.		Retrograde.

The general resemblance of these elements to those of the great comet, will excite remark. The middle observation shows a difference from computation of − 6″·2 in right ascension, and − 3′·2 in declination by the second orbit; perhaps unavoidable error in an estimated place, or vagueness of the nebulosity may account for the differences, yet Dr. Schmidt speaks of having observed "Die Positionen des eines Kernes des seitlichen Nebels." Further, it may be observed that the orbit in which the great comet is now moving does not accord with the positions given by Dr. Schmidt: thus with the last elements published in NATURE, the observed and computed right ascension on October 9 will agree if the perihelion passage be assumed to have occurred September 13·732, but the calculated declination is north of that observed by 1° 39′, and for the observation on October 11, the calculation is in error +1° 56′ in right ascension and +2° 31′ in declination. Nevertheless the general similarity in the arrangement of the elements suggests a past connection of the two bodies, and it may be hoped that further light will be thrown upon the question, if either earlier or later observations of Dr. Schmidt's object are forthcoming.

COMET 1882 *b*.—In an unusually clear sky for the season on the morning of October 23, a fine view of this comet was obtained in the vicinity of London; the length of the more definite portion of the tail was about 16½°. At 5 a.m. on October 30, with strong moonlight and a somewhat vaporous sky, it was still conspicuous, notwithstanding the material diminution in the theoretical intensity of light. If the tail extended in the same direction from the nucleus on both dates, there was a large increase in its real dimensions in the course of the week. In fact, on October 30, if we assume the tail to have been a prolongation of the radius-vector, it would cover a space considerably greater than the mean distance of the earth from the sun, and with any reasonable assumption as to deviation from that line, its true length could hardly have been less than 70,000,000 miles.

The place given by M. Cruls for the comet he found at Rio Janeiro on September 12 a.m., differs 5° 43′ in right ascension, and 1° 25′ in declination from that occupied by the great comet at the time.

From an observation at the Collegio Romano, in Rome, on the morning of October 25, kindly communicated by Prof. Millosevich, it appears that the elements last published in this column were in error −2′·4 in right ascension and −0′·3 in declination, small differences, considering that the last observation used in their determination was made on October 1, and a proof of the precision of the observations issued from the Collegio Romano.

GEOGRAPHICAL NOTES

THE Council of the Geographical Society have made the final arrangements for their new African expedition under Mr. Joseph Thomson. Mr. Thomson hopes to leave England in the end of November for Zanzibar, where he will stay some months getting together his retinue and goods, and making other provisions for his hazardous journey. He will probably leave the coast in April or May next. The field of the new expedition lies to the east and north-east of Lake Victoria Nyanza, and may include a running survey of the eastern shore of the lake. The expedition will probably start from Pangani, and ascend the river of that name as far as Kilima Nyaro, whence they will proceed direct to Victoria Nyanza. The route after that will depend much on circumstances, but Mr. Thomson hopes to visit the reputed Lakes Bahringo and Samburu, as also Mount Kenia. Probably Mounts Kenia and Kilima Nyaro will be more carefully examined than they have been, and beyond them the country to be traversed by the expedition is almost totally unexplored. On its borders we meet with the names of such travellers as Denhardt, Krapf, New, Wakefield; but the field is practically virgin. A great part of the region is a wilderness, rendered so by roving Masai, whose depredations have scattered the population and rendered culture impossible. Besides the danger from these roving freebooters, the expedition will be compelled to carry its own provisions to a large extent, as there is no likelihood of getting a regular supply on the spot. Water, too, it is feared, will be scarce, so that on the whole Mr. Thomson will have a trying task before him. The expedition will be purely geographical, but it is almost certain that a naturalist will accompany Mr. Thomson as far as Kilima Nyaro, partly at the expense of the British Association. Mr. Thomson will, however, be in no way responsible for the safety or the conduct of the naturalist's party. It is probable that Dr. Aitchison, who did good work in natural history during the Afghan war, will be selected for this work, and his retinue and all his arrangements will be quite independent of those of Mr. Thomson; the two parties will simply go together so far as their route is in common.

MR. STANLEY has published separately a full report of the address he recently gave in Paris. From this we glean one interesting item of exploration. After he had launched his steamer on the upper waters of the Congo, above the cataracts, he proceeded up the river and entered the Kwango, the great southern tributary. One hundred miles from its mouth he came to where two large streams united to form the main river; a greyish-white stream from south by east, the other, of an inky colour, from east by south. Ascending the latter, much less rapid than the former, Mr. Stanley came, after steaming another 120 miles, to a large lake, into which the river widened. On circumnavigating it, he found it about seventy miles in length, and with a breadth varying from six to thirty-eight miles. The natives he found very wild, and naturally astonished at the puffing monster. A splendid country the shore seemed to be —dense, impenetrable—lofty forests, alternating with undulating grass lands. Mr. Stanley was altogether three years away from Vivi, and doubtless he has collected much information in the country around the Congo. If the five stations established on the banks—one at the mouth of the Kwango—are left unmolested, much material of value to science may be collected; they are superintended by Europeans, who have all the apparatus for taking meteorological and other observations.

ON Sunday, October 29, the Paris Society of Topography distributed its medals in the large Hall of the Sorbonne. M. de Lesseps was in the chair. The three great medals were awarded to M. D. Brazza, M. Roudaire, and Commander Perier. One of the others to M. Triboulet, treasurer of the Academy of Aérostation Météorologique, for his continuous efforts in aerial photography and the success obtained nineteen days ago in photographing the horizon visible from a captive balloon, with an apparatus put in operation from the ground.

AT the last meeting of the Section of Physical Geography of the Russian Geographical Society, M. Grigorieff made a report on the results of Arctic exploration during last summer; W. E. Fuss read a communication on his visit to Novaya Zemlya, which was made to determine accurately the position of the new meteorological station; and M. Rykatcheff a communication on meteorological observations he made during an ascent in a balloon.

THE students of the Physical and Mathematical Faculty of St. Petersburg have presented M. Miklukho Maclay with an address of thanks for his valuable researches, and express the wish that the results may soon be given to the world.

A RECENT issue of the *North China Herald*, published at Shanghai, contains an article on a Chinese work entitled "Travels in India." The work is of interest as exhibiting the impression made on an intelligent Chinese traveller by the results of Western civilisation. The author, Huang Mao-ts'ai, is, it appears, a literary graduate of Kiangsi, who became impressed with the importance to China of knowing what is going on in neighbouring countries, and accordingly obtained, in 1878, a commission from the Governor of Szechuen to pass through Thibet to India. Arriving at Patang he was deterred by the hostility of the hill-tribes from proceeding further in that direction, and he therefore retraced his steps, turning southward into Yunnan, whence he crossed into Burmah, and descending the Irawaddy to Rangoon, he took passage for Calcutta. He spent six months in India, returning to China by Singapore and Saigon.

R176

His four volumes and maps were laid before the throne, and he was rewarded with an appointment in Yunnan. Around China he sees on all hands powerful and aggressive neighbours. To the ambitious schemes of these powerful neighbours and the means of checkmating them he devotes many pages. He dreams even of conquest, and suggests that by encouraging emigration to the southern seas, establishing consuls to look after the emigrants, opening schools to enlighten them in foreign science, and at the same time keeping up the knowledge of their native language, the great islands of that region could be made to fall like ripe fruit into the lap of China. In the territorial acquisitions of other countries Mr. Huang finds three degrees of villainy, which he describes respectively as "stealthily beguiling," "encroaching by degrees," and finally "swallowing up." Notwithstanding the offensive discrimination of these terms, he exhibits a high appreciation of English rule in India. In the latter country, he says, there are no idle officers; each has his sphere, into which no other intrudes. The will of each high functionary is limited by his council. Salaries are sufficiently liberal to prevent extortion. All are animated by a regard for their own good name. The law is faithfully executed and public spirit prompts to efforts for the general good. He is struck by the magnificence of Calcutta and its great public works. On the subject of taxes, he says: "The ground is taxed, houses are taxed, shop-signs are taxed, all manner of beasts are taxed, all handicrafts are taxed, and even fire and water are taxed. There are other taxes more than I can mention; yet you do not hear one murmuring word from the people. Why is this? It is owing to two causes: Firstly, they regard the humane Government of the English as a great improvement on the oppressive cruelty of their native rulers: and secondly, they are aware that the revenue thus collected is expended for the good of the country—in making roads, founding schools, and so on." The author is so impressed by the railway system of India that he is extravagant in his advocacy of something similar in China. He wants a railway from the north-western frontier of China proper into Ili, as the only means of retaining that province and Kashgaria. In reply to objections on the score of the enormous expense of this undertaking, he exclaims with true Chinese vanity: "What other countries can do, China can do, as she is ten times richer, and a hundred times more populous."

NOTICE OF SOME DISCOVERIES RECENTLY MADE IN CARBONIFEROUS VERTEBRATE PALÆONTOLOGY

In the course of my work upon the carboniferous rocks of the neighbourhood of Edinburgh, I have succeeded in obtaining several specimens which throw some additional light upon the little known Selachians of the Palæozoic age. It was considered a great step in advance when Prof. Kner, in Germany, and Sir P. Egerton in England, proved that the spine of the tooth known as *Diplodus*, which occurs frequently in Carboniferous rocks, was the equally well-known *Pleuracanthus*, a geuns of not infrequent occurrence in the same beds. A very interesting slab from the ironstone of Burghdee, near Edinburgh, in the Carboniferous Limestone series, advances our knowledge another important stage. Upon it there are several teeth of the species *Diplodus parvulus*, Traq., associated with cranial cartilage, and a spine which is certainly not *Pleuracanthus*, but is totally unlike it, and one which does not appear to have been ever described. Upon showing it to my friend, Dr. Traquair, he said it confirmed an opinion at which he had long since arrived, that the *Diplodus* tooth would be found common to several genera of Selachian fishes. It certainly was a singular fact, and one which must have struck those palæontologists who have most carefully examined the fish-faunas of particular beds and horizons, that the number of the species of spines usually exceed those of teeth. Another important conclusion may be drawn from this discovery, viz. that spines are of very little value in relation to the affinities of sharks. Nothing can be more different than the spine of *Pleuracanthus* and that of *Diplodus parvulus*, Traq.

These conclusions are supported by another specimen in a nodule obtained from a much lower horizon, viz. on that of the Wardic Shales at Hailes Quarry, near Edinburgh. Here we have a *Hybodont* tooth associated with the spine known as *Tristychius*. The tooth, indeed, cannot be distinguished from *Hybodus*; it is deeply furrowed as in many of the Mesozoic species, and has the two depressed lateral cusps. This form of tooth is very persistent, extending from the Lower Carboniferous to the Chalk. Germar was the first, I think, to point out the existence of a Hybodont tooth in rocks of Carboniferous age, but (though I have not yet carefully examined his figures and description) the spines appear to be different from those I find associated with the Hailes specimen, though they appear to me to be of the same general type. That a *Tristychine* spine, with its smooth surface and strongly arcuate shape, should be associated with a *Hybodus* tooth is certainly unexpected, and shows again the necessity of caution in dealing with spines, for the Mesozoic spines associate with *Hybodus* are very different from *Tristychius*. *Hybodus* and *Diplodus* are therefore generalised forms of teeth associated with spines known as *Tristychius*, *Pleuracanthus*, with one undescribed genus, probably with many others. Messrs. Hancock and Atthey, to whom British science is indebted for some of the most important ichthyological observations made since Agassiz' time suggested the possibility of *Cladodus* being the tooth of *Gyracanthus*. I have seen nothing to confirm or refute this suggestion. They also referred certain small tooth-like bodies with success to the dermal skeleton of that genus. I have obtained a nodule from the Wardic shales, which has these in a remarkably good state of preservation in connection with a large fragment of the fin of that powerful shark. These dermal denticles are so closely approximated to each other that they form a dense covering, through which however appear distinctly traces of the skeleton of the fin. The occurrence of the genus at so low a horizon is of itself deserving of record, and in addition to this fragment, I have found imperfectly preserved speci- of spines of the same genus at the same place.

The remains of Labyrinthodonts are exceedingly scarce below the Burdichoun horizon. I am not aware of more than one having been discovered, and that proves to be *Ophiderpeton*, or a closely allied genus. This specimen was discovered in the Wardic shales, low down in the Calciferous sandstone series. The position of the Wardic shales in the Carboniferous series has not yet been exactly defined. Owing to the confused nature of the rocks, and the fact that they are so deeply covered with drift in a good deal of the Edinburgh area, it has not been found possible to settle quite clearly the relative position of the different members of the Carboniferous series. Nevertheless the opinion appears to be universal that the shales along the shore between Seafield and Granton are very low in the Carboniferous system. All that I have seen confirms this conclusion. I was amused, indeed, to see them in an otherwise well got up map, lately published, coloured as the Millstone grit! Antiquated, surely! The fossils are generally identifiable with those which are everywhere found to underlie the marine limestones (in the Scotch beds, at any rate), and from all that the drift will let one see, there must be several thousands of feet of such rocks with the Wardic and Granton beds near the base. This being so, the occurrence of this vertebrate so low down is of interest and importance, and helps to confirm Prof. Fritsch's view, arrived at in his case from anatomical considerations, that *Ophiderpeton* and its allies are the roots of the Amphibian genetic tree.

T. STOCK

A NUMERICAL ESTIMATE OF THE RIGIDITY OF THE EARTH[1]

ABOUT fifteen years ago Sir William Thomson pointed out that, however it be constituted, the body of the earth must of necessity yield to the tidal forces due to the attraction of the sun and moon, and he discussed the rigidity of the earth on the hypothesis that it is an elastic body.

If the solid earth were to yield as much as a perfect fluid to these forces, the tides in an ocean on its surface would necessarily be evanescent, and if the yielding be of smaller amount, but still sensible, there must be a sensible reduction in the height of the oceanic tides.

Sir William Thomson appealed to the universal existence of oceanic tides of considerable height as a proof that the earth, as a whole, possesses a high degree of rigidity, and maintained that the previously received geological hypothesis of a fluid interior was untenable. At the same time he suggested that careful observation would afford a means of arriving at a numerical estimate of the average modulus of the rigidity of the earth's mass as a whole. The semi-diurnal and diurnal tides present phenomena of such complexity, that it is quite beyond the power of mathematics to calculate what these heights would be, if the earth's mass were absolutely unyielding. But the tides of long period are nearly free from the dynamical influences which render those of short period so intractable to calculation, and must in fact nearly follow the laws of the "equilibrium theory."

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[1] Paper read by G. H. Darwin, F.R.S., at the British Association Southampton meeting.

In 1867 it was not, however, even definitely known whether or not the tides of long period were of sensible height at any station. Although there has been a continual advance in the knowledge of tidal phenomena since that time, it is only within the last year that there is a sufficient accumulation of tidal observations, properly reduced by harmonic analysis, to make it possible to carry out Sir William Thomson's suggestion. The great advances in knowledge that have been recently made are principally due to the adoption of systematic tidal observation at a great number of stations by the Indian Government. The results of these observations are now being issued yearly by the Secretary of State for India in the form of tide-tables for the principal Indian ports. I have had the pleasure of carrying out the examination of the tidal records, and a detailed account of the work will appear at § 848 of the new edition of Thomson and Tait's "Natural Philosophy," now in the press.

The tides chosen for discussion were the lunar fortnightly declinational tide, and the lunar monthly elliptic tide. These tides must be free from the meteorological disturbances which make the heights of all the solar tides quite beyond prediction. The fortnightly and monthly tides consist in an alternate increase and diminution of the ellipticity of the elliptic spheroid of which the sea-level (after elimination of the tidal oscillations of short period) forms a part. There are two parallels of latitude respectively north and south of the equator which are nodal lines, along which the water neither rises nor falls. When, in the northern hemisphere, the water is highest to the north of the nodal line of evanescent tide, it is lowest to the south of it, and *vice versâ*; and the like is true of the southern hemisphere. If the ocean covered the whole earth, the nodal lines would be in latitudes 35° 16′ N. and S. (at which latitudes $\frac{1}{3} - \sin^2 lat.$ vanishes); but when the existence of land is taken into consideration, the nodal latitudes are shifted. Now according to Sir William Thomson's amended equilibrium theory of the tides, the shifting of the nodal latitudes depends on a certain definite integral, whose limits are determined by the distribution of land on the earth's surface.

For the purpose of examining the tidal records, it was therefore first necessary to evaluate this integral. Approximation is of course unavoidable, and for that end the irregular contours of the continents were replaced by meridians and parallels of latitude, and the integral evaluated by quadrature. This procedure will give results quite accurate enough for practical purposes. It appeared as the result of the quadrature that, if we assume the existence of a large Antarctic continent, the latitude of evanescent tide is 34° 40′, and if there is no such continent it is 34° 57′. Hence the displacement of the nodal latitudes due to the existence of land is very small.

This point having been settled, the mathematical expressions for the fortnightly and monthly tides are completely determinate, according to the equilibrium theory, with no yielding of the earth's mass.

If there is yielding of the earth, either with perfect or imperfect elasticity, and with frictional resistance to the motion of the water, the height of tide and the time of high water must depart from the laws assigned by the equilibrium theory. This conclusion may also be stated in another way, which is more convenient for practical purposes; for we may say that at any station there must actually be a tide with a height equal to some fraction of the full equilibrium height, and with high water exactly at the theoretical time, and a second tide, of exactly the same nature, with a height equal to some other fraction of the equilibrium height, but differing in the time of high water by a quarter-period from the theoretical time, viz. about three-and-a-half-days for the fortnightly, and a week for the monthly tide. These two tides may, according to geometrical analogy, be called perpendicular component tides. According to the theory of the composition of harmonic motions, the two components may be compounded into a single tide, with time of high water occurring within a half-period of the theoretical time; and this is the way in which the results of elastic yielding and frictional resistance were first stated above. Thus the actual tide at any station involves two unknown fractions, x and y, being the factors by which two components, each of the full theoretical height, are to be multiplied in order to give the two components in proper amount to represent the reality.

If the equilibrium theory is fulfilled without sensible elastic yielding of the earth, the first component has its full value, or x is equal to one, and the second component vanishes, or y is zero. If fluid friction exercises a sensible influence, y will have a sensible value; and if the solid earth yields tidally, x will be less than unity. The amount of elastic yielding, and hence the average modulus of elasticity of the whole earth may be computed from the value of x. After rejecting the observations made at certain stations for sufficient reasons, I obtained from the Tidal Reports of the British Association and from the Indian Tide Tables, the results of thirty-three years of observation, made at fourteen different ports in England, France, and India.

These results, when properly reduced, gave thirty-three equations for the x and thirty-three for the y of the fortnightly tide, and similarly thirty-three for the x and thirty-three for the y of the monthly tide; in all 132 equations for four unknowns.

The x and y of the two classes of tide were in the first instance regarded as distinct, but the manner in which they arise shows that it is legitimate to regard them as identical, and thus we have sixty-six equations for x and sixty-six for y.

The equations were then reduced by the methods of least squares, with the following results:—

For the fortnightly tide—

$$x = \cdot675 \pm \cdot056, \quad y = \cdot020 \pm \cdot055.$$

And for the monthly tide—

$$x = \cdot680 \pm \cdot258, \quad y = \cdot090 \pm \cdot218.$$

The numbers given with alternative signs are the probable errors.

The very close agreement between the x and y for the two tides is probably somewhat due to chance.

The smallness of the two y's is satisfactory; for, as above stated, if the equilibrium theory were true, they should vanish. Moreover, the signs are in agreement with what they should be, if friction is a sensible cause of tidal retardation. But considering the magnitude of the probable errors, it is of course more likely that the non-evanescence of the y's is due to errors of observation or to the method of reduction.

I have already submitted to the British Association at this meeting a paper on a misprint, discovered by Prof. Adams, in the tidal report for 1872. This report forms the basis of the method of harmonic analysis which has been employed in the reduction of the tidal observations, and it appears that the erroneous formula has been systematically used. The large probable error in the value of the monthly tide may most probably be reduced by a correct treatment of the original tidal records.

It has been already remarked that it is legitimate to combine all the observations together, for both sorts of tide, and thus to obtain a single x and y from sixty-six years of observation. Carrying out this idea, I find:

$$x = \cdot676 \pm \cdot076, \quad y = \cdot029 \pm \cdot065.$$

These results really seem to present evidence of a tidal yielding of the earth's mass, and the value of the x is such as to show that the effective rigidity of the whole earth is about equal to that of steel.

But this result is open to some doubt for the following reason:—

Taking only the Indian results (forty-eight years in all), which are much more consistent than the English ones, I find

$$x = \cdot931 \pm \cdot056, \quad y = \cdot155 \pm \cdot068.$$

We thus see that the more consistent observations seem to bring out the tides more nearly to their theoretical equilibrium values with no elastic yielding of the solid.

It is to be observed however that the Indian results being confined within a narrow range of latitude give (especially when we consider the absence of minute accuracy in my evaluation of the definite integral) a less searching test for the elastic yielding than a combination of results from all latitudes.

On the whole we may fairly conclude that, whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and that the effective rigidity is at least as great as that of steel.

SCIENTIFIC SERIALS

The Journal of Physiology, vol. iii. Nos. 5 and 6, August, 1882, with Supplement number. No. 11 contains:—Optical illusions of motion, by H. P. Bowditch and G. S. Hall.—On reflex movements of the frog under the influence of strychnia, by G. L. Walton.—A contribution to our knowledge of the action of certain drugs upon bodily temperature, by H. C. Wood and E. T. Reichert.—Influence of Peptones and certain inorganic salts on the diastatic action of saliva, by R. H. Chittenden and J. S. Ely.—On cerebral localisation, by S. Exner.—The physiological action of methylkyanethine, by G. L. Walton.—On the influence of variations of intra-cardiac pressure upon the inhibitory action of the vagus nerve, by H. Sewell and F. Donaldson.—Preliminary observations on the innervation of the heart of the tortoise, by W. H. Gaskell.—Concerning the influence exerted by each of the constituents of the blood on the contraction of the ventricle, by S. Ringer (plate xix.).—The Supplement contains a list of works and papers on physiology published in 1881.

The American Naturalist for October, 1882, contains:—Sketch of the progress of North American Ichthyology in the year 1880-81, by W. N. Lockington.—On the methods of microscopical research in the zoological station at Naples, by C. O. Whitman.—On the homologies of the crustacean limb, by A. S. Packard, jun.—On the idols and idol worship of the Delaware Indians, by C. C. Abbott.

Journal de Physique, September.—Dynamo-electric machines with continuous currents, by M. Potier.—Influence of a metal on the nature of the surface of another metal placed at a very small distance, by M. Pellat.

SOCIETIES AND ACADEMIES

LONDON

Mineralogical Society, October 24.—Anniversary Meeting. W. H. Huddleston, F.G.S., president, in the chair.—Nine new Members were elected.—The officers and Council were elected for the ensuing year, the only changes being the election of Messrs. T. D. Gibb, T. M. Hall, Jas. I'Anson, and H. M. Plattnault, on the Council in place of Dr. Aitken, Professors Crum Brown and Hughes, and Mr. Louis, who retired in rotation.—It was resolved to hold the meetings of the Society at fixed dates for the ensuing year, viz, on December 13, 1882, February 15, May 15, and October 23 (Anniversary), the meeting for May to be held in Scotland.—The Report of the Council was read and adopted.

PARIS

Academy of Sciences, October 25.—M. Blanchard in the chair.—Herr Wiedemann presented the first volume of a new work by him, "Die Lehre von der Electricitat."—On the effect of a stroke of an inclined cue on a billiard ball, by M. Résal.—Separation of gallium (continued), by M. Lecoq de Boisbaudran.—Contribution to the study of typhoid fever in Paris; the present epidemic, from September 22 to October 19, 1882, by M. de Pietra Santa. There have been 2225 deaths this year (up to the latter date), more than during the whole of last year 2130), (628 in the last four weeks). All the twenty arrondissements have been affected, and all the eighty quarters, except the four American and that of St. Fargeau in the west and Salpêtrière and Petit-Montrouge in the south. The seventh arrondissement has suffered most. M. Santa notices the unwholesome state of the houses.—On a bed of coal discovered in the province of Algiers, and on layers of white sand accompanying it, by M. Pinard. This is near Bou Saada. The coal is at least equivalent in illuminating power and yield of gas to the best French and English coal. The yield of coke varies between 62 and 66 per cent. The sand, which might be used for the finest glass, and is very abundant, is the product of disaggregation of immense banks of grit.—Results of modes of treatment adopted in 1881–82, in the Alpes-Maritimes, for destruction of phylloxera, by M. Laugier. More than 200 hectares have been treated with sulphide of carbon and sulphocarbonate of potassium, and the results are very satisfactory.—Observations of the great comet (Cruls) at the Observatory of Marseilles, by M. Borrelly.—Spectroscopic observations on the same comet, by MM. Thollon and Gouy. On October 9, the sodium lines seen on September 11, had disappeared; the four ordinary carbon bands were present; the nucleus gave a narrow continuous spectrum with many dark and bright lines. On the 16th the violet band was almost gone, and the continuous spectrum considerably weakened. The disappearance of the sodium lines and others observed by M. Lohse shows that under ordinary conditions the spectroscope cannot give us a complete analysis of cometary matter. If the temperature is sufficient to produce the emission spectrum of carbon compounds, it should be sufficient to produce that of sodium; but the facts are contrary. The authors incline to the electric theory of comets; in the case of a gaseous carburet traversed by the efflave from a Holtz machine, and holding fine metallic dust in suspension, the carbon bands appear, but not the metallic lines.—Relations between the residues of a function of an analytic point (x, y), which is reproduced, multiplied by a constant, when the point (x, y) describes a cycle, by M. Appell.—On the hyper-geometric functions of two variables, by M. Goursat.—Decomposition of a whole number N into its maximum nth powers, by M. Lemoine.—Lunar induction and its periods, by M. Quet.—On the automatic transmission and registration of messages of optic telegraphy, by M. de Brettes. A claim of priority.—On metallic thorium, by M. Nilson. He has reduced thorium by heating with sodium, the anhydrous double chloride of thorium and potassium, and adding to the mixture chloride of sodium; all in an iron crucible. The specific gravity of the pure metal is about 11·00; the substance, as prepared by Chydenius (density 7·657–7·795), was probably impure. For atomic volume, M. Nilson gets the value 21·1 (coinciding with the atomic volumes of zirconium (21·7), cerium (21·1), lanthanum (22·6), and didymium (21·5)).—Determination of the equivalent of thorium, by the same. The equivalent is equal to 58·10, if that of oxygen = 8 and that of sulphur = 16.—On benzylene orthotoluidine and methyl phenanthridine, by M. Etard.—On the reduction of nitrates in arable land, by MM. Deherain and Maquenne. An earth loses the property of reducing nitrates when it has been heated or submitted to chloroform vapours. Earth that has lost the property through heat, reduces anew when a little normal earth is added.—On the convulsing action of curare, by M. Couty. Curare is not only a paralysing poison, but also in the first place, slightly convulsing; nor merely a peripheric poison, but also, in certain measure, a poison of the nerve-centres.—On parasites of the blood in impaludism, by M. Laveran. He has observed them in 300 cases.—Isanemones of summer in the North Atlantic, by M. Brault. These curves of equal velocity of wind coincide with the isobars.—On turriform constructions of earth-worms in France, by M. Trouessart. He observed them in gardens in Angiers, and found they were produced by *Lumbricus Agricola*. Darwin knew only of this production by a *Perichaeta* naturalised at Nice, from the east. M. Gautrelat, in a note, affirmed that M. Le Bon's glyceroborate of soda is not a definite salt, but a mixture of monoborine (monoboric ether of glycerine), sub-borate of soda, and glycerine.—A map, by M. Durand Claye was presented, showing the increase of population in the department of the Seine, and adjacent parts of the department of Seine-et-Oise. The variations of growth are indicated by means of curves called *isoplethes*.—Some documents from M. de Lesseps, relating to construction of the hospital of Panama, by the Canal Company, were presented.

CONTENTS

PAGE
HYDRAULIC EXPERIMENTS 1
LETTERS TO THE EDITOR:—
  "Weather Forecasts"—The BISHOP of CARLISLE 4
  The Comet—Major J. HERSCHEL; GEO M. SEABROKE; ARTHUR WATTS 4
  The Burman—SHWAY YOE; Dr. E. B TYLOR, F R.S. 5
  River Thames—Abnormal High Tides—J. B. REDMAN 6
  Umdhlebi-Tree of Zululand—W. T. THISELTON DYER, C.M G., F.R S., Rev. Dr. G. W PARKER 7
  The Origin of our Vernal Flora—J. E. TAYLOR 7
  On Coral-eating Habits of Holothurians—Surgeon-Major H. B. GUPPY 7
  Railway Geology—a Hint.—J. C. G. 8
  Complementary Colours—JOSEPH JOHN MURPHY 8
  Palæolithic River Gravels.—C. EVANS 8
LAVOISIER, PRIESTLEY, AND THE DISCOVERY OF OXYGEN By G. F. RODWELL 8
A NEW DREDGING IMPLEMENT. By A. MILNES MARSHALL 11
WIRE GUNS. By JAMES A LONGRIDGE. C E (*With Diagrams*) 11
MR. FORBES' ZOOLOGICAL EXPEDITION UP THE NIGER 14
WORK IN THE INFRA-RED OF THE SPECTRUM. By Capt W. DE W. ABNEY, R.E., F R S (*With Diagrams*) 15
NOTES 18
OUR ASTRONOMICAL COLUMN:—
  Schmidt's Cometary Object 20
  Comet 1882 *b* 21
GEOGRAPHICAL NOTES 21
NOTICE OF SOME DISCOVERIES RECENTLY MADE IN CARBONIFEROUS VERTEBRATE PALÆONTOLOGY. By T. STOCK 22
A NUMERICAL ESTIMATE OF THE RIGIDITY OF THE EARTH. By G. H. DARWIN, F.R.S. 22
SCIENTIFIC SERIALS 23
SOCIETIES AND ACADEMIES 24

THURSDAY, NOVEMBER 9, 1882

A SEARCH FOR "ATLANTIS" WITH THE MICROSCOPE

THE revival of the idea of a former "Atlantis" has given rise in recent years to much ingenious argument. The presence of so many widely separated islands or groups of islets along the depression filled by the Atlantic Ocean has to some writers been in itself sufficient proof of a submerged continent, the islands remaining still above water as the last visible relics of the foundered land. The same conclusion has been drawn from the Atlantic soundings, which have undoubtedly shown the existence of a long ridge running down the length of the Atlantic at an average depth of some 2000 fathoms from the surface. From this ridge rise the oceanic islands of Tristan d'Acunha, Ascension, St. Paul, the Azores, and Iceland. Other writers have invoked the former presence of land over the Atlantic area, from the difficulty of otherwise accounting for the resemblance of the flora in North America and Europe during later geological times. On the other hand, it has been forcibly argued that in every case the peaks of the supposed submerged land are of volcanic origin, that not a single fragment of any truly continental rock has been detected on any of these islands, and therefore that no evidence can be adduced save of a submarine ridge on which volcanic cones have gradually been built up above the sea-level. Reasoning based on similar data furnished by the other great oceans, and also upon the evidence supplied by the stratified rocks as to the permanence of the continental areas, has led many thoughtful geologists to regard the ocean-basins as primeval depressions of the globe's surface, and consequently to reject the tempting hypothesis of a lost Atlantis.

This vexed question was one on which it was hoped that the *Challenger* Expedition might cast new light. The careful surveys of the ocean-floor made by that Expedition, and the attention it paid to the nature of the emergent peaks were precisely the kinds of direct observation needed to supply facts in place of previous mere speculation. We must patiently await the completed Reports before the final answer of the *Challenger* observers is given. But an interesting and important instalment of evidence and argument has just been published in the form of a "Report on the Petrology of St. Paul's Rocks," by M. Renard of Brussels, whose name is itself a guarantee for the accuracy and exhaustiveness of the memoir. Sent in to the *Challenger* authorities as far back as October, 1879, it is now issued as Appendix B in volume ii. of the *Narrative of the Expedition*.

No more typically oceanic an island anywhere rises out of the deep than the lonely wave-washed rocks of St. Paul. Lying nearly on the equator and between 500 and 600 miles to the east of the South American coast, these rocks consist of four principal rugged horse-shoe-shaped masses not a quarter of a mile in their greatest length, and mounting into five peaks, the highest of which does not exceed 60 feet in height. Their bare rough summits have a yellowish tint that deepens into black towards sea-level. So utterly barren are they that not a plant of any kind—not even a lowly lichen—clings to their sterile surface. Are these rocks the last enduring remnants of a continent that has otherwise disappeared, or are they portions of a volcanic mass like the other islands of the same ocean?

To those who have not noted the modern progress of geological inquiry it may seem incredible that any one should propose to solve this problem with the microscope. To seek for a supposed lost continent with the help of a microscope may seem to be as sane a proceeding as to attempt to revive an extinct Ichthyosaurus with a box of lucifer matches. Yet in truth the answer to the question whether the St. Paul's rocks are portions of a once more extensive land depends upon the ascertained origin of the materials of these rocks, and this origin can only be properly inferred from the detailed structure of the materials, as revealed by the microscope. The importance of microscopic examination in geological research, so urgently pressed upon the notice of geologists for some years past, has sometimes been spoken of disparagingly as if the conclusions to which it led were uncertain and hardly worth the labour of arriving at them. We occasionally hear taunts levelled at the "waistcoat-pocket geologists," who carry home little chips of rock, slice them, look at them with their microscopes, and straightway reveal to their admiring friends the true structure and history of a whole mountain-range or region. That the sarcasm is often well-deserved must be frankly conceded. Some observers with the microscope have been so captivated by their new toy as to persuade themselves that with its aid they may dispense with the old-fashioned methods of observation in the field. But there could not be a more fatal mistake. The fundamental questions of geological structure must be determined on the ground. The microscope becomes an invaluable help in widening, and correcting the insight so obtained; but its verdict is sometimes as ambiguous as that of any oracle. In any case it must remain the servant not the master of the field-geologist.

Perhaps no more suggestive example could be cited of the use of the microscopic study of rocks even in the larger questions of geological speculation than that which is presented by an examination of the material composing the islets of St. Paul. These rocks were described many years ago by Mr. Darwin as unlike anything he had ever seen elsewhere, and which he could not characterise by any name. He found veins, of what he believed to be serpentine, running through the whole mass. The observers of the *Challenger* Expedition looked upon the St. Paul's Rocks as composed of serpentine. But these remote islets have never until now been subjected to modern methods of petrographical investigation. M. Renard has studied them chemically and microscopically, and finds them to be composed of a granular olivine-rock, containing chromite, actinolite, and enstatite. A remarkable structure is presented in the thin sections when seen under the microscope. The large crystals or grains of olivine and enstatite are arranged with their vertical axes parallel to the lines of certain bands in which the minuter constituents are grouped, the whole aspect of the section suggesting at once a movement of the component particles in the direction of the bands. When the rock was first sliced and examined by the naturalists of the

Challenger some years ago this minute structure was looked upon as what is known to petrographers by the name of "fluxion-structure," such as may be seen in obsidian and other volcanic rocks, the ingredients of which have arranged themselves in layers or planes according to the direction in which the mass while still molten was moving. The same view was at first adopted and published by M. Renard. He now, however, expresses himself more doubtfully on the subject, and indeed is rather inclined to class the rock among the crystalline schists.

Now the importance of the point in question will be at once perceived when it is stated that if St. Paul's Rocks belong to the series of schists, they must once have lain deeply buried beneath overlying masses, by the removal of which they have been revealed. They would thus go far to prove the former existence of much higher and more extensive land in that region of the Atlantic; land too, not formed of mere volcanic protrusions, but built up of solid rock-masses, such as compose the framework of the continents. If, on the other hand, the rock is volcanic, then the islets of St. Paul belong to the same order as the oceanic islands all over the globe.

M. Renard reviews the arguments so cautiously that only towards the end do we discover him rather inclining to the side of the crystalline schists. With all deference to so competent an authority, however, we venture to maintain that the balance of proof is decidedly in favour of the volcanic origin of the rock. In the first place, as the distinguished Belgian petrographer himself admits, the law of analogy would lead us to expect the peridotite of St. Paul to be a volcanic protrusion. So cogent, indeed, is the argument on this head that, unless some irrefragable evidence against it is furnished by the rock itself, it must be allowed to decide the question. When the rock is studied under the microscope it presents precisely the banded fluxion-structure of true lavas, thus corroborating the inference of a volcanic origin for the mass. To say that this structure also resembles the foliation of true schists is to repeat what may be remarked of hundreds of examples of undoubtedly eruptive rocks. Unless some peculiarity can be shown to exist in the St. Paul's rock inconsistent with the idea of its being a volcanic extravasation, we are surely bound to regard it as no exception to the general rule that all oceanic islands are fundamentally of volcanic origin. M. Renard, however, fails to adduce any such peculiarity. He appears to have been led to doubt the validity of his first conclusions, and, be it also remarked, those of other observers, by finding so many published instances of peridotic rocks among the crystalline schists. A bed of peridotite among a group of schists, however, need not be of contemporaneous origin, any more than an intrusive sheet of basalt can be supposed to have been deposited at the same time and by the same processes that produced its associated sandstones and shales. Synchronism is not necessarily to be inferred from juxtaposition. We do not mean to dispute the assertion that some peridotites belong to the series of crystalline schists. But others are most assuredly eruptive rocks. It is among these that we should naturally seek for analogies with the rock of St. Paul.

To sum up the reasoning we may infer that, judging from the structure of other oceanic islands, the material comprising the rock of St. Paul should be of volcanic origin; this inference is confirmed by chemical and microscopical analysis, and especially by the discovery of a minute structure in the rock identical with that of many lavas, though a similar structure can be recognised in some schists; the islets of St. Paul furnish therefore no evidence of an ancient land having formerly existed in the middle of the Atlantic Ocean, on the contrary they have probably been built up on the submarine Atlantic ridge by long continued volcanic eruption like the other islands of the same Ocean.

The exhaustive methods of research employed by M. Renard in the study of the rock of St. Paul furnish an excellent illustration of the great strides made in recent years by petrography. The other rocks collected by the *Challenger* Expedition are to be treated in the same manner, but it is understood that instead of being thrown into separate Reports the petrographical details will be interspersed through the "Narrative" at the places where the localities are described. These contributions will form not the least important parts of this great work, the advent of which has been so long and so patiently waited for.

ARCH. GEIKIE

THE LIFE OF CLERK MAXWELL

The Life of James Clerk Maxwell, with a Selection from his Correspondence and Occasional Writings, and a Sketch of his Contributions to Science. By Lewis Campbell, M.A., LL.D., Professor of Greek in the University of St. Andrews, and William Garnett, M.A., Late Fellow of St. John's College, Cambridge, Professor of Natural Philosophy in University College, Nottingham.

THIS volume will be heartily welcomed by all who knew Clerk Maxwell, and who cherish his memory, and by the still wider circle of those who derive pleasure and new vigour from the study of the lives and work of the great men that have gone before them.

The work consists of three parts, a biography with selections from Maxwell's correspondence, a popular account of his scientific work, and a selection from his poetry, both juvenile and of later years, including the serio-comic verses on scientific subjects, some of which are already so well known.

The biography is mainly the work of Prof. Lewis Campbell, whose schoolboy friendship and life-long intimacy with Maxwell amply qualified him for the task.

As far as vicissitudes of fortune are concerned, the life of Clerk Maxwell was absolutely uneventful. Worldly struggles he had none; from the very first he was warmly, if not always quite fully, appreciated by all whose good opinion he could have valued; promotion such as he cared for came almost unsought, and scientific distinction of the honorary kind was conferred upon him unstintedly while he lived to enjoy it. But in truth all these things moved his serene spirit as little as they disturbed his outward life; the interest of his biography lies in tracing the growth of a mind which was dedicated, literally from infancy, to the pursuit of science, and which nevertheless neglected nothing becoming a man to know. For unity of aim and singleness of heart, for high-minded neglect of the worldly strife that is begotten of vanity, ambition, or love of gain, for the steadfast pursuing of a path remote from the ways of ordinary men, the life of Maxwell stands in our mind associated with the lives of Gauss and Faraday. Nevertheless, without seeking to compare him with either of these great men in respect of intensity of genius, we may safely assert that he was superior to both in universality and many-sidedness. The mere objective circumstances of the career of such a man count for little, and the biographer tells his tale so far as these are concerned, with an artless grace that befits the subject. It is needless to dwell upon them here, for our readers have already been furnished with a summary of the outward events of Maxwell's life (NATURE, vol. xxi. p. 317). The interest and freshness of Prof. Campbell's story lie in the light it throws on the subjective influences that moulded the character of the gentle physicist, a character which was the most extraordinary combination, that this generation has seen, of practical wisdom, childlike faith, goodness of heart, metaphysical subtlety, and discursive oddity, with wonderful critical sagacity and penetrating scientific genius.

Intellectual power, and to some extent also eccentricity, appear to have been hereditary with Maxwell, as will be seen from the racy notes at the end of the first chapter on the Clerks of Penicuik and the Maxwells of Middlebie. After the early loss of his mother, he became the constant companion and confidant of his father, who initiated him into all his economic mysteries, interested him in applied sciences of every kind, encouraged his boyish essays in physical experimenting, and anxiously patronised his earliest memoir, on Cartesian Ovals and kindred curves, read to the Royal Society of Edinburgh by Forbes when its author was a boy of fourteen. This sympathy between father and son continued unbroken to the end, and had undoubtedly the happiest effect on Maxwell's destiny.

The chapters on the student life at Edinburgh and Cambridge are deeply interesting, and we earnestly commend them to the young men of our time who wish not to seem, but to be indeed, men of science.

With his appointment to the chair in the Marischal College, Aberdeen, begins his career as a recognised authority in scientific matters. Henceforth the biography is mainly an account of Maxwell's contributions to the advancement of physical science; the purely personal interest revives in the sad chapter that recounts his last illness and death.

Glimpses into his mental history during the later period of his life are afforded us by means of extracts from his intimate correspondence, and from essays, some read at Cambridge to a select circle of friends, others, not intended for publication even to that limited extent, but merely written as records of the author's communion with his own soul. We thus learn how the great physicist dealt with the grand problem of man's relation to that which went before, and that which shall be hereafter. It cannot but be profoundly interesting to read what was thought on such a subject by one of the greatest scientific minds of our day. We are left in no doubt as to the solution in which Maxwell ultimately reposed, and it is instructive to note how in this respect, as in so many others, he was akin to Faraday. Some will doubtless think that needless emphasis is laid upon the exact form of the final solution, and upon the precise methods by which it was reached. It must be remembered that the difficulties of the man of action and of the scientific man or professed thinker, are widely different. The former rests naturally in the arms of precept and dogma; he is distracted merely by the choice of preceptor and authority. The thinker by profession *must* examine for himself; it is a necessity of his nature so to do; and his difficulties arise from having to deal with matters in which the best of his scientific methods fail. Thus it happens that the example of a scientific mind is little likely to profit the unscientific; and that one scientific mind is scarcely in such matters to be led by the experience of another. The solutions of the great problem by different minds of the highest order have, as we know, differed, in outward appearance at least, very widely. But is it well to dwell on these differences? seeing that no man of finite intellect can tell how little or how great after all the distances may be that separate the resting places in the infinite of good men and true.

With regard to the selections from the correspondence it might have been better perhaps, in the interest of science, to have given more of the scientific correspondence. It must be known to many of our readers from pleasant experience that Maxwell was indefatigable in writing and answering letters on scientific subjects. His letters rarely failed to contain some sagacious criticism, some ingenious thought, or some valuable suggestion. Most possessors of such letters would we imagine be glad to put them at the disposal of a competent editor for publication, or at all events to take some steps to prevent the ultimate loss of matter so full of interest for all scientific men. Those that have read the volumes containing the correspondence of Gauss with Bessel and Schumacher will understand how instructive such collections can be.

Not the least interesting parts of the biography are the chapters containing extracts from the occasional essays already referred to. Maxwell when a student at Edinburgh had attended the lectures of Hamilton, and had been greatly impressed by that distinguished philosopher and accomplished enemy of the exact sciences. Accordingly, we find that among the studies of his earlier years mental and moral science had no small share. He resolves, for instance, at one period to read Kant and to make him agree with Hamilton, and, at the same time, he criticises in a somewhat unflattering strain the flaccid morality embodied in the lectures of Christopher North. It is not surprising, therefore, that the subjects of these occasional essays are mainly metaphysical or psychological. They are mostly very discursive, and their graver meaning is often veiled in a cloud of that humorous irony which figured so much in his familiar conversation. The general tendency is, however, sufficiently plain: in the essay on Psychophysik, for example, he thus delivers his opinion on the theory of "Plastidule Souls," which played so prominent a part lately in the classic duel between Virchow and Haeckel, and in sundry ultra-physical discussions nearer home:—

"To attribute life, sensation, and thought to objects in which these attributes are not established by sufficient evidence, is nothing more than the good old figure of personification."

At the end of the same essay he thus sums up the answers that have been given to the great ontological problem "What am I?":—

> "In this search for information about myself from eminent thinkers of different types, I seem to have learnt one lesson, that all science and philosophy, and every form of human speech, is about objects capable of being perceived by the speaker and the hearer; and that when our thought pretends to deal with the Subject, it is really only dealing with an Object under a false name. The only proposition about the subject, namely, 'I am,' cannot be used in the same sense by any two of us, and, therefore, it can never become science at all."

Prof. Campbell has succeeded in presenting to us a most vivid picture of Maxwell's character. The view which he gives will be fresh, and partly strange, to many even of those who knew Maxwell well. It is no reproach to him to say that, in our opinion, he has by no means exhausted the different aspects of his subject. So many-sided was Maxwell's character, that it would have required the united efforts of several biographers to do it the fullest justice.

In the second part of the book will be found a good account by Mr. Garnett, of Maxwell's scientific work. Of this nothing further need be said, for an excellent summary has already been given in the pages of NATURE by Prof. Tait (vol. xxi. p. 317).

It may be questioned whether the literary merit of many of the pieces of occasional poetry in the third part will be sufficient to secure for them the interest of the general reader; but many will greet with pleasure the reappearance of old friends among the serio-comic verses. We are glad to find among them our favourite, "To the Committee of the Cayley Portrait Fund"; finer compliment to a mathematician surely never was penned. Among those hitherto unpublished may be mentioned the Paradoxical Ode to Hermann Stoffkraft, beginning as follows:—

My soul's an amphicheiral knot,
Upon a liquid vortex wrought
By Intellect, in the Unseen residing.
And thine doth like a convict sit,
With marlinspike untwisting it,
Only to find its knottiness abiding;
Since all the tools for its untying
In four-dimensioned space are lying,
Wherein thy fancy intersperses
Long avenues of universes,
While Klein and Clifford fill the void
With one finite, unbounded homaloid,[1]
And think the Infinite is now at last destroyed.

We ought to mention in conclusion that the book is beautifully illustrated; there are vignettes of Maxwell and of his father and mother; some quaint and suggestive illustrations of scenes from his early life, after originals by Mrs. Blackburn; and a variety of diagrams, several of them beautifully coloured, reproduced from originals—by Maxwell's own hand—in illustration of his researches on light and colour. G. C.

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[1] Here the author takes a poetic licence.

OUR BOOK SHELF

Description Physique de la République Argentine d'après des Observations Personelles et Étrangères. Par le Dr. H. Burmeister. (Buenos Ayres, 1876-82.)

SOME account of the progress of this extensive work, in which the veteran naturalist, Dr. H. Burmeister, formerly of Halle, proposes to give a complete physical history of his adopted country, may not be unacceptable. Of the octavo text, which is accompanied by folio atlases, in order to give the illustrations on a large scale, we have seen four volumes, numbered 1, 2, 3, and 5. The fourth volume, which we suppose will contain the birds, is not yet issued, and the atlases in some cases do not appear to be complete.

The first volume (issued in 1876) is devoted to the history of the discovery and general geographical features of the Argentine Republic; and the second, published in the same year, to its climate and geological conformation. The third volume, of which the text was issued in 1879, has been already noticed in our columns (NATURE, vol. xxiv. p. 209). It contains an account of the Mammal-fauna both recent and extinct. We have now just received the first *livraison* of the folio atlas to this volume, containing a series of plates illustrating the whales of the Argentine coasts, a subject to which Dr. Burmeister has devoted special attention for many years. Of the fifth volume, devoted to the Lepidoptera of Buenos Ayres, we have already likewise spoken (see NATURE, vol. xx. p. 358).

It remains, therefore, for us only to wish the venerable author, who, for fifty years at least, has been a most energetic worker in many branches of zoology, health and strength to bring this important work to a conclusion.

Nomenclator Zoologicus. An Alphabetical list of all Generic names that have been employed by Naturalists for Recent and Fossil Animals, from the earliest Times to the close of the Year 1879. In two parts. I. Supplemental List. By Samuel H. Scudder. (Washington: Government Printing Office, 1882.)

EVERY working naturalist must be acquainted with Agassiz's "Nomenclator Zoologicus," published at Solothurn in 1846, which is, in fact, a dictionary of generic terms used in zoology. Without its valuable aid it is almost a fruitless task to endeavour to ascertain where or by what author any particular generic term has been instituted, or whether a generic term has been already used in zoology or not. Agassiz's work, in the preparation of which he was assisted by some of the best zoologists of the day, though by no means perfect in its manner of execution or free from occasional errors, answers very well for all practical purposes for genera established prior to the date of its preparation, and affords an excellent basis to work upon. It contains upwards of 32,000 entries of names of generic terms and of names of higher groups. In 1873 Graf A. v. Marschall, of Vienna, prepared and issued for the Imperial and Royal Zoological and Botanical Society of Austria, a supplementary volume, on something of the same plan. But to Marschall's "Nomenclator" no general index was attached, and, as those who have used the volume know full well, it is neither so accurate nor so complete as the work which it purports to supplement.

A new "Nomenclator Zoologicus," carrying the subject up to the present day, and correcting the errors and omissions of its two predecessors, has therefore long been a work of paramount importance to working naturalists. The question was who would undertake the ungrateful task, which was likely to confer neither fame nor fortune on the performer, and would be, above all others, long and laborious. Mr. Samuel H. Scudder of Boston, a well-known American entomologist, in response to appeals from his friends, has consented to devote his energies to the subject, and the first portion of his work is now before us.

The present part of the new Nomenclator is of a supplemental character, as is explained by Mr. Scudder in his preface, and contains "15,369 entries of genera established previous to 1880, not recorded, or erroneously given in the nomenclators of Agassiz and Marschall."

The second part, which will be of still greater consequence to naturalists will be a universal index to the first part and to the previous nomenclators and will contain altogether about 80,000 references. We shall thus shortly have, it is to be hoped, a most useful general work upon this important though technical subject brought up nearly to the present date.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

"Weather Forecasts"

HAD the Bishop of Carlisle, in his letter in NATURE (vol. xxvii. p. 4), instead of extracting from the *Times* a description of some results of the storm of October 24 last, quoted the statements as to the passage of this storm, issued in the reports of the Meteorological Office on October 24 and 25, his query concerning the failure of the weather forecasts would scarcely have needed reply.

A system of six pickets is established on our extreme western coasts, along a line which may be roughly regarded as describing the third of a circle, from Stornoway in the north-west, to Brest in the south-west. The enemy whose movements these outposts are to watch, pours in upon us a series of attacks in the form of cyclonic disturbances, by which the weather experienced in our islands is affected on 63 per cent. of our days. These circulations vary indefinitely in intensity. This element, and also their size, figure, direction, and velocity of propagation, are in great measure dependent on the distributions of atmospheric pressures and temperatures over a larger area than that occupied by our network of telegraphic stations. It will be enough to mention here that the velocity of advance of the cyclonic centres, as also of the front arcs of those exterior isobars which form closed curves, varies from zero to about 70 English miles per hour. In stormy periods like the present, the number and variability of the cyclonic circulations which attack us is extremely great, more than one per diem passing over some part of the British Isles. Now let it be remembered that our pickets sleep through the night, or that however wakeful they may be, they have, during the night hours, no means of communication with their commanding officers. How often a phalanx of the enemy will pass these outposts so as to occupy a position fairly within our area at 8 a.m., no instrumental indications having been given at 6 p.m. of the previous day—this, if treated as a question of probabilities, may be left to the Bishop of Carlisle. It is certainly obvious that such an advance, instead of being "very strange," *must* at times occur, if there be no miraculous interference in behalf of the Meteorological Office. At 8 a.m. on October 24, the centre of the disturbance referred to lay over Dorset, and was then moving to north-east at the rate of thirty-five miles per hour. Supposing the direction and velocity to have been uniform, the position occupied by the centre at 6 p.m. on the 23rd would have been about 180 miles north of Cape Finisterre, and, supposing the extent of the storm to have been also uniform, our outposts at that hour could have received no instrumental indication of the storm's progress, of a character distinct enough to justify the Meteorological Office in the issue of warnings. As a matter of fact the 6 p.m. observations telegraphed to the Office on the 23rd did show, as I think, no indications whatever of the existence of the storm.

It is obvious that the extreme velocity of the propagation of some of our severest storms is the element that especially renders it *possible* "that a storm of the first magnitude" may "come upon us unawares." As a matter of fact, the velocity of propagation on October 24 was considerably above the average. But if we refer to the charts for March 12 and 13, 1876, we find, at 8 a.m. on the former morning, a cyclone-centre occupying the precise position of that of the 24th ult., and that this disturbance moved to east-north-east with a mean velocity of 62·5 miles per hour.

There is a further risk, against which our system of telegraphy cannot protect us, viz., that of a storm centre being primarily developed within our area of observation during the hours when there is no telegraphic communication, and storms in their first stage of development are often the most dangerously rapid and intense. The telegraphic observations transmitted at 6 p.m. on October 23 and at 8 a.m. on October 24, afford no materials for deciding whether this may not have been the case in the instance under consideration, although this question can be decided from data since received. On the whole, to the minds of many students of the subject it will appear rather "strange" that the Office, *with the materials at its disposal*, does not more often fail to furnish satisfactory warnings of the more serious of our gales. It is easy to say, in view of occasional failures, "the system itself must be at fault:" it is still easier to reply, "better it!" If the country cares enough for the welfare of "fishermen and others" to do so, let it provide the necessary funds for a system of night telegrams, and if possible for a series of oceanic stations. If it does not, it must be content with things as they are.

I have been careful to speak of instrumental observations only. It is already well known that observations of the movements of the higher clouds commonly give indications of the position and advance of distant cyclonic systems. But it has hitherto been found impossible to train our observers in the difficult art of taking these observations. To the accomplishment of this task, which would greatly add to the utility of our weather-forecasts, some of us are now devoting ourselves with every prospect of success.

W. CLEMENT LEY

Ashby Parva, Lutterworth, November 3

P.S.—Since the above was sent to press a storm-centre has crossed Scotland with a velocity of about 45 miles per hour. Indications of its progress were however afforded by cloud observations at a distance of more than 800 miles in advance of the centre, the velocity of propagation being supposed uniform.—W. C. L.

The Comet

YOUR engraver has missed what I thought the most important feature in the drawings which I made of the comet on the 21st inst., viz. the *shadow* beyond the end of the tail, of the length of 3 or 4 degrees, very obviously darker than the surrounding space, in which it was lost, without demarcation. This was expressed in my sketch by a shade of lampblack, very slight, to avoid exaggeration, and perhaps just sufficient to escape the engraver's notice. The comet, as seen this morning, is diminished much in size, and still more in brightness, and the present moonlight much impairs its beauty and distinctness.

C. J. B. WILLIAMS

Villa du Rocher, Cannes, France, October 30

NOTICING Major J. Herschel's remark in NATURE, vol. xxvii. p. 5, as to the difficulty he experienced in London of observing the comet, apparently owing to the moonlight, I may state that on the morning of the same Sunday to which he refers, I saw the comet very plainly when at Rothsay, Isle of Bute, Scotland. The time was between 5 and 6 a.m., and therefore before sunrise. The moon was brilliant, and the whole sky wonderfully clear, and but few stars noticeable, on account of the moonlight, nevertheless, the comet showed well, extending about 20° across the sky due south, magnetic; the nucleus was well defined, and about as bright as the stars then visible. The tail was straight, spreading outwards to the extremity. No glass was used in the observation recorded.

W. J. MILLER

Glasgow, November 3

IT might be interesting to some of the readers of your paper to know that this morning, at 5 a.m., Mr. Manning, the agent here for Messrs. F. and A. Swanzy, merchants, and myself, saw a very fine comet bearing south-east, and the tail of which was as long as my first finger, from tip to last joint; its head, bearing a little to the east, was pointing into the sea, and was about the height from the sea of my four fingers held at arm's length; it was very brilliant, and we seem to have seen it to great advantage. Unfortunately we had only a field glass to view it through, and being also without instruments, were unable to take its proper altitude or bearings. We were standing on the verandah of the house at the time, which is on the beach, and about forty feet above the level of the sea.

We should be glad to know if the comet has been seen further north by anyone else. Quitta is situated 5° N. latitude, and 1° E. longitude.

WALTER HIGGINSON
B. MANNING

Quitta, West Coast Africa, September 25

Two Kinds of Stamens with Different Functions in the same Flower

IT may be worth mentioning that cases strongly analogous to those described in NATURE (vol. xxiv. p. 307, and vol. xxvi. p. 386, are also to be observed among the Monocotyledons in the family of Commelynaceæ, and that these cases offer some graduations.

In *Tradescantia virginica*, L., the flowers, as is generally known, are turned upwards and quite regular, the leafy organs of each whorl (3 sepals, 3 petals, 3 outer, 3 inner stamens, 3 united carpels) being alike and equal in size. As Delpino has clearly shown (*Ulteriori osservazioni*, parte ii. fascic. 2, p. 297) these flowers are adapted to Apidæ, which in order to collect pollen take hold of the articulated hairs of the filaments. In some other species here to be considered the adaptation to pollen-collecting bees has remained, but the flowers have turned laterally, and thus not only has their form become irregular (bi-laterally symmetrical or zygomorphous), but also the function of the stamens has gradually changed.

In *Tinnantia undata*, Schlecht. (Fig. 1), sepals and petals are still almost unaltered in form and size, only stamens and pistil have become markedly irregular. The broad roundish petals, which are light purple, spread in a perpendicular plane. The 3 upper stamens, with shorter filaments projecting from the middle of the flower, are highly conspicuous by a diverging tuft of bright yellow articulated hairs, which on the last third of the light-purple filaments surround the golden yellow anthers like a cone of golden rays. At the tips of these filaments golden yellow pollen-grains are presented by the whole front side of the three upper anthers.

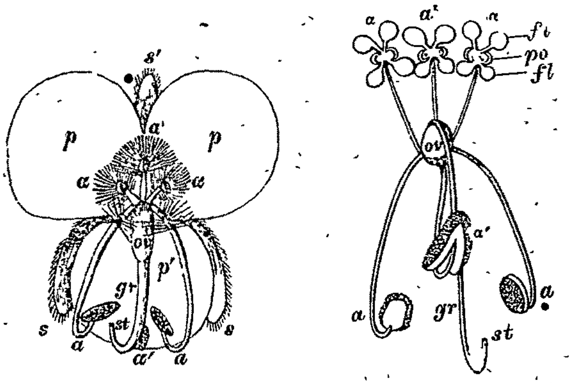

FIG 1. FIG. 2.

FIG. 1.—Front view of the flower of *Tinnantia undata*, Schlecht. FIG 2.—Front view of the andrœceum and gynæcium of *Commelyna cælestis*, Willd. *s*, *s*, *s'*, sepals, *p*, *p*, *p'* petals, *a*, *a*, *a'*, outer whorl of anthers; *a*, *a*, *a'*, inner whorl of anthers, or ovary; *gr*, style ("Griffel"); *st*, stigma.

The three lower stamens are much longer, directed obliquely downwards and forwards, with only their tips bending upwards, a little overtopped by the pistil, which has the same direction and incurvation. These parts, like the same parts in the described Melastomaceæ, will hardly be perceived by an advancing insect, "owing to their projection against the broad-petalled corolla of the same colour in the background," for not only the style and the filaments, but also the hairs on the base on the two lateral lower filaments are of the same purple colour as the petals, and even the bluish lower anthers with their yellowish pollen are but feebly conspicuous. Any one of the Apidæ or Syrphidæ of suitable size, however, when making for the upper yellow stamens in order to collect their pollen (I have only once observed the honey-bee doing so), will involuntarily repose on the projecting parts, and at first bring the stigma and then the two lateral of the lower anthers into contact with the under-side of its abdomen, and thus regularly effect cross fertilisation.

Here, then, as in Heeria, &c., the anthers have differentiated into upper ones, which attract insects and afford food to them, and lower ones which attach their pollen to the visitors, and cause it to be transported by them to the stigma of the next visited flowers. Also differentiation in the pollen of the two kinds of anthers in our Tinnantia has begun to take place, but contrary to Melastoma, the pollen-grains of the short stamens here are smaller than those of the longer ones. I measured numerous pollen-grains of two individuals in a moistened state (where they are of elliptical form), and found in the one stem the pollen-grains of the short stamens (in 1-1000 m.m.) 62-74 long, 31-38 broad, those of the longer ones 68-94 long, 38-44 broad; in the other stem, those of the short stamens 53-69 long, 28-37 broad; those of the longer ones 59-78 long, 31-40 broad. Both kinds of pollen proved to be quite fertile.

Commelyna coelestis, Willd. (Fig. 2) possesses in general the same contrivances for cross-fertilisation, but has gone a step further in differentiation. Its upper sepal is plainly smaller, its lower petal plainly larger than the two other ones; its upper anthers (*a*, *a' a*) have differentiated in themselves; two small lateral portions of each of them (*po*) produce a little pollen and four cross-like diverging flaps (*fl*), which are much larger, attract insects by their bright yellow colour strikingly contrasting with the azure corolla, and perhaps at the same time serve as food to the visitors. The articulated hairs of the filaments thus having lost not only their original function (which they have in all stamens of Tradescantia) as supports for the feet of pollen-collecting bees, but also their secondary function (which they have in the upper stamens of Tinnantia) of attracting insects, have disappeared altogether. The middlemost of the lower anthers, which in Tinnantia is nearly useless from its position behind the style here, has erected and become much larger than the two lateral ones, so as to be eminently useful.

The pollen-production of the upper anthers appears to be vanishing, not only from the diminution of the quantity of produced pollen, but also from the great variability of the size of the pollen grains. For whilst the pollen grains of the two lateral lower anthers only differ in length from 75 to 90, in breadth from 45 to 68, and those of the middlemost lower anther in length from 56 to 82, in breadth from 37 to 56, those of the three upper anthers fluctuate from 50 to 87 length, and from 31 to 56 breadth.

In *Commelyna communis*, differentiation has gone still further; the upper sepal and the lower petal are relatively very small; the upper filaments, like the upper petals, are blue-coloured; the lower filaments, like the pistil and the lower petal, are colourless. The upper anthers, as far as I have seen (without microscope) produce no more pollen

The examination of other species and genera of Commelynaceæ probably would show a longer scale of gradations.

Lippstadt, October 25 HERMANN MÜLLER

A Curious Halo

THERE appeared in NATURE, vol. xxvi. pp. 268, 293, two articles headed "A Curious Halo," which reminded me of an analogous and still more curious phenomenon occurring sometimes here in China, during the hot season. I beg to hand you a few lines on that subject, from the *Monthly Bulletin* of the Zi-ka-wei Observatory for August, 1877:—

"A phenomenon to which I wish to call the attention of meteorologists was observed many times during that month (August), as also in July. It does not seem to take place in Europe, and I am inclined to think that it cannot occur except with an atmosphere over-charged with aqueous vapour, as it is the case with us in July and August. In the evening, just after sunset, or in the morning even long before sunrise, no matter what the direction of the wind and the barometric pressure may be, provided the day or night were very warm, *bands* of a tint varying from the faintest to the deepest *blue* are seen to appear upon the whitish or roseate vault of heaven. They usually are first seen in the east at evening and in the west at morning time, seemingly radiating from a common centre diametrically opposite the sun's position. At other times they emerge from the very position of the sun, or from both points at once, the interval being either free from bands or completely encircled by them.

"Last year (1876), on the morning of September 4, I enjoyed a most interesting sight. It was about 5 a.m., the moon, then on her nineteenth day, was above the western horizon, and just being partially eclipsed; now from her bright disc, as from a radiating centre, shot out a number of those bands or blue beams; they traversed the whole expanse of the sky, and seemed to converge towards a point whose situation in the east below the horizon corresponded with that of the moon in the west above the horizon.

"These bands or shoots are more or less numerous, bright, and persistent; some have been observed in the evening, forty-five minutes after sunset, and in September, 1876, I saw them appear with the first break of day. They are evidently movable in the sky, and there is no doubt that they are due to cumuli floating about the horizon, below or above, through which the light of the sun is sifted and split; they are, in fact, nothing else than the shadows of the clouds in the faint white or rosy tint of the twilight. According as the clouds before the sun are more or less compact or loose, the bands may be blue, white, or red. More than once also have I seen the sky half white and half blue, the separation of the two colours being plainly perceivable, and Venus shining brilliantly in the blue sky close to that limit, whilst it would probably have been almost invisible through the milky sky hard by."

Any one who gazes for the first time at this beautiful phenomenon cannot help wondering and acknowledging it to be greatly different from anything to be seen elsewhere. The celebrated Jesuit, Father Bouvet, an old missionary to China, witnessed the phenomenon when on his way from China to Europe as envoy of the great Emperor Kang-hi, in the year 1693; the relation of the voyage (du Halde, vol. i., 1755) gives the following account of his observations:—

"25 Juillet, 1693.—Ce jour-là, environ un quart d'heure avant le lever du soleil, je vis dans le ciel un phénomène que je n'ai jamais vu et dont je n'ai point oui parler en France, quoiqu'il soit fort ordinaire en Orient, surtout à Siam et à la Chine; car je l'ai observé distinctement plus de vingt fois, tantôt le matin, tantôt le soir, dans chacun de ces deux Royaumes, sur mer et sur terre, et méme à Péking.

"Ce phénomène n'est autre chose que certains demi-cercles d'ombre et de lumière que paraissent se terminer et s'unir dans deux points opposés du Ciel, savoir d'un côté dans le centre du Soleil, et de l'autre dans le point qui est diamétralement opposé à celui-là. Comme ces demi-cercles sont tous terminés en pointe, tant en Orient qu'en Occident, c'est-à-dire vers les points opposés de leur réunion et qu'ils vont en s'élargissant uniformément vers le milieu du Ciel à mesure qu'ils s'éloignent de l'horizon, ils ne ressemblent pas mal, pour leur figure aux *Maisons Célestes*, de la manière dont on les trace sur les Globes, à cela près seulement que ces Zônes d'ombre et de lumière sont ordinairement fort inégales pour la largeur et qu'il arrive souvent qu'il y a de l'interruption entr'elles, surtout lorsque le phénomène n'est pas bien formé.

"Toutes les fois que je l'ai observé, et je l'ai vu quatre fois différentes dans ce voyage en moins de quinze jours, j'ai toujours remarqué que le temps était extrêmement chaud, le ciel chargé de vapeurs, avec une disposition au tonnerre et qu'un gros nuage épais entr'ouvert était vis-à-vis du Soleil. Ce phénomène semble pour la figure *fort différent de ces longues traces d'ombre et de lumière* qu'on voit souvent le soir et le matin dans le ciel aussi bien en Europe qu'ailleurs et auxquelles leur figure pyramidale a fait donner le nom de *verges*. Si l'on demande pour quelle raison ce phénomène paraît plutôt en Asie qu'en Europe et en été que dans les autres saisons, il me semble qu'on pourrait en attribuer la cause à la nature des terres de l'Asie, qui étant pour la plupart beaucoup plus chargées de *nitre* que celles d'Europe, remplissent l'atmosphère, surtout en été, et lorsque le soleil a plus de force pour les élever, d'exhalaisons nitreuses, lesquelles étant répandues également dans l'air, les rendent plus propres à réfléchir la lumière et par conséquent à former le météore."

The phenomenon described by the old Jesuit astronomer is undoubtedly the same I have witnessed hundreds of times at Zi-ka-Wei. He evidently considers it as different from any hitherto observed atmospheric phenomenon; but his explanation is tainted with the false science of his time. It is quite certain that the phenomenon is due to the atmospheric vapour, but I am rather at a loss to give a more satisfactory explanation. The dispersion of the direct rays of the sun *into* the minute drops resulting from a partial but wide-spreading condensation of the aqueous vapour in the upper strata of the air, might account for the milky or roseate appearance of the sky at morning and evening time. Besides, the interposition of a light cloud in the way of the sun's rays does not impair the transparency of the drops, and the blue sky may be visible. Now, in the morning and evening the rays of the sun are almost parallel with the horizon; they traverse the whole expanse of the sky, and their apparent convergence on the both sides is only due to the same optical illusion which shows us the two rails of a railway track or the walls of a tunnel as converging.

Let this explanation be worth what it may, the fact in itself is interesting, and I would beg you, Sir, to notice it in NATURE, dealing, however, with this long communication as you may deem proper.

MARC DECHEVRENS

Zi-ka-Wei Observatory, near Shanghai, (China), August 28

Habits of Scypho-Medusæ

THE communications to NATURE of Mr. Archer (vol. xxiv. p. 307), and of Mr. Alexander Agassiz (vol. xxiv. p. 509), on the subject of Medusæ lying upon the bottom with their tentacles upward, lead me to forward some observations which I made on a similar habit of Medusæ in the island of Simbo, one of the Solomon Islands. The Medusa in question frequents a small mangrove swamp, which lies inclosed in the low point that forms the south shore of the anchorage. Numbers of these animals of a large and dirty-white colour were lying lazily on the mud at the bottom of the water, which varied in depth from one to three feet, with their umbrellas lowermost, and a magnificent mass of arborescent tentacles well displayed. When one of them was disturbed and turned over with a stick, it immediately began to contract the umbrella, until, after swimming a short distance, it resumed its former position on the bottom, of tentacles upward. The dark mud which formed the bottom of the swamp was composed of decayed vegetable matter—low confervoid growths, and a few infusoria and living diatoms. But I invariably observed, after raising several of these Medusæ from the bottom, that a layer of white sand covered over the place where each had lain, its light colour forming a marked contrast with the dark mud around. The form of these patches of sand corresponded with the outline of the animal; but when the Medusa lay in its usual position, the umbrella completely concealed them from view. The sand was sometimes fine, at other times coarse, and was derived from the coral and trachytic rocks in the vicinity, with occasionally fragments of shells intermingled. The sand did not adhere to the surface of the umbrella.

The Medusæ measured generally some eight or nine inches across the umbrella, and appeared to belong to the Rhizostomidæ.

H. B. GUPPY

H.M.S. *Lark*, St. Christoval, Solomon Islands, June 29

Prof. Owen on Primitive Man

IN the first number of *Longman's Magazine* Prof. Owen criticises an article of mine on Primitive Man, in the *Fortnightly Review*. In doing so, he quotes some words from my article, which are there given as a quotation from Prof. Schaafhausen. He proceeds to make them the text of his paper, as though the opinions expressed in them were my own. On the question at issue—the Neanderthal skull—I am not competent to form any personal opinion; I merely abstracted the opinions of Rolleston and Schaafhausen. Prof. Owen would hardly have spoken in the same lofty magisterial tone had he attributed those opinions to their real authors, whose reputation can take care of itself. The respect I feel for Prof. Owen's work makes me deeply regret the necessity for this explanation; but I cannot allow him to quote as mine words which I placed between inverted commas, attributing them at the same time to their real author.

GRANT ALLEN

Magnetic Arrangement of Clouds

THERE is a curious arrangement of clouds which, though seen myself for the first time this year, may doubtless have been observed by others, though I have never seen it referred to anywhere. Light clouds of the cirrus formation apparently at great elevations range themselves round two poles—one about in the direction of the magnetic north pole, and the other in that of the south. The space between the two poles is filled more or less completely by wispy cirri. The exact point where the various threads or wisps should form themselves into a pole I have never been able to clearly see, owing to the dense stratum of vapour which even on the clearest day accumulates at the horizon. On Sunday, October 29, the arrangement above noticed was remarkably distinct in the afternoon.

C. H. ROMANES

Worthing

The Umdhlebe Tree of Zululand

THE word "umdhlebe" does not, I think, appear in Döhne's "Zulu-Kaffir Dictionary." I presume it to be a derivative from the root *hlaba*, which Dohne interprets as denoting, among other things, the giving of pain. Some native tales of the tree will be found in part iv. of Bishop Callaway's "Religious System of the Amazulu," in which it is asserted that "there are several kinds, not one kind only of umdhlebe; some are small." I should be disposed to think the kernel of fact will be found to lie in native observation of the deleterious properties and weird aspects of certain *Euphorbiaceæ*. H. M. C.

Charlton, November 4

The Weather

THE past month has probably been one of the wettest on record. I have registered here 5·14 inches of rain during the month; only on seven days out of the thirty-one has the gauge shown less than 0·1; and on three days out of the seven rain has been recorded. J. M. FOUNTAIN

Hillingdon, Uxbridge, November 2

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE

THERE are several methods by which galvanometers may be graduated so as to measure currents and potentials in absolute measure, but they all involve, directly or indirectly, a comparison of the indications of the instrument to be graduated with those of a standard instrument, of which the constants are fully known for the place at which the comparison is made. There are various forms of such standard instruments, as, for example, the tangent galvanometer which Joule made, consisting of a single coil of large radius, and a small needle hung at its centre, or the Helmholtz modification of the same instrument with two large equal coils placed side by side at a distance apart equal to the radius of either; or some form of "dynamometer," or instrument in which the needle of the galvanometer is replaced by a movable coil, in which the whole or a known portion of the current in the fixed coil flows. The measurement consists essentially in determining the couple which must be exerted by the earth's magnetic force on the needle or suspended coil, in order to equilibrate that exerted by the current. But the former depends on the value, usually denoted by *H*, of the horizontal component of the earth's magnetic force, and it is necessary therefore, except when some such method as that of Kohlrausch, described below, is used, to know the value of that quantity in absolute units.

The value of *H* may be determined in various ways, and I shall here content myself with describing one or two of the most convenient in practice. The easiest method is by finding (1) the angle through which the needle of a magnetometer is deflected by a magnet placed in a given position at a given distance, (2) the period of vibration of the magnet when suspended horizontally in the earth's field, so as to be free to turn round a vertical axis. The first operation gives an equation involving the ratio of the magnetic moment of the magnet to the horizontal component *H* of the terrestrial magnetic force, the second an equation involving the product of the same two quantities. I shall describe this method somewhat in detail.

A very convenient form of magnetometer is that devised by Mr. J. T. Bottomley, and made by hanging within a closed chamber, by a silk fibre from 6 to 10 cms. long, one of the little mirrors with attached magnets used in Thomson's reflecting galvanometers. The fibre is carefully attached to the back of the mirror, so that the magnets hang horizontally and the front of the mirror is vertical. The closed chamber for the fibre and mirror is very readily made by cutting a narrow groove to within a short distance of each end, along a piece of mahogany about 10 cms. long. This groove is widened at one end to a circular space a little greater in diameter than the diameter of the mirror. The piece of wood is then fixed with that end down in a horizontal base-piece of wood furnished with three levelling-screws. The groove is thus placed vertical; and the fibre carrying the mirror is suspended within it by passing the free end of the fibre through a small hole at the upper end of the groove, adjusting the length so that the mirror hangs within the circular space at the bottom, and fixing the fibre at the top with wax. When this has been done, the chamber is closed by covering the face of the piece of wood with a strip of glass, which may be either kept in its place by cement, or by proper fastenings which hold it tightly against the wood. By making the distance between the back and front of the circular space small, and its diameter very little greater than that of the mirror, the instrument can be made very nearly "dead beat," that is to say, the needle when deflected through any angle comes to rest at once, almost without oscillation about its position of equilibrium. A magnetometer can be thus constructed at a trifling cost, and it is much more accurate and convenient than the magnetometers furnished with long magnets frequently used for the determination of *H*; and as the poles of the needle may always in practice be taken at the centre of the mirror, the calculations of results are much simplified.

The instrument is set up with its glass front in the magnetic meridian, and levelled so that the mirror hangs freely inside its chamber. The foot of one of the levelling screws should rest in a small conical hollow cut in the table or platform, of another in a V-groove the axis of which is in line with the hollow, and the third on the plane surface of the table or platform. When thus set up the instrument is perfectly steady, and if disturbed can in an instant be replaced in exactly the same position. A beam of light passing through a slit, in which a thin vertical cross-wire is fixed, from a lamp placed in front of the magnetometer is reflected, as in Thomson's reflecting galvanometer, from the mirror to a scale attached to a lamp-stand, and facing the mirror. The lamp and scale are moved nearer to or farther from the mirror, until the position at which the image of the cross-wire of the slit is most distinct is obtained. It is convenient to make the horizontal distance of the mirror from the scale for this position if possible one metre. The lamp-stand should also have three levelling screws, for which the arrangement of conical hollow V groove and plane should be adopted. The scale should be straight, and placed with its length in the magnetic north and south line, and the lamp should be so placed that the incident and reflected rays of light are in an east and west vertical plane, and that the spot of light falls near the middle of the scale. To avoid errors due to variations of length in the scale, it should be glued to the wooden backing which carries it, not simply fastened with drawing pins as is often the case.

The magnetometer having been thus set up, four or five magnets, each about 10 cms. long and ·1 cm. thick, and tempered glass-hard, are made from steel wire. This is best done as follows. From ten to twenty pieces of steel wire, each perfectly straight and having its ends carefully filed so that they are at right angles to its length, are prepared. These are tied tightly into a bundle with a binding of iron wire and heated to redness in a bright fire. The bundle is then quickly removed from the fire, and plunged with its length vertical into cold water. The wires are thus tempered glass-hard without being seriously warped. They are then magnetised to saturation in a helix by a strong current of electricity. A horizontal magnetic east and west line passing through the mirror is now laid down on a convenient platform (made of wood put together without iron and extending on both sides of the magnetometer) by drawing a line through that point at right angles to the direction in which a long thin magnet hung by a single silk fibre there places itself. One of these magnets is placed, as shown in Fig. 1, with its length in that line, and at such a distance that a convenient deflection of the needle is produced. This deflection is noted and the deflecting magnet turned end for end, and the deflection again noted. Make in the same way a pair of observations with the magnet at the same distance on the opposite side of the magnetometer, and take the mean of all the observations. These deflections from zero ought to be as nearly as may be the same, and if the magnet is properly placed, they will exactly agree; but the effect of a slight error in placing the magnet will be nearly eliminated by taking the mean of all the deflections as the deflection of the magnet for that position. The exact distance in cms. of the centre of the deflecting magnet from the mirror is also noted. The same operation is gone through for each of the magnets, which are carefully kept apart from one another during the experiments. The results of each of these experiments give an equation involving the ratio of the magnetic moment of the magnet to the value of H. Thus if m denote the magnetic moment of the magnet, m' the magnetic moment of the needle, $2r$ the distance of the centre of the magnet from the centre of the needle, $2l$ the distance between the poles of the magnet which, for a uniformly magnetised magnet of the dimensions stated above is nearly enough equal to its length, and $2l'$ the distance between the poles of the needle, r, l, and l' being all measured in cms., we have for the repulsive force (denoted by F in Fig. 1) exerted on the blue[1] pole of the needle by the blue pole of the magnet, supposed nearest to the needle, as in Fig. 1, the value of $\frac{m}{2l} \cdot \frac{m'}{2l'} \cdot \frac{1}{(r-l)^2}$, since the value of l' is small compared with l. Similarly for the attraction exerted on the same pole of the needle by the red pole of the magnet, we have the expression $\frac{m}{2l} \cdot \frac{m'}{2l'} \cdot \frac{1}{(r+l)^2}$. Hence the total repulsive force exerted by the magnet on the blue pole of the needle is

$$\frac{m\,m'}{4\,l\,l'}\left\{\frac{1}{(r-l)^2}-\frac{1}{(r+l)^2}\right\} \text{ or } m\,\frac{m'}{l'}\;\frac{r}{(r^2-l^2)^2}.$$

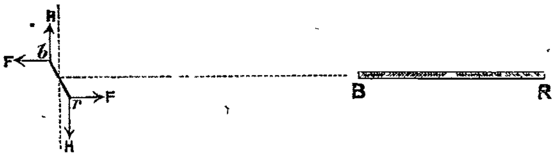

FIG. 1.

Proceeding in a precisely similar manner, we find that the magnet m exerts an attractive force equal to $m\frac{m'}{l'} \cdot \frac{r}{(r^2-l^2)^2}$ on the red pole of the magnet. The needle is therefore acted on by a couple which tends to turn it round the suspending fibre as an axis, and the amount of this couple, when the angle of deflection is θ, is plainly equal to $m\,m'\,\frac{2r}{(r^2-l)^2}\cos\theta$. But for equilibrium this couple must be balanced by $m'H\sin\theta$; hence we have the equation :—

$$\frac{m}{H}=\frac{(r^2-l^2)^2}{2r}\cdot\tan\theta \quad . \quad . \quad . \quad . \quad (1)$$

The angle θ is to be measured thus :—The number of divisions of the scale which measures the deflection divided by the number of such divisions in the distance of the scale from the mirror, is, if the scale is placed as described above in the magnetic north and south line, equal to $\tan 2\theta$.

Instead of in the east and west horizontal line through the centre of the needle, the magnet may be placed, as represented in Fig. 2, with its length east and west, and its centre in the horizontal north and south line through the centre of the needle. If we take m, m', l, l', and r to have the same meaning as before, we have for the distance of either pole of the magnet from the needle, the expression $\sqrt{r^2+l^2}$. Let us consider the force acting on one pole, say the red pole of the needle. The red pole of the magnet exerts on it a repulsive force, and the blue pole an attractive force. Each of these forces has the value $\frac{m}{2l} \cdot \frac{m'}{2l'} \cdot \frac{1}{r^2+l^2}$. But the diagram shows that they are equivalent to a single force, F, in a line parallel to the magnet, tending to pull the red pole of the needle towards the left. The magnitude of this resultant force is plainly $2\,\frac{m}{2l} \cdot \frac{m'}{2l'} \cdot \frac{l}{(r^2+l^2)^{\frac{3}{2}}}$ or $\frac{m\,m'}{2\,l'(r^2+l^2)^{\frac{3}{2}}}$. In the same way it can be shown that the action of the magnet on the red pole of the needle is a force of the same amount tending to pull the blue pole of the needle towards the right. The needle is, therefore, subject to no force tending to produce motion of translation, but simply to a "couple" tending to produce rotation. The magnitude of this couple when the needle has been turned through an angle θ, is $\frac{m\,m'}{2\,l'}\;\frac{2\,l'\cos\theta}{(r^2+l^2)^{\frac{3}{2}}}$, or $\frac{m\,m'}{(r^2+l^2)^{\frac{3}{2}}}\cos\theta$ If there be equilibrium for the deflection θ, this couple must be balanced by that due to the earth's horizontal force, which, as before, has the value $m'H\sin\theta$. Hence equating these two couples we have—

$$\frac{m}{H}=(r^2+l^2)^{\frac{3}{2}}\tan\theta. \quad . \quad . \quad . \quad . \quad (2)$$

Still another position of the deflecting magnet relatively to the needle may be found a convenient one to adopt. The magnet may be placed still in the east and west line, but with its centre vertically above the centre of the needle. The couple in this case also is given by the formula just found, in which the symbols have the same meaning as before.

The greatest care should be taken in all these experiments, as well as in those which follow, to make sure that there is no movable iron in the vicinity, and the instruments and magnets should be kept at a distance from any iron nails or bolts there may be in the tables on which they are placed.

We come now to the second operation, the determination of the period of oscillation of the deflecting magnet when under the influence of the earth's horizontal force alone. The magnet is hung in a horizontal position in a double loop formed at the lower end of a single fibre of unspun silk, attached by its upper end to the roof of a closed chamber. A box about 30 cms. high and 15 cms. wide, having one pair of opposite sides, the bottom and the roof made of wood, and the remaining two sides made of plates of glass, one of which can be slided out to give access to the inside of the chamber, answers very well. The fibre may be attached at the top to a horizontal wire which can be turned round from the outside so as to wind up or let down the fibre when necessary. The suspension-fibre is so placed that two vertical scratches, made along the glass sides of the box, are in the same plane with the magnet when the magnet is placed in its sling, and the box is turned round until the magnet is at right angles to the glass sides. A paper screen with a small hole in it is then set up at a little distance in such a position that the hole is in line with the magnet, and therefore in the same plane as the scratches. The magnetometer should be removed from its stand and this box and suspended needle put in its place. If the magnet be now deflected from its position of equilibrium and then allowed to vibrate round a vertical axis, it will be seen through the small hole to pass and re-pass the nearer scratch, and an observer keeping his eye in the same plane as the scratches can easily tell without sensible error the instant when the magnet passes through the position of equilibrium. Or, a line may be drawn across the bottom of the box so as to join the two scratches, and the observer keeping his eye above the magnet and in the plane of the scratches notes the instant when the magnet going in the proper direction is just parallel to the horizontal line. The operator should deflect the magnet by bringing a small magnet near to it, taking care to keep the small deflecting magnet always as nearly as may be with its length in an east and west line passing through the centre of the suspended magnet. If this precaution be neglected the magnet may acquire a pendulum motion about the point of suspension, which will interfere with the vibratory motion in the horizontal plane. When the magnet has been properly deflected and left to itself, its range of motion should be allowed to diminish to about 3° on either side of the position of equilibrium before observation of its period is begun. When the amplitude has become sufficiently small, the person observing the magnet says sharply the word "Now," when the nearer pole of the magnet is seen to pass the plane of the scratches in either direction, and another observer notes the time on a watch having a seconds hand. With a good watch having a centre seconds hand moving round a dial divided into quarter-seconds, the instant of time can be determined with greater accuracy in this way than by means of any of the usual appliances for starting and stopping watches, or for registering on a dial the position of a seconds hand when a spring is pressed by the observer. The person observing the magnet again calls out "Now" when the magnet has just made ten complete to and fro vibrations, again after twenty complete vibrations, and, if the amplitude of vibration has not become two small, again after thirty; and the other observer at each instant notes the time by the watch. By a complete vibration is here meant the motion of the magnet from the instant when it passes through the position of equilibrium in either direction, until it next passes through the position of equilibrium going in the same direction. The observers then change places and repeat the same operations. In this way a very near approach to the true period is obtained by taking the mean of the results of a sufficient number of observations, and from this the value of the product of m and H can be calculated.

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[1] The convention according to which magnetic polarity of the same kind as that of the earth's northern regions is called blue, and magnetic polarity of the same kind as that of the earth's southern regions is called red, is here adopted. The letters B, R, b, r in the diagrams denote blue and red

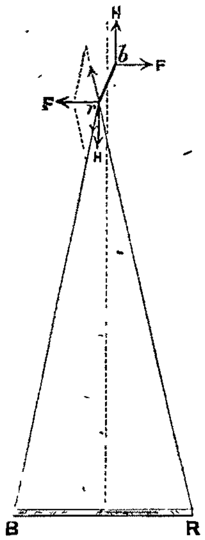


Fig. 2.

For a small angular deflection θ of the vibrating magnet from the position of the equilibrium the equation of motion is

$$\frac{d^2\theta}{dt^2} + \frac{mH}{\mu}\theta = 0,$$

where μ is the moment of inertia of the vibrating magnet round an axis through its centre at right angles to its length. The solution of this equation is

$$\theta = A \sin\left\{\sqrt{\frac{mH}{\mu}}\,t - B\right\}$$

and therefore for the period of oscillation T we have

$$T = 2\pi\sqrt{\frac{\mu}{mH}}.$$

Hence we have

$$mH = \frac{4\pi^2\mu}{T^2}$$

Now, since the thickness of the magnet is small compared with its length, if W be the mass of the magnet μ is $\frac{l^2}{3}W$, and therefore

$$mH = \frac{4\pi^2 l^2 W}{3T^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

combining this with the equation (1) already found we get for the arrangement shown in Fig. 1.

$$m^2 = \frac{2}{3} \cdot \frac{\pi^2 (r^2 - l^2)^2 W \tan\theta}{T^2 r} \quad . \quad . \quad (4)$$

and

$$H^2 = \frac{8}{3} \cdot \frac{\pi^2 l^2 r W}{T^2 (r^2 - l^2)^2 \tan\theta} \quad . \quad . \quad . \quad (5)$$

If either of the other two arrangements be chosen we have from equations (2) and (3)

$$m^2 = \frac{4}{3} \cdot \frac{\pi^2 l^2}{T^2} (r^2 + l^2)^{\frac{3}{2}} W \tan\theta \quad . \quad . \quad (6)$$

and

$$H^2 = \frac{4}{3} \cdot \frac{\pi^2 l^2 W}{(r^2 + l^2)^{\frac{3}{2}} T^2 \tan\theta} \quad . \quad . \quad . \quad (7)$$

Various corrections which are not here made are of course necessary in a very exact determination of H. The virtual length of the magnet, that is, the distance between its poles or "centres of gravity" of magnetic polarity, should be determined by experiment: and allowances should be made for the magnitude of the arc of vibration; the torsional rigidity of the suspension fibre of cocoon silk of the magnetometer in the deflection experiments, and of the suspension fibre of the magnet in the oscillation experiments; the frictional resistance of the air to the motion of the magnet; the virtual increase of inertia of the magnet due to motion of the air in the chamber; and the effect of induction in altering the moment of the magnet. The correction for an arc of oscillation of 6° is a diminution of the observed value of T of only $\frac{1}{80}$ per cent., and for an arc of 10° of $\frac{1}{20}$ per cent. Of the other corrections the last is no doubt the most important; but even its amount for a magnet of glass-hard steel, nearly saturated with magnetism, and in a field so feeble as that of the earth, must be very small.

The deflection-experiments are, as stated above, to be performed with several magnets, and when the period of oscillation of each of these has been determined, the magnetometer should be replaced on its stand, and the deflection experiments repeated, to make sure that the magnets have not changed in strength in the mean time. The length of each magnet is then to be accurately determined in centimetres, and its weight in grammes; and from these data and the results of the experiments, the values of m and of H can be found for each magnet by the formulas investigated above. Equation (5) is to be used in the calculation of H when the arrangements of magnetometer and deflecting magnet, shown in Fig. 1, is adopted, equation (7), when that shown in Fig. 2 is adopted.

The object of performing the experiments with several magnets, is to eliminate as far as possible, errors in the determination of weight and length. The mean of the values of H, found for the several magnets, is to be taken as the value of H at the place of the magnetometer. We have now to apply this value to the measurement of currents.

ANDREW GRAY

(*To be continued.*)

THE ITALIAN EXPLORATION OF THE MEDITERRANEAN

I BELIEVE it will interest the numerous readers of NATURE, especially those who have studied the important subject of the deep-sea fauna, or who are geologists, to learn that the further exploration of the Mediterranean this year, on the part of the Italian Government, has not been fruitless, although it has been short. I have just received a letter from Prof. Giglioli, of Florence, the purport of which I will, with his permission, now give :—

It seems that this summer the surveying-vessel, *Washington*, had to undertake a search (which proved unsuccessful) for some imaginary coral-banks in the shallow sea between Sicily and Africa, besides her usual hydrographical work, and that consequently very little time could be devoted to deep-sea exploration. However, Prof. Giglioli was allowed to accompany the hydrographer, Capt. Magnaghi, with the chance of taking any favourable opportunity that might occur. He thus got three deep-sea hauls: the first near Marittimo, in 718 metres, or about 389 fathoms; the second, half-way between Sicily and Sardinia (lat. 38° 38′ N., long. 10° 40′ E.), at a depth of 1583 metres, or about 857 fathoms, when a very rare and peculiar abyssal fish (*Paralepis cuvieri*), was obtained. That day (August 15) was also appropriated to hydrographical researches, and particularly to the successful trial of Capt. Magnaghi's new water-bottle, as well as to the marvellous work of his new currentometer, a most valuable discovery, by means of which the direction and force of sub-marine currents can be accurately determined at any depth. A large new trawl was used, and brought up a block of newly-formed limestone, which had been hardened with recent shells of Pteropods embedded in its mass. The third and last deep-sea dredging was made on September 1, between Tavolara in Sardinia, and Montecristo, in 904 metres, or about 490 fathoms, with indifferent results. He will send me the shells for examination. The Italian Ministry have promised him that a whole month next year will be allowed for deep-sea exploration.

J. GWYN JEFFREYS

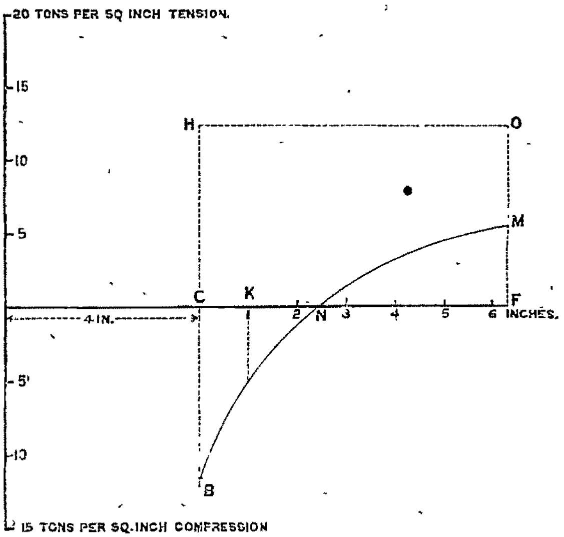
WIRE GUNS[1]

II.

IT has been necessary to dwell thus at length on the hoop method of construction in order to contrast it with the wire system, which we now proceed to describe.

A wire gun consists first of an internal tube, the function of which is to contain the rifling, and to transmit the internal pressure to the wire which is coiled upon it, and which gives the strength. This tube no doubt has a certain amount of strength of its own, but this is not its real function. The gun may be so designed and constructed that the tube is never in a state of tension. It may therefore be made, and possibly with advantage, of hard cast iron. In the 3 inch breech-loading gun made by the writer in 1860, the tube was of cast-iron $\frac{1}{2}$ inch thick, and this gun has been severely tested without injury. Hard cast-iron possesses many advantages, and amongst others that of great economy as compared with the steel tubes now generally used; but whatever be the material of the tube, its principal function is to contain the rifling and transmit the strain to the wires coiled around it.

Upon the inner tube is wound steel wire, square or rectangular in section. The tube is mounted in a machine similar to a lathe, and the wire is coiled upon one or more cylindrical drums, which are fixed horizontally on axes parallel to the tube and provided with proper apparatus for regulating the feed and tension. The tensions having been first calculated, the coiling begins from the breech-end where the end of the wire is made fast. When the muzzle end is reached the wire is coiled back again to the breech, and this process goes on till the whole of the coils are in place. The end of the wire is then made fast, and the gun, so far as strength to resist a bursting strain, which is called circumferential strength, is concerned, is complete.



E

Before proceeding to show how the longitudinal strength is provided for, it will be well to devote a little time to the substitution of coils of wire for the hoops above described, pointing out as we go along the superiority of the wire system. It has already been shown how important it is in the hoop system that the initial tensions of each hoop should be accurately calculated and applied. This is no less necessary with the wire coils, and it would at first appear that this must involve very intricate and tedious calculation. In the case of the gun represented in Diagram C, it was stated that the same strength which was given by 4 coils of steel, making with the tube a total thickness of $22\frac{3}{4}$ inches, might be obtained by $6\frac{3}{4}$ inches of wire, but supposing the wire to be $\frac{1}{10}$th inch square in section there would be required no less than 67 different coils and tensions, and as it is desirable to use even smaller wire for the first portion of the coils, there would probably be not less than 80 or 90 coils and the same number of tensions to be calculated. A formula has, however, been found which makes these calculations comparatively simple. In order to make this intelligible we must resort to another diagram, E, which represents the state of strain of the interior of a wire gun, or rather of the wire portion of it, on which alone we depend for circumferential strength. Assuming the wire to be very small, say $\frac{1}{16}$th of an inch square in section, the strains are represented very nearly by the curved line B N M. The coils between the inner circumference, *i.e.* the first coil, and the point N are all in compression, the maximum being at C; at N is the neutral point, when the wire is neither in compression nor tension; and from N to F all the coils are in tension, the maximum being at F.

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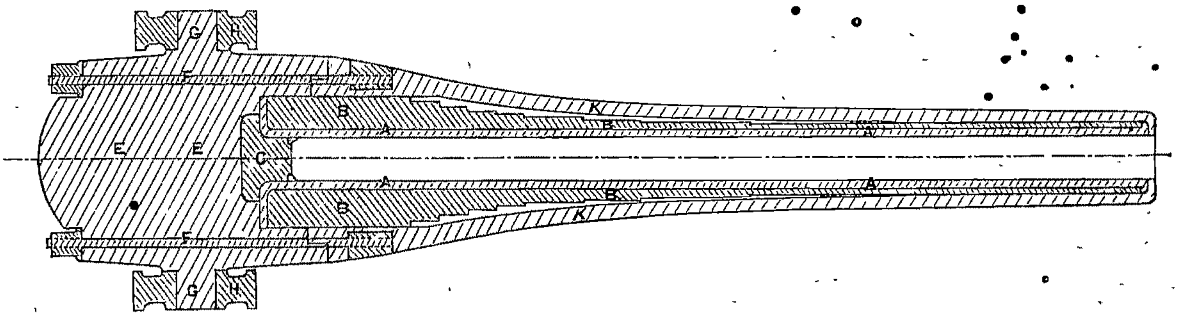
[1] Continued from p. 14

Now if we consider the case of any one coil, such as that at the position K, we see that when the gun is completed it is under considerable *compression*, but whilst the construction is proceeding, when the coil at this point is being laid on, it is laid on under *tension*, which tension is reduced by every successive additional coil until it attains the state of compression shown in the diagram of the finished structure. The question therefore to be solved is this, What is the proper tension for putting on the coil at K, so that when all the other coils are put on, it may be in the required state of compression? This problem must be solved for every individual coil. This having been done, each coil is laid on by automatic machinery with its proper tension, and the final result is that shown in the diagram.

When the full internal pressure of the explosion operates, the result is as follows:—Every coil is brought up to the same tension simultaneously and exerts the same resistance per square inch of section throughout the whole thickness of the gun as denoted by the line H O.

The ultimate strength therefore of such a gun increases in the simple ratio of the number of coils, a result not attainable by any other mode of construction, and this is the first advantage over the hoop system. The second is, that there is no fear of error through inaccurate workmanship or unequal shrinkage. The tensions of the wire coils are actually measured by the machine by which they are laid on, instead of being *inferred* from presumed accuracy of workmanship or uniform shrinking power of the material. In the next place there is no danger from latent defects. The wire is not subject to such defects as thick hoops are, and can moreover be easily tested before it is applied. Then again the process of construction is simple and expeditious, it is the substitution of accurate automatic machinery for very highly skilled labour. Beyond this it is much less costly, for although the wire itself costs a high price per ton as compared with the raw material used in the hoops of the Woolwich guns, yet when the labour and work in the latter is taken into account, it will be found that it largely exceeds that of the wire gun ton for ton, and as was before pointed out, the wire gun of equal strength can be made very much lighter.

FIG. 1.



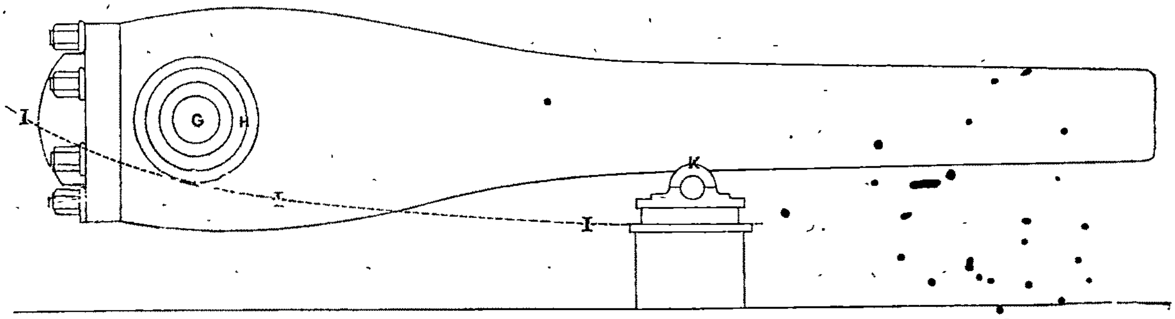


FIG. 2.

In a paper read before the Institution of Civil Engineers in 1879 the writer estimated the cost of a muzzle-loading 20 inch gun weighing 150 tons, constructed on the wire principle, at £5,041, or £33 16*s*. per ton. We believe that the price paid by Government to Sir Wm. Armstrong for the 100-ton guns produced from his firm was £16,000 each, or £160 per ton.

We now proceed to the question of longitudinal strength. In the old guns, as well as the present Woolwich guns, the Armstrong, Whitworth, and Krupp guns, the longitudinal strain between the breech and the trunnions is borne by the chase of the gun itself, that is to say, that the same material which has to resist the enormous circumferential strain has at the same time to resist a very intense longitudinal strain. Now it has been generally maintained that although this is very large in the gross, yet when it is divided by the sectional arm of the chase, it is comparatively small per square inch of section. This is a very great mistake as was pointed out several years ago by the late Sir William Palliser. The fact is, that this strain is no more uniformly divided over the sectional area of the chase than is the circumferential strain between the inner and outer circumferences.

Sir Wm. Palliser devised a method of breech construction which has since been adopted at Woolwich, by means of which the longitudinal strain is much more equally distributed, and since then the accident of a breech blowing out has been comparatively rare, and we believe has never occurred in Sir Wm. Palliser's own guns. It has always been a difficulty with many people to understand how the breech is to be secured in a wire gun. It is obvious that the coils of wire afford no longitudinal strength, and the general idea has been that it was therefore necessary to resist the whole longitudinal strain by the inner tube.

The writer has always maintained that no real difficulty exists, and that the connection between the breech and the trunnions should be by means of material quite independent of and placed outside of the coils of wire.

It was in this way that his gun made in 1860 was constructed, and we believe the same principle has been adopted by Capt. Schultz in the wire guns built under his directions by the French Government. Thus the circumferential strain is provided for by one portion of material, and the longitudinal strain by another, and it does not admit of a doubt that this is far preferable to subjecting the same material to two strains at right angles to each other at one and the same time.

Another objection has been taken to wire guns, and it is this. It is well known that guns become heated by firing, and it is thought that this heating would disturb the tensions to such an extent as to render all the calculations of strain useless. Now if this be an objection, it applies with far greater force to the system of hoop construction than to that of wire, but as there is much misconception on this point it is desirable to say a few words about it.

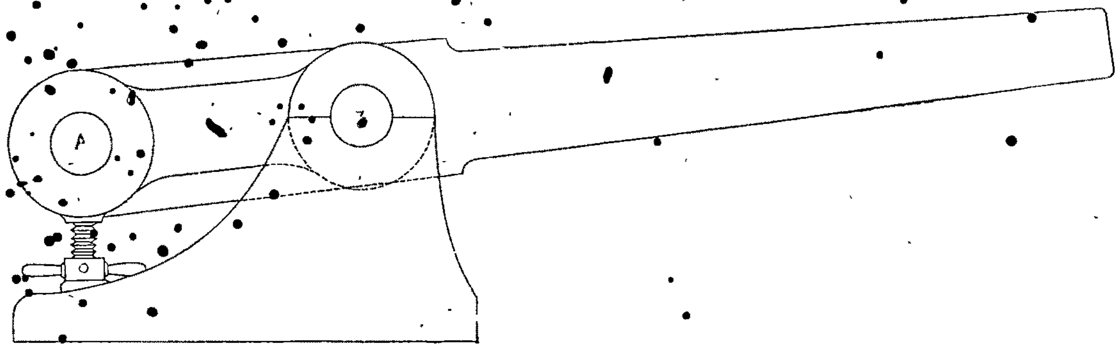
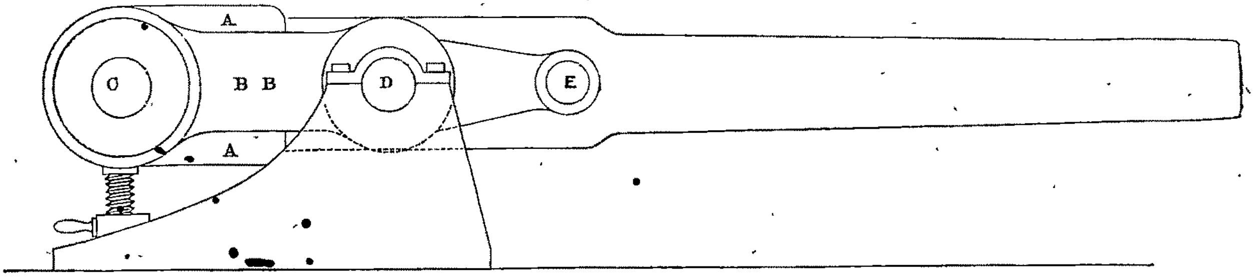


Fig. 3.

In the first place, it is a mistake to suppose that the heating of guns depends chiefly on the heat absorbed by the metal from the powder gases. Though this heat is very intense, its application is for a very small fraction of a second, and it may be shown that in this very short time only a small amount of heat can be absorbed by the surface of the gun exposed to it. It may further be shown, that during the very short time the heat is applied, it can only be transmitted by internal conduction to a very small depth into the metal of the gun. But as guns do heat by firing, how is this to be accounted for?

Fig 4.



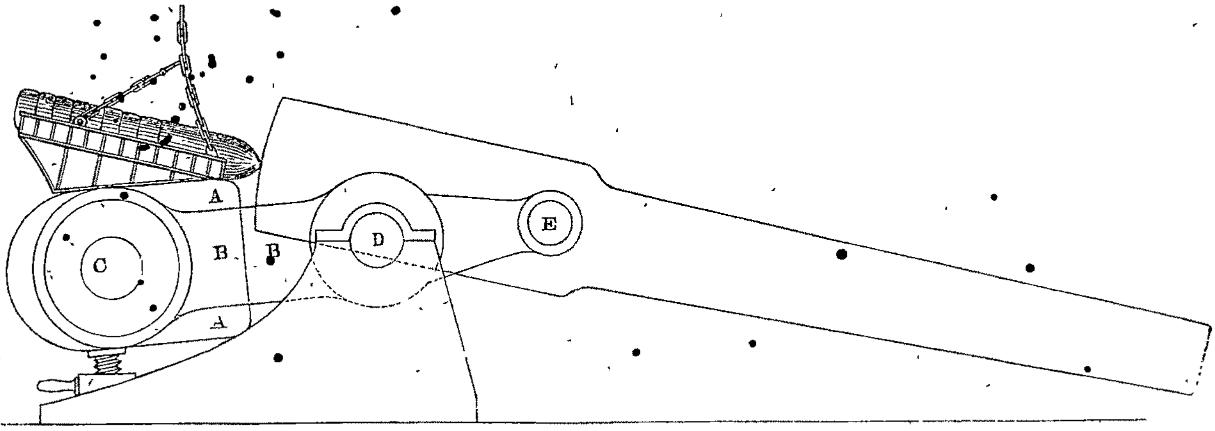


Fig. 5

The reason seems to be the following. By the explosion of the powder, a considerable amount of mechanical energy is absorbed in expanding the gun against the elastic form of the material. When the projectile leaves the gun, the internal pressure is removed, the mechanical energy is thus given back, but as it does no external work, it appears in the form of heat, which remains in the metal of the gun, until it is dissipated by convection through the surrounding air.

We are quite aware that this explanation does not agree with the views of some physicists of great reputation. For instance, in a recent discussion at the Institution of Civil Engineers, Dr. Siemens asserted that not a single unit of heat would be set up in the body of the gun by compressive action, and maintained that the whole heat produced was due to the heated products of combustion of the powder. But an experiment recorded by Hirn in his Treatise on Thermodynamics seems to support the view we have above set forth. He found that if an elastic bar of india-rubber was extended by tension it grew sensibly warmer, if then it was allowed to contract by the gradual decrease of the extending force, it cooled again to its original temperature; but if on the contrary it was let go suddenly, it did not cool, but remained at its higher temperature. In the one case the mechanical energy was given out in work done in the extending force, whilst in the other no external work was done. This is exactly what happens in the gun.

There is moreover another cause which operates in heating the body of a gun. The explosion of powder is an impact. Now in the impact of two elastic bodies one portion of the *vis viva* is expended in overcoming the elastic force of the material; another portion is converted into heat, and this portion remains in the body after the elastic force has restored it to its original form, and can only be got rid of by convection.

Thus there are two causes operating in heating a gun exclusive of the very small effect due to the heated products of combustion. Let us now examine what would be the result of this heating upon the various constructions of guns.

Take first the homogeneous gun, of which the state of strain is represented by diagram A, page 12. The strain at the inner surface of the gun during explosion is about 27 tons, whilst at the outer circumference it is only 3 tons per square inch. Now when the internal pressure is removed, the energy stored up in this strained mass is converted into heat, and we may suppose the amount of heating to be directly as the amount of energy so converted and inversely as the quantity of material heated. This being so, it follows that the inner layer of the gun would be heated nine times as much as the extreme outer layer by reason of conversion of energy, but the mass heated in each layer being in proportion to its length, and the lengths being as 4½ to 19½, or as 1 to 4·3 nearly, the rise of temperature would be as 4·3 × 9 to 1, *i.e.* thirty-nine times greater in the innermost than in the outermost layer, and it is easy to see how this inequality of temperature must cause great internal strain by expansion, and thus weaken the gun.

Let us now consider the case of the 9-inch gun, the strains of which are shown by diagrams B_1 and B_2. As regards the steel tube, the result of the explosion is to change the inner surface from a state of compression of 11 tons to a state of tension of 12 tons per square inch, and the outer layer from about 7 tons compression to about 2½ tons tension. Whilst this is going on the tube is giving out work in aid of the powder guns until it arrives at the neutral state, after which it is absorbing work; the whole tube is therefore cooling. Now let us take the outer hoop. The effect of the explosion here is to increase the initial tension of 6 tons to 17 tons at the inner, and from 2 tons to 4½ tons at the outer surface. Now when the internal pressure is removed the energy given out is expended, first in the compression of the tube, and this part of the energy gives rise to no change of temperature, but the whole of the rest of the energy represented by 11 tons at the inner and 2½ tons at the outer surface is converted into heat, and taking into consideration the masses the relative rise of temperature will be as $\frac{11}{7\frac{1}{2}}$ is to $\frac{2\frac{1}{2}}{19\frac{1}{2}}$, or as 11½ to 1 nearly. Thus it appears that whilst from this cause the tube is cooled, the hoop is heated and expanded, which is equivalent to reducing the initial shrinkage of the hoop.

But we have still to deal with the heat set up by the percussive force of the explosion. This we may assume to be some direct function of the induced strain. It will therefore, as regards the tube, be a maximum at the inner and will be zero at the outer surface, whilst it will be greater at the inner surface of the hoop as compared with the outer in the proportion of 11 to 2½ (assuming it to vary directly with strain).

Lastly, as regards the heat imparted from the powder gases. It may be shown that in the very short time of the operation this is confined to a very thin layer of the inner surface of the tube. The final result then is, that the inner surface of the tube is heated, whilst the outer surface is probably actually cooled, at the same time the inner surface of the hoop is considerably heated, and the outer surface also heated, though to a much less degree. The effect of the changes must therefore be to weaken the gun, though in a very different manner from the case of the homogeneous gun.

We come now to the wire gun, diagram E. Here the work done by the powder gases is represented by the arm BHOMNB, less the area BCN, that is, by the area CHOMNC. When the internal pressure is removed, the whole of this is converted into heat, but a portion of this between C and N would be neutralised by the cooling effect of the wires whilst converted into mechanical energy in passing from the compressive to the neutral state, and consequently the heating of the gun, though not absolutely uniform throughout, would be very nearly so. The heating from the percussive action would also be nearly uniform, being rather greater towards the inner surface. Now it can be shown that if a gun properly constructed either with hoops or wire be uniformly heated, the strains are not affected, and it therefore follows that in the wire gun the effect of heating is very slightly to alter the conditions and strength of the gun, and the wire gun, therefore, is in this respect far superior to the hooped systems.

We have now pointed out the difference in the mode of construction with hoop and with wire, we have compared the two systems and shown that for strength, facility, and economy of construction, the wire system has greatly the advantage; we have refuted the objections which have been taken to it, and the task which we undertook is completed. Doubtless it will occur to our readers to ask how it is that a system which promises so fair, and which was brought prominently forward upwards of a quarter of a century ago, has never till quite recently been tried by the gun-makers. How is it that millions upon millions have been spent at Woolwich on hoop guns and that this system has been persistently neglected?

We know that not only was it brought before the Ordnance Select Committee, twenty-seven years ago, and that not as a mere idea, but accompanied with experimental facts, which, as the late Mr. Bidder, then (1860) President of the Institute of Civil Engineers, stated publicly, established such a *primâ facie* case as should have received the attention of Government, but we know further that at various times since it has been fruitlessly urged that trials of the system should be made.

We presume that those who had the decision of such matters were so satisfied with what they were doing, and had so much confidence in their own system that they never gave their serious attention to what they thought to be the dream of a theorist. The inexorable logic of facts seems, however, at last to have come into play, and we believe that the recently-constituted Ordnance Committee is at present seriously engaged in the reconsideration of the whole subject of gun construction, and that wire guns will be admitted to be within the region of practical gun-making.

We trust it may be so, and that the system may be fairly tried, but in order that the trial may be fair, it is essential that it be conducted with due regard to those principles which it has been our object to explain—that the initial tensions of the wire coils be duly calculated and applied We insist specially on this, because not only has the Woolwich practice hitherto been to treat the shrinkage question in a hap-hasard rule of thumb method, but also Sir William Armstrong, in his late address as President of the Institution of Civil Engineers, made light of the precise degree of initial tension, and spoke of the tendency of the explosive force to effect an adjustment of the strains.

We cannot too strongly protest against such a view, as crude and unscientific, and any results which may be obtained from guns so constructed must be inconclusive as regards the principle of wire construction.

In concluding this article we bring before our readers sketches of three types of wire guns showing the application of the principle. The first is a heavy muzzle-loading gun, designed by the writer for land defences (Figs. 1 and 2). The gun is furnished with rollers on the trunnions at G, and recoils up a curved inclined plane, I I I, which is mounted on a turnable, so as to be capable of training in any direction in azimuth. The elevation is given by a hydraulic lift at K. The construction of the gun is shown in Fig. 1, in section. A A is the inner tube; B B the wire coiled on it; C the breech plug; E E is a heavy casting of cast iron, against which the breech plug rests, and which also forms the trunnions, G G; K K is a cast-iron casing covering the chase of the gun, and attached to the casting E E by strong iron bolts, F F. In this gun there is no longitudinal strain on the chase; the recoil being taken up by the insertion of the heavy mass behind the breech plug and by the force of gravity on the ascending planes of the carriage, aided by compressors.

The second type, Fig. 3, is a muzzle-loading gun mounted on an ordinary carriage. The main trunnions are behind the breech and are connected to the carriage trunnions B by side links C, so that the longitudinal strain is transmitted direct from the breech to the carriage without the intervention of the chase of the gun.

Figs. 4 and 5 represent the type for heavy breech-loading guns. In this case the breech plug is fixed in a massive block, A A, which slides backwards and forwards along the side rods, B B. Through this block passes an eccentric shaft, C, which terminates on each side in the side rods B B. When the eccentric is in its forward position the sliding block closes the breech. In the backward position the breech is open and the gun tops up on the forward trunnions E, so as to allow of the introduction of the charge as shown in Fig. 5. When the charge is introduced the preponderance is restored to the breech end, the gun falls back to its normal position, the eccentric is removed, the breech closed, and the gun is ready for firing.

In all these cases it is obvious that there is no longitudinal strain on the chase of the gun, and it is obvious that so far as construction is concerned there is no limit to the possible size of the gun.

James A. Longridge

BEN NEVIS OBSERVATORY

THE conditions of weather on Ben Nevis are now such as to render it impracticable and hazardous to continue the daily observations satisfactorily. I have therefore judged it best to discontinue them, after a very successful season, under the auspices of the Scottish Meteorological Society, of five months from June 1, without the break of a single day. The work at the six intermediate fixed stations has, I am very pleased to say, been well and generally punctually kept up throughout, and I trust that much good will result. Simultaneous observations were of course made at the observatory at Achintore, Fort William. The Stevenson's screens at these stations have now been made firm by wire stays to withstand the storms of winter. Yesterday Colin Cameron, the guide, accompanied me. The track was snowed up, and it was necessary to force a way through great banks and drifts of snow. The average depth was two feet; once we got off our course in the blankness of thick cloud-fog and trackless snow. To-day the weather was very bad on the summit, the hut was partly filled by drift, and the south-east gale was so violent at times that I could hardly make way. Possibly I shall attempt weekly or periodical ascents during the winter to keep up the registrations of the rain-gauges and self-recording thermometers.

I have to-day commenced provisionally a three-hourly system of observation at Fort William (including 3 a.m.). The special features are sea temperature, ozone, and the reading and setting of the self-registering instruments on each occasion. Of course all the other usual elements are three-hourly observed also. Further particulars are reserved for a future number.

Clement R. Wragge

Fort William, November 1

THE OYSTER INDUSTRY OF THE UNITED STATES

A VERY complete account of the history and present condition of the oyster industry of the United States has been recently prepared by Ernest Ingersoll, under the direction of Prof. Baird, United States Commissioner of Fisheries. The importance of this industry it is not easy to over-estimate, and the United States Government deserve every credit for their efforts to preserve and extend it.

As having an important bearing on the question, the oyster-beds of the maritime provinces of Canada are briefly referred to. The eastern coast of the province of New Brunswick is washed by the Gulf of St. Lawrence; down in the bottom of the Gulf lies the long, irregularly shaped Prince Edward's Island, between which and the mainland flow the shallow but troublesome currents of Northumberland Strait. The shores on either side of this Strait are for the most part low bluffs of reddish soil and sloping meadows; there is little solid rock, few prominent headlands, but a continuous line of shore, shelving very gradually into water, nowhere deep; many rivers come down along the coast of the Gulf, and at the mouth of each there is an estuary proportionate to the size of the stream, from the mighty channel of the St. Lawrence to the miniature bay of Bedeque. Most of these estuaries are shallow, and most of them are protected from gales. This condition of affairs seems well suited for oyster growth, since nearly all of these estuaries either contain or contained large colonies of these mollusks. Except at its western end, Prince Edward Island is engirdled with oysters. That most beautiful salt-water lake in the world, the Bras d'Or, which occupies the whole interior of Cape Breton Island, fattens multitudes of oysters. These Canadian oysters are of large size, and have thick, strong shells; oysters with shells from eight to ten inches in length are not extraordinary. The best are not the longest, but those with straight and narrow, or evenly-rounded shells. All the oysters on the eastern shores of North America, belong to the species known as *Ostrea virginiana*, which embraces many varieties, of which *O. borealis* is perhaps the best-marked. Except at wholly unsuitable places, it is to be found almost without interruption from the northern shores of the Gulf of Mexico and the coast of Florida to the Canadian districts just referred to. It is, however, said not to be found along the eastern shores of Maine, nor in the Bay of Fundy, though the shells, in a semi-fossil state, are dug up in quantities from the deep mud in the harbour of Portland, Maine.

Mr. Ingersoll gives a very interesting account of the former extent and condition of the native beds in the Gulf of Maine, and of the evidence of the immense consumption of the oyster by the Indian tribes. The shell mounds discovered are of immense size, and the shells themselves reached a quite monstrous dimension; the animals were killed either by fire, or by smashing in the shell at the attachment of the adductor muscles, and possibly even by the opening of the shell by stone knives. In many localities north of Cape Cod, the disappearance of the oyster has been comparatively recent. Some ascribe this to the pollution of the water by mills, but Prof. Verrill thinks a change of climate may have had something to do with it. Oyster culture has been tried, but unsuccessfully, on this coast; a great business in "laying down" oysters is still carried on at Wellfleet.

Coming south of Cape Cod, we find Buzzard Bay and Vineyard Sound, early known for their fine beds of natural oysters. More than a century ago, strict regulations were made about their take and export, but these beds would seem to be nearly worked out.

The charter of Charles II. gave the colony of Rhode Island (1683) free fishing in every form. At one period large quantities of oysters were destroyed for the sake of the lime in their shells. Now statutes are in force specially guarding the mollusc, and the oysters are now yearly increasing in quantity and lessening in price, and over 960 acres of oyster-ground were let in Rhode Island in 1879. About one-half of the oysters raised are natives, and the other half are Virginia oysters brought to the grounds to be fattened. The probable amount of capital invested in this district may be about 1,000,000 dollars, and the yield and value as against this is about 600,000 dollars at wholesale prices.

The Virginia trade began some fifty years ago, when Capt Farran gathered a sloop-load of some 600 bushels. Now the profits of a single firm in 1856 were 25,000 dollars a year. When the native supply grew slack, very successful efforts at cultivation were made. Out of seven to eight thousand acres marked for oyster-culture in New Haven Harbour, only one-half are in use. One proprietor operates on 1500 acres, and full details of the various methods of culture adopted are given in this report.

Coming further south, the southern shore of Long Island was early famous for its oysters, and we know how the old blue point oysters were relished by the Dutch settlers. In 1853 they were sold for an average of ten shillings a hundred from the beds. In 1873 Count Pourtales called attention to their getting scarce, and since 1879 it has required an importation of 100,000 bushels of seed to keep up the supply. This seed then had only to be gathered, or was worth but little, now its price has increased threefold. The principal market now-a-days for these Blue Points is Europe. In the markets of London they commanded a high price, retaining their supremacy over all other sorts, until in 1879 when the season being a bad one, the oysters grown in Staten Island Sound surpassed them. Not only are they of a superior flavour, but they have a round thin shell, and are of a medium size. The Rockaway district supplies an immense quantity of oysters; it is but the western end of the south shore of Long Island. While most of the stock finds its way to New York, lately the oysters from this district have found their way into the European market, selling as "French" stock. In New York Bay the growth of transplanted oysters is fairly rapid, and a great many are sent from there to Europe. In New York City the oyster trade is of very considerable importance, which centres itself in two localities at the foot of Broome Street, East River, and of West Tenth Street, North River. The quantities handled each year in the city has been approximately estimated as about 765,000,000 oysters. A large number go to England, where the "Blue Points" having lost favour, the "East Rivers" and "Sounds" have taken, in a measure, their place. Between October 9, 1880, and May 14, 1881, being one season, there was exported from New York to Europe a total of 70,768 barrels, of which 68,140 barrels went to Liverpool. Each barrel contained on an average 1200 oysters.

Along the New Jersey shore a large quantity of oysters are raised, and the western shore of Delaware Bay is the scene of planting the southern oysters, which are brought annually from the Chesapeake, and are fattened for the markets of Philadelphia. This city is credited with an intake of oysters, amounting in 1880 to about 800,000,000, but then, unlike New York, this quantity is not wholly consumed in Philadelphia, but is in part distributed to the surrounding regions, but the calculation has been made that this million-peopled city consumes on an average during half the year, 300,000,000. The retail trade gives employment to over 3500 people.

The oyster fisheries of Maryland are among the most important in America, and it is claimed that the beds of Chesapeake Bay, about equally divided between the two States of Maryland and Virginia, contain the best oysters in the world. The oyster trade of this region is immense, giving employment to thousands. A body of police, with a steamer and two tenders, with eight sloops, watch hourly over the grounds, but the territory to be watched is so vast—the beds of Maryland extend for a distance of 125 miles—that the police sometimes fail to catch illegal dredgers, and serious encounters, as in the winter of 1879-80, have occurred.

It cannot be too often asserted that even the splendid beds of this district may, by unrestricted dredging, become exhausted. Properly protected and cared for, this wealth might be increased manyfold. Thirty years ago we read, the depletion of the beds at Pocomoke Sound and in Tangier seemed a thing impossible, now from want of a period of rest they have fallen off in their produce, the former by four-fifths, the latter by two-thirds. The statistics of this great fishery extends over many pages. It was at Baltimore the "steamed" oyster trade began, and this city, the great oyster market of the United States, packs more of this mollusc than any other city in the world.

In North Carolina the business in oysters and their culture is of small proportions, and not much is known of the fisheries of Georgia. Of the oyster interests in Florida there is little to be said. Coming to the Gulf of Mexico, the Mobile supply must be noted, as they have a high reputation for excellence. The New Orleans market is supplied from an extent of coast comprising the whole water front of North Mississippi and Louisiana.

Appended to this report there is a condensed account of the anatomy and development of the oyster, taken from the memoir of Dr. W. K. Brooks, of the John Hopkins University of Baltimore, and accompanied by a full series of drawings of the growth of the young oyster.

NOTES

THE following is the list of names nominated for the Council of the Royal Society to be balloted for on November 30:—President, William Spottiswoode, M.A., D.C.L., LL.D. Treasurer, John Evans, D.C.L., LL.D. Secretaries: Prof. George Gabriel Stokes, M.A., D.C.L., LL.D., Prof. Michael Foster, M.A., M.D. Foreign Secretary, Prof. Alexander William Williamson, LL.D. Other Members of the Council: Prof. W. Grylls Adams, M.A., F.C.P.S., John Ball, M.A. F.R.A.S., Thomas Lauder Brunton, M.D., Sc.D., Prof. Heinrich Debus, Ph.D., F.C.S., Francis Galton, M.A., F.G.S., Prof. Olaus Henrici, Ph.D., Prof. Thomas Henry Huxley, LL.D., Prof. E. Ray Lankester, M.A., Prof. Joseph Lister, M.D., Prof. Joseph Prestwich, M.A., F.G.S., Prof. Osborne Reynolds, M.A., Prof. Henry Enfield Roscoe, B.A., LL.D., Marquis of Salisbury, K.G., M.A., Osbert Salvin, M.A., F.L.S., Warington W. Smyth, M.A., F.G.S., Edward James Stone, M.A., F.R.A.S.

THE death is announced, at the early age of thirty-two years, of Prof. Marino Palmieri, Professor of Physics at Naples University, and so well known for his seismological researches. We hope to refer to Palmieri's work in an early number.

WE also regret to announce the death of Prof. J. Th. Reinhardt, Professor of Zoology at Copenhagen University and Inspector of the Natural History Museum of that city, an ornithologist of great merit; he died at Copenhagen on October 23, aged sixty-six. The death is also announced of Dr. F. H. Troschel, Professor of Zoology at Bonn, and of Dr. Julius Friedländer, the head of the well-known Berlin publishing house and scientific agency of that name.

PROF. VIRCHOW has had a serious attack of illness, but we are glad to learn from the latest intelligence that he is slightly better.

WE see from *The Gazette* of Montreal that the meeting held in that city on October 26, in connection with the proposed visit of the British Association to Canada in 1884, was large and influential. Much enthusiasm was displayed at the prospect of the Association's visit, and several resolutions were passed guaranteeing a hearty welcome and every provision for the success of the meeting, and the comfort and entertainment of the visitors. A large committee was appointed to carry out arrangements, and at the close of the meeting a considerable sum was subscribed as a guarantee fund. Dr. Sterry Hunt stated that in 1884 the American Association would probably meet at Newhaven, at such a time as to admit of the English visitors attending both meetings.

ON October 9 was unveiled, at the University town of Würzburg, a memorial to Von Siebold, the celebrated Oriental *savant*. For some years past the Horticultural Society of Vienna has collected subscriptions for this purpose, and it is interesting to note that a considerable sum was subscribed amongst the Japanese, although they have already erected a colossal stone to his memory at Nagasaki. Siebold was the greatest of all the students of Japan during what may be called the Dutch period, that is, from about 1620, when all Europeans except the Dutch were expelled from Japan, down to 1854, when Perry succeeded in making the first of the recent treaties with that country. During this time the facilities for the foreign student were few. The members of the Dutch factory were confined to the settlement at Deshima, which was about the size of a small London square; all egress, except on certain rare occasions, was denied to them, and this intercourse with the people was confined to the few interpreters and officials employed to watch their movements. Once a year the head of the factory, with a small suite, journeyed overland to Yedo with presents to the Shôgun; but while on the road the foreigners were as closely guarded as prisoners, and all opportunity of conversation or intercourse with the people was denied them. Notwithstanding these unpromising circumstances, however, Kaempfer, Titsingh, Thunberg, and especially Siebold, succeeded in obtaining the materials for works which will for years to come retain their position as the very best works in the country. About 1820 Siebold was appointed surgeon to the Dutch forces in Java, and in 1826 made his first voyage to Japan, where he became physician to the factory at Deshima. He seems first to have acquired a sound knowledge of the language, and then, through the native *employés*, to have procured books as he required them. For eight or ten years he remained quietly in Japan, accumulating vast stores of information for subsequent use, and journeying occasionally with the annual mission to Yedo. On his return to Europe he proceeded to publish his great works, "Fauna Japonica," and "Flora Japonica," the expenses of which were defrayed, we believe, by the King of the Netherlands. He again returned to Japan, and was there during the signing of the American and other treaties, and was even in this early time constantly employed by the Japanese Government in advising them how to thread their way through the difficulties of their new position. On one of his previous journeys to Yedo he had received permission to reside there for a period, provided he taught western medicine to a number of Japanese students. He got into serious danger through having in his possession a complete native map of Japan, which one of his pupils had succeeded in conveying to him. The latter is said to have lost his life, and Siebold returned to Deshima. On his second return to Europe with his large collection of Japanese books, maps, specimens of the artistic productions of the country, of the fauna, flora, &c., he was received with honour by the Emperor of Russia and other European potentates. He then commenced the publication in parts of his *Magnum opus Nippon*, which he never lived to complete. This work might with much justice be styled the Encyclopædia Japonica. Besides native works, every book published in the East in European language was consulted. Whatever the labours of subsequent students, large sections of this book, such as the history of European discoveries in the Eastern seas, will always retain their value. After his death his vast collections were distributed among various museums on the Continent. The larger share, as was only natural, went to Leyden; but the British Museum succeeded in obtaining his splendid library of Japanese books and maps.

THE August number of the *Mittheilungen der deutschen Gesellschaft* of Yokohama contains several papers of much interest. The numerous and curious New Year's customs of Japan are described by Mr. Sataro Hirose, a native medical student, while Mr. Schült gives a topographical sketch of Mount Fugi and its neighbourhood. Dr. Scheube contributes a long paper on the food of the Japanese. He was enabled, in the college with which he is connected, to examine the food of various classes, and from his statistics, meat appears to play but a small part in the nourishment of the people. Rice occupies about 50 per cent. of their total diet. Dr. Baelz describes the various infectious diseases of Japan, and Mr. Leysner furnishes statistics for the past ten years of the climate of Niigata, the principal town on the West Coast. The number and value of the contributions of this small society—it numbers only forty-nine resident members—would be little short of astounding, did we not recollect that most of the Germans employed by the Japanese Government are men of scientific attainments, and devote much study to the country in which they live.

WE have received from M. Georges Dary, of Paris, a note commenting on Prof. S. Thompson's article upon Electric Navigation. M. Dary informs us that the source of power upon which M. Trouvé has fallen back is a bichromate (primary) battery weighing only 120 kilogrammes, or less than one-tenth of the accumulators used by Mr. Volckmar in the iron launch *Electricity*. This battery, M. Dary states, has an electromotive force of 96 volts—equal to that of the 45 accumulators—but he does not state what strength of current it will furnish, nor for how many hours. M. Dary adds that 500 similar apparatuses—he does not say whether this means 500 boats, or 500 batteries, or 500 motors—like that used by M. Trouvé in navigating the Seine in his skiff, have already been exported from Paris. This bichromate battery, it appears, has enabled M. Trouvé to undertake journeys which with little exaggeration may be called long voyages, as, for example, from Havre to Rouen; and there are numerous owners of electrical boats who run every day between places twelve or fourteen miles apart, using two sets of cells for the run. We are glad to be able to do to so ingenious an inventor as M. Trouvé the justice of making more widely known the real progress which he has made in this matter.

A COLOSSAL statue of George Stephenson, and another of James Watt, both after models by Prof. Keil, are now being completed in the studio of the eminent German sculptor, Herr Bock, and are intended for the new Polytechnic at Charlottenburg, near Berlin.

THE comet was seen at the Paris Observatory by M. Bigourdan, one of the astronomers, on October 23. It was found to be very brilliant. The observation was presented by M. Mouchez, with two others done by M. Thollon at the Nice Observatory. The sodium lines, which were very brilliant on September 18, had wholly disappeared on October 9, when the comet was seen for the first time after a very long observation of the sky.

THE first meeting of the One Hundredth and Twenty-Ninth Session of the Society of Arts will be held on Wednesday, November 15, when the Opening Address will be delivered by Charles William Siemens, D.C.L., LL.D., F.R.S., Chairman of the Council. The following papers are announced for reading at the meetings before Christmas:—J. Hopkinson, D.Sc., F.R.S., Ice-making and Refrigerating; W. H. Preece, F.R.S., Electrical Exhibitions; William A. Gibbs, the Artificial Drying of Crops; P. L. Simmonds, the Utilisation of Waste; W. K. Burton, the Sanitary Inspection of Houses. For meetings after Christmas:—J. H. Evans, the Modern Lathe; Capt. J. H. Colomb, R.N., Collisions at Sea; A. J. Hipkins, the History of the Pianoforte; J. Donaldson, the Construction of Torpedo Boats; C. F. Cross. F.C.S., Technical Aspects of Lignification; W. N. Hartley, F.R.S.E., Self-purification of River Waters; James J. Dobbie, D.Sc., and John Hutchinson, the Application of Electrolysis to Bleaching and Printing." Arrangements have been made for Five Courses of Cantor Lectures:—On Dynamo-Electric Machinery, by Prof. Silvanus P. Thomson, D.Sc.; on Solid and Liquid Illuminating Agents, by Leopold Field; on the Decorative Treatment of Metal in Architecture, by G. H. Birch; on the Transmission of Energy, by Prof. Osborne Reynolds, M.A., F.R.S.; on Secondary Batteries, by Prof. Oliver J. Lodge, M.A., D.Sc. The usual short Course of Juvenile Lectures will be given during the Christmas holidays by Prof. Henry Nottidge Moseley, M.A., F.R.S., on the Inhabitants of the Ocean.

PROF. GEORGE M. MINCHIN will publish very shortly, at the Clarendon Press, a work on "Uniplanar Kinematics of Solids and Fluids, with Applications to the Distribution and Flow of Electricity." It aims at supplying a deficiency in the course of mathematical physics usually pursued by the higher-class students in our colleges and universities, by enabling them to enter into the study of kinetics with clear notions of acceleration and other leading conceptions which belong to the province of kinematics.

THE delegates of the Clarendon Press have determined to issue a series of translations of important original papers in foreign languages on biological subjects, and have committed the editing of these memoirs to Dr. Michael Foster, Dr. Pye-Smith, and Dr. Burdon Sanderson. It is proposed that the series should begin with a single volume of about 750 pages, to be divided into three parts: the first to comprise the treatise of Prof. Heidenhain on "The Physiology of the Process of Secretion"; the second a series of four papers by Prof. Goltz on "The Functions of the Brain," and a memoir by N. Bubnoff and Prof. Heidenhain on "Excitatory and Inhibitory Processes in the Motor Centres of the Brain"; and the third a series of memoirs by Prof. Engelmann on "The Structure and Physiology of the Elementary Contractile Tissues." It is intended that each part should be complete in itself, and should be published separately.

THE medical faculty of the Göttingen University has announced as a subject of prize competition, for 1883, a thorough investigation with the more recent aids of microscopical art, of the mucous membrane of the bladder and urethra of both sexes, especially with reference to their gland-contents, and the varying forms of the epithelial cells in expansion of the ducts. The philosophical faculty propounds two subjects, one of which is an investigation and setting forth of the mode of development of the flower of our common mistletoe (*Viscum album*), with critical consideration of the literature of the subject.

MOUNT ETNA has for some days been showing great and increasing activity, emitting flashes of fire and dense volumes of smoke.

AN Arabic manuscript of the year 1365, from which Herr Gildemeister has translated several extracts for the Göttingen Society of Sciences, affords an interesting peep at nautical matters among the Arabians of those times. The author deals separately with the ships of the Mediterranean, of the Indian Ocean, and Red Sea, and of the Nile and other rivers. *Inter alia*, he describes a mariner's compass; and this is noteworthy, inasmuch as only one description of the instrument in an Arabian work has hitherto been known (it is of date 1242). The following is a curious picture:—"A ship [of the Indian Ocean] carries generally four divers, whose only duty is, when the water rises in the ship, to smear themselves with sesame oil, stop their nostrils with wax, and, while the ship is sailing, jump into the sea. Each has two hooks connected with a thin line; one of these he fixes in the wood of the ship, and with the other he dives. He swims like a fish a little under the water, and uses only his ear. Where he hears the trickling of water he stops with wax where there are holes, stopped with palm stems, and where there is sewing, he often passes a piece of cocoa fibre through the fixed palm stem. The thing is easy to him; in a day he stops over twenty or thirty leaks. The diver comes up, without inconvenience, whether there is wind or calm."

THREE new Lyceums, in which instruction will be given in Finnish, will be opened in a few weeks in Finland, at Abo, Uleaborg, and Bjorneborg, thus raising the number of Finnish Lyceums to eight. In the Helsingfors University, lectures in Finnish are delivered on all subjects in connection with the Archæology and History of the north, as also in Botany by Prof. Wainio.

M. W. DE FONVIELLE has just published (Hachette and Co.) a little volume on "La Pose du Premier Cable," in which the principal incidents connected with this great undertaking are told in a dramatic and popular manner.

MR. MUYBRIDGE has issued a series of his well-known instantaneous pictures of animals in motion, adapted for the zoetrope. Those sent to us include the horse under various conditions, the deer, and the dog. They are exact reproductions of the photographs, and in their faithfulness to reality are a great improvement on the existing zoetrope pictures. Mr. Muybridge is preparing for publication a complete series of his original photographs, adapted for his zoopraxiscope.

UNDER the title of "La Navigation Électrique" (Paris, Bandry), M. Georges Dary gives some interesting notes on electric navigation, with special reference to the experiments of M. Trouvé. Bemrose and Son have issued a little handbook, "The Electric Light Popularly Explained," by Mr. A. Bromley Holmes; and Macmillan and Co. a useful manual of "Electric Light Arithmetic," by Mr. R. E. Day, M.A.

THE Austrian Archæological Expedition to Asia Minor has returned to Vienna, and the objects found in the excavations made and packed in 167 cases have arrived there.

PROF. SIMONY has recently ascended the Dachstein in order to make some exact measurements concerning the decrease of the Dachstein glaciers. He found that the so-called Karlseisfeld has since 1856 lost about 50-60 metres in thickness, the middle portion about 40-50 metres. The decrease in the thickness of the ice is most noticeable in the high and steep descent from the middle to the lower portion of this glacier. Here a piece of the glacier-bed—a rock of about 30 metres in height and 60 broad—has been laid quite bare. Up to 1856 the glaciers were steadily increasing, but since then the decrease has been equally incessant.

IN the ordinary air thermometer the pressure of the air in the thermometric bulb is generally measured by means of a mercury manometer. M. Schneebeli, of Zurich (*Archives des Sciences*), employs, instead of the latter, a metallic manometer, of the Hottinger-Goldschmidt system. The bulb of the thermometer terminates in a capillary tube, to which the manometer is connected by means of another capillary tube of lead. The space between the latter and the elastic membrane of the manometer is filled with glycerine. M. Schneebeli believes the arrangement capable of being really serviceable to industry, because of the simplicity of its construction and of the manipulations required. A mere reading of the position of the manometric pointer gives the temperature.

OUR ASTRONOMICAL COLUMN

COMET 1882, *b*.—In consequence of cloudy mornings, it is stated that this comet was not seen at Melbourne until 5 a.m. on September 10; it was visible with the telescope till within one minute of sunrise, and its intrinsic brightness was estimated equal to that of the planet Jupiter. The tail was well defined and bright, but extending only over 3° or 4° at most. At 5h. 24m. 51s. a.m. its right ascension was 9h. 45m. 46·61s., with 0° 53′ 36″ south declination.

At Adelaide the comet was remarked from the observatory on the morning of September 9, but Mr. Todd reports that a police-constable had seen it a few mornings previously.

Prof. Riccò observed it at 11 a.m. on September 22, with the Palermo refractor of 0·25 m. aperture; there was a trace of a tail towards the south-west. At the same hour on September 23 Prof. Millosevich saw it at Rome, and describes it as "un fiocchetto di lana disegualmente illuminato."

It appears by no means improbable that with our larger telescopes the comet may be visible till the end of the year, or later. About the time of new moon, or at midnight on January 8, its place will be in R.A. 6h. 53m., with 23° south declination, distant from the earth 2·21, and from the sun 2·57, so that it will be upon the meridian at 11h. 40m. p.m., with an altitude of more than 15° at Greenwich.

With regard to the distinguishing letter which has been attached to the comet in this column, Mr. T. W. Backhouse writes from Sunderland:—"Surely it is a mistake to call the present comet 'Comet *b* 1882.' Is not Well's comet *a*; the comet seen in the eclipse, *b*; the great comet, *c*; Barnard's comet, *d*; and Schmidt's, *e*?" On this point we should reply that the main or indeed only reason for attaching letters to comets as they are discovered is to afford a ready means of distinguishing them *while they are under observation*: when the orbits are catalogued the comets appear as I., II., III., &c., of a particular year. The comet of May 18 was only seen for a minute during the totality of the eclipse, having been looked for unsuccessfully morning and evening subsequently, at least by M. Trepied. It is not likely to be mentioned except in connection with the eclipse, and there is, consequently, no apparent utility in assigning a letter to it. We may take the opportunity to remark that M. Trepied, who did not regard this object as a comet while he had it in view, has informed us in conversation within the last fortnight that he is now quite convinced of its cometary nature.

THE NOVEMBER METEORS.—The first comet of 1866, in the track of which the periodical meteors of November are found to move, has probably just passed the aphelion point of its orbit, which is distant from the sun 19·673, the earth's mean distance being taken as unity. It may be interesting to note the character of the shower under this condition, should it be repeated when the earth arrives at the descending node of the comet's orbit on the evening of November 13.

On the morning of October 23, when the great comet was so favourably viewed in the vicinity of London, a number of bright meteors diverged from a point not far from the radiant of the November shower.

GEOGRAPHICAL NOTES

ACCORDING to the Russian newspaper *Sibir*, the meteorological expedition to the mouth of the Lena has started on board large boats provided with all necessaries for building a house, and for successful wintering. The station will be erected on the Tumanskaya branch of the Lena, if the water is deep enough in this branch to allow the passage of the boats. It is hoped that, with the exception of the three summer months, the reports of the station will reach Yakutsk regularly. They will be sent, first, by M. Jurgens to Bulun; thence they will be forwarded to Nerkhoyansk, where they will be taken up by the post, which will run twice a month instead of once every four months as before. In the summer, the *tundra* being covered with water, messages can be sent only *viâ* the Lena; they will be taken by the merchants who leave Bulun for Yakutsk, as soon as the ice is melted, and reach Yakutsk in the end of July; another message can be sent with the returning fishermen, who reach Yakutsk in September.

THE *Germania*, which conveyed the German North Polar Expedition to Kingawa in Cumberland Sound, has returned to Hamburg. When the *Germania* left Kingawa on September 6, the observatory was completed, so that observations had already begun. Besides the two larger expeditions sent out by the German Government, Dr. Koch has also been sent to Labrador in order to establish meteorological observatories among the missionary settlements of the Moravian brotherhood. Dr. Koch arrived at Hoffenthal Port on August 10, and was liberally supported by the missionaries. All the stations set down in the programme, viz.: Hoffenthal, Zoar, Nain, Ramah, Hebron, and Obak have now been established. A meteorological station has also been established on the Falkland Islands. It is to form an intermediary between the stations on the South American continent and that on South Georgia, and also to help in rendering more valuable the observations made on board of vessels passing through the neighbouring seas. Capt. Seemann, who was sent to the Falkland Islands by the Deutsche Seewarte, reports that work has begun.

A DESPATCH, dated September 19, has been received in Stockholm from the Swedish Meteorological Expedition at Smith's Observatory, Spitzbergen. It states that observations are being regularly made, and that all was well with the members.

THE November part of Hartleben's "Deutsche Rundschau für Geographie und Statistik" contains articles on land formations in the Sunda district, by Jos. v. Lehnert; on the position of women in the life of peoples, by Dr. M. Geistbeck; on the North Sea according to the investigations of the Norwegian Expedition during the years 1876 to 1878, by Dr. J. Chavanne; on the ethnography of Central Asia, by Prof. Ujfalvy; on the transit of Venus and the solar parallax, by Dr. J. Holetschek; on the hydrography of Africa and the Welle problem, by J. Chavanne. There is a good ethnographical map of Central Asia.

A CATALOGUE of the fine commercial collections in the Oriental Museum in Vienna has been issued, as also a small volume of "Neue Volkwirthschaftliche Studien uber Constantinopel und das anliegende Gebiet." In the latter, especially, the ornithologist will find several things to instruct him.

THE Municipal Council of Paris has granted unanimously a gold medal of 120*l*. to M. Savorgnan de Brazza, for his discoveries in Tropical Africa.

LIEUT. BOVE, together with the Italians of the Antarctic Expedition scientific staff, arrived at Genoa all well.

THE well-known Bremen naturalist, Dr. Otto Finsch, to whose travels in Polynesia we recently referred, has just returned to Berlin. During the last six months the traveller was in New Guinea, and instituted anthropological comparisons between the Papuans and the Eastern Melanesians. He is accompanied by a native of New Britain, aged fifteen. His sketches of New Zealand, New Britain, New Guinea, and the Caroline Archipelago are exceedingly well drawn and valuable. He brings about 100 cases of ethnographical specimens intended for the new Ethnological Museum at Berlin.

THE AIMS AND METHOD OF GEOLOGICAL INQUIRY[1]

IN entering upon the duties of this Chair I can hardly do better, perhaps, than try to set before you what are the primary aims and general bearings of that branch of natural science which we are about to study, and to indicate the nature of the problems with which it deals. In doing so I will endeavour at the same time to point out the method of research and the mode of reasoning which we must pursue if we are to be successful investigators. Geology (in which comprehensive term I include mineralogy and palæontology), is concerned in the first place with observations of minerals, rocks, and fossil organic remains, and in the second place with the inferences which may be drawn from those observations. Its object is thus not only to note the nature and position of the various materials which constitute the solid crust of our globe, but by processes of inductive and deductive reasoning to ascertain how minerals and rocks have been formed and caused to assume the different appearances which they now present. In few words, then, our science might be defined as an inquiry into the history or development of the earth's crust, and of the several floras and faunas which have clothed and peopled its surface. It thus treats of the genesis of oceanic and continental areas—of mutations of climate—of the appearance and disappearance of successive tribes of plants and animals. More than this, in revealing the past it throws strong light upon the present, and has, perhaps, more than any of the cognate sciences, tended to revolutionise our conceptions of nature, and to lead zoologists and botanists into fruitful fields of inquiry which their own proper studies, no matter how assiduously prosecuted, could never have enabled them to reach.

Dealing, as geology does, with the operations of Nature in the past, it is obvious that before we proceed to interpret the record of the rocks we ought to have a clear knowledge of the mode in which Nature works at present. Without this preliminary knowledge, it is just as hopeless to attempt to decipher that record as it would be to endeavour to understand a page of Greek without having first mastered the grammar and rudiments of that language. We must turn our attention then, at the very outset, to a study of those great forces by the action of which the crust of our globe is being continually modified. It is essential that we learn to appreciate the work done by the atmosphere, by frost and snow and ice, by rain and underground water, by rivers and lakes, by the sea, by plants and animals, and by the subterranean forces, before we can hope to recognise the different parts which those various agents of change have performed in the past. All geologists are agreed upon this, and are ready to acknowledge it as the chief article of their faith. Nevertheless, this obligatory article has received different interpretations. Some, for example, have held that the present condition of things must be taken as the exact type of all the phases through which the earth's surface has passed, during the different stages of which we have any recognisable records preserved to us in the stratified rocks of the globe. They admit that countless modifications of land and sea have taken place—that the climate of particular areas has varied again and again—that the subterranean and volcanic forces have manifested themselves with unvarying intensity, now in one place, now in another—but they hold that all these changes have been accomplished upon the same scale and at the same rate as at present, and that, as a consequence, the development of floras and faunas, so far as that is dependent upon physical conditions, has proceeded no more rapidly in former times than in our own day. They do not, indeed, deny that in the very earliest stages of the earth's history the agents of geological change must have acted with greater intensity than now, but of such a period, they tell us, we have no certain evidence treasured up in the sedimentary rocks, or at least such evidence, if it should exist, has not yet been detected. Only allow time, they say, and the constant drop will wear away the hardest stone. The gradual elevation of land, which is now going on in certain parts of the globe at so slow a rate that some have been inclined to doubt whether there is any movement at all, would nevertheless suffice in time to lift tracts now within tide wash into stupendous table-lands and mountains. Nor is it necessary, we are assured, to suppose that the apparent evidence of convulsive rending and displacement of strata, which is often so conspicuous in the heart of great mountain-chains and ranges, is really any proof of paroxysmal action. All the rupturing and confusion which we may note among the Alps and not a few mountain-regions, may quite well have been brought about, we are informed, in the most gentle and gradual manner.

Other theorists, again, are of opinion that, while the agents of change have necessarily been through all time the same in kind, they have yet varied again and again in degree, and that the present moderate condition of things cannot therefore be taken as an exact type and pattern of all preceding phases in the world's history. They cannot allow that the elevation of mountain-chains and the larger fractures and displacements of strata are the result of the repetition of such small movements of the crust as are taking place now. Admitting that considerable areas of the earth's surface are rising at the rate of a yard or more in a century, they yet cannot agree that this is a criterion by which to estimate the time required for the elevation of all protuberant parts of the earth's crust. They remind us that in our own day we have had experience of paroxysmal changes of level, nor can they doubt that similar sudden catastrophes must have happened oftentimes in the lapse of ages. They point to the appearance of ruin and confusion which may be traced along a line of mountain-elevation, and maintain that the broken and shattered strata are proofs of a more or less sudden yielding to enormous strain or tension. They do not deny that upheaval may have been going on over a given area at an extremely slow rate during long periods of time, but they argue that a point would at last be reached when the tension to which the strata were subjected could no longer be resisted. A sudden fracturing would at last take place—the strata would be violently dislocated, thrust forward, crumpled, and heaped, as it were, in confused and steeply-inclined masses along the main line of dislocation. Again, it is objected to uniformitarian views that these do not explain and cannot account for certain remarkable mutations of climate which are known to have occurred. It is not denied that the earth has been receiving for untold ages the same annual amount of heat from the sun, but it is maintained that, owing to certain astronomical changes, and the modifications induced thereby, that heat must have been very differently distributed over the globe at various epochs in the past. It is held, in short, that the climate both of the northern and the southern hemispheres has thus been frequently modified, and that in consequence of this the action of the geological agents has been influenced again and again—the decay and reconstruction of rocks the oscillations of the land—and the development of floras and faunas having been alternately accelerated and retarded according as extreme or moderate conditions prevailed.

Thus each school has its own method of interpreting the fundamental axiom of our science—that the Present is the key to the Past. And as the primary aim of geology is to interpret the stony record with a view to the reconstruction of our earth's history, it is obviously important that we should be able to satisfy ourselves as to which of these rival conceptions is most consonant with truth. In other words, we must do our utmost to ascertain which gives the most reasonable interpretation of geological phenomena. Each view must in its turn be tested by an appeal to facts, and a rigorous application of logical analysis. Probably we shall find that while there is much to be said on both sides, we can agree entirely neither with the one school nor the other. Before we are in a position, however, to discuss such questions, we must first have ranged over a very wide field of inquiry, and obtained a thorough grasp of the principles of our science.

Meanwhile, our chief concern in beginning our studies must necessarily be to detect resemblances between the present and the past. For every observation we make we must endeavour to discover a correlative phenomenon in the present order of things. And so long as we confine our attention to the facts before our eyes and to the more obvious interpretations of these which are suggested by forces now in action, we shall not fail to be impressed with the uniformity of nature.

We examine, let us suppose, a section of strata exposed upon the sea-shore or along the banks of a river. Our knowledge of the different kinds of sediment in course of transportation and accumulation at the present day enables us at once to recognise, in the conglomerate sandstone and shale of our section, simply the consolidated sediments of earlier times. The occurrence of fossils in the strata determines whether the deposits were formed in fresh water, brackish water, or the sea—whether near to a coast-land, or at a greater distance from the shore, and so forth. If some of the fossils be of terrestrial origin, while others are brackish-water and marine, we gain not only certain knowledge of the life of the period, but, if the evidence be full enough, we may even form more or less reliable conclusions as to the physical and climatic conditions which at one time existed in the locality under our examination. In short there are many almost obvious conclusions, as we may term them, which the appearances presented by an individual exposure of rocks must suggest to any observer who has previously become familiar with the operation of the natural forces in the world around us. He simply compares the facts with what is now taking place, and is thus led to conclude that effects the same in kind have been produced in the same way.

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[1] The Inaugural Lecture at the opening of the Class of Geology and Mineralogy in the University of Edinburgh, October 27, 1882, by James Geikie, LL.D., F.R.S. L. and E., Regius Professor of Geology and Mineralogy in the University.

Sometimes, however, the rocks present appearances which are harder to interpret in this obvious and ready manner. We encounter, for example, a rock-mass having none of the features presented by ordinary sedimentary strata. Instead of being made up, like conglomerate and sandstone, of rounded stones or grains, arranged in layers, it is entirely composed of larger and smaller crystalline particles, not lying in lines and layers, but scattered indiscriminately through the whole mass. It does not occur in beds like ordinary sedimentary strata, but on the contrary we see it cutting, as it were, across other rocks, and sending out veins which penetrate the latter in all directions. The observer immediately concludes that the crystalline rock is of younger age than the beds traversed by it; and not only so, but that the whole mass with all its veins was injected into its present position in a liquid, semi-liquid, or pasty, and probably heated condition. And in confirmation of this last conclusion he may perhaps note that the rocks immediately adjoining the dykes and veins betray the appearance of having been subjected to the action of heat. The grits, we shall suppose, are hardened and much cracked and shattered, and the shales baked and porcellanised; both rocks, when closely examined, showing traces of an incipient fusion along the line of contact with the intrusive rock. They may even lose their original granular texture, and assume a more or less crystalline aspect for some distance away from the dykes and veins that intersect them. All these features the observer may have seen exemplified in a modern volcanic district, and he may therefore feel justified in the opinion he has formed as to the formerly molten state and therefore igneous origin of our crystalline rock. His induction, however, is not complete. He compares his supposed igneous rock with the undoubted products of existing volcanoes, and although many of these last send out dykes and veins, and have a crystalline texture, yet not a single one may have any further resemblance to the crystalline rock of his section. He cannot, therefore, be any longer certain that his dykes and veins have originated in the same way as those of Etna and Vesuvius. The origin of such a crystalline rock as I am speaking of (which we may suppose is granite), cannot be determined, like that of conglomerate or sandstone, by direct comparison with similar rocks in process of formation. Exhaustive examination of the granite itself, an intimate knowledge of its ingredients, and the conditions of formation which these imply, combined with careful observation of the mode in which this rock occurs wherever it is met with—these and other studies must be prosecuted before any assurance can be obtained as to the precise mode in which granitic dykes and veins have originated. The observer then learns that these are really of igneous origin, as he at first inferred, but his notion that they have been injected into strata at or near the surface like the dykes of modern volcanoes, cannot, he finds, be maintained. All the evidence supplied by careful microscopical examination and physical considerations, leads to the conclusion that granite has been formed and consolidated at considerable depths. Having satisfied himself upon this point, the observer will readily conclude that the dykes and veins that now appear at the surface were formerly buried under great masses of rock, which have since been removed. Of course there are many other facts connected with the history of granite which I do not touch upon at present. By and by we shall learn that all masses of granite are not intrusive, but that certain considerable areas of this rock, although agreeing in composition with the granite of dykes and veins, are nevertheless not irruptive.

The conclusion that granite is of deep-seated origin is not, you will observe, contrary to our canon that the past is to be interpreted by the present. Molten rock, as we know, is forced into fissures in the neighbourhood of active volcanoes, and there consolidated, and chemical analyses show that some volcanic rocks have the same ultimate composition as granite. Partly by observation and partly by experiment we detect in granite evidence to prove that it has consolidated under pressure, and that, had the original molten mass cooled more rapidly and under moderate pressure, the resulting rock would have presented a very different appearance. Had injection taken place at or near the surface, or had the melted matter flowed out of a volcanic orifice, it might well have resembled some of the products of modern volcanoes.

Let us now take another sample of the mode of interpreting geological phenomena. We shall go back to our section of conglomerate, sandstone, and shale—and these deposits we shall suppose belong to a comparatively recent date—to the Tertiary period, let us say. Suppose, moreover, that the fossils are numerous, and so well preserved that we are enabled to compare them with living forms. A few, we find, belong to existing species, others are closely related to these, while yet others, although without doubt extinct, can nevertheless be referred with confidence to living genera. These facts enable us to come to a trustworthy conclusion as to former climatic conditions, for all we have to do is to examine the conditions under which the existing species presently flourish, and draw the obvious inference. Of course the larger the number of living species, and the more highly organised these are, the more reliable our theoretical results will be. But suppose our fossils indicate a warm and genial climate, and that the locality in which we discover our section lies far within the Arctic Circle—what must our conclusion be? Simply this: that the climate of those high latitudes was formerly much warmer than it is now. We appeal to the present, and that is the reply we get. But the next question arises: How could such a climate obtain within the Arctic Circle? This is one of those crucial cases which must eventually determine whether Uniformitarians are justified in maintaining that the present is the exact type of all that has gone before, within known geological periods. According to them it is not necessary to look beyond this earth itself for an explanation of such an apparent anomaly as the occurrence of southern faunas and floras in the Arctic regions. All we have to assume, they tell us, is a former very different distribution of land and water. They refer us to the well-proved fact that there have been frequent considerable elevations and depressions of the land, which must have indirectly affected the climate of wide areas by modifying the course of oceanic and aerial currents. They argue that were the larger land-areas of the globe to be grouped about the equator, with oceanic islands scattered over the higher latitudes, this arrangement of land would induce all the conditions that are necessary to account for the former growth of walnuts and oaks and beeches within the Arctic Circle.

This hypothesis is opposed by others who maintain that no such distribution of land and water existed at the epoch in question. According to them, the position of the main continental ridges and oceanic depressions was established at a very early period in the earth's history. The persistence of these main features, however, does not imply a total invariability of outline. On the contrary, the protuberant areas, it is admitted, have been modified again and again all through the geological ages—considerable portions having been alternately depressed below and lifted above the sea-level. But as the relative positions of the more important ridges and depressions of the earth's surface—the continental areas and oceanic basins—were determined long anterior to the deposition of the Tertiary strata, and probably date back to azoic times—such a re-arrangement of land and sea as the Uniformitarian view requires cannot have taken place. It is further maintained that, even could such a re-arragement be substituted for the present, it would not bring about a genial climate in the Arctic regions. We must look beyond our globe itself, we are told, if we wish to find the key to those greater revolutions of climate of which we have evidence in such a case as the occurrence of a southern flora within the Arctic Circle. The greater climatic revolutions of the past are due, we are assured, to periodical changes in the eccentricity of the earth's orbit, combined with the precession of the equinoxes, and the influence which such mutations must have exerted upon the ordinary agents of geological change.

The soundness of these opposing views must of course be tested by an appeal to facts, and it will be our duty in the course of our investigations to examine all the data which have been adduced in their support. I have referred to them on this occasion merely to show you that above and beyond the more or less obvious interpretation of geological phenomena, larger questions arise, the consideration of which demands not only laborious and far-extended observation, but must call into exercise all the varied powers of the human mind.

In the initial stages of our geological investigations we are occupied in detecting the more apparent resemblances and correspondences between the present and the past. We readily discover in sedimentary strata the evidence of their accumulation by the action of water, nor do we experience much difficulty in discovering the igneous origin of many rock-masses in regions now far removed from scenes of volcanic activity. But each observation we make and every well-founded correspondence we establish between the present and the past leads on to larger and larger deductions, until, as in the case of our granitic dykes and veins, we eventually find that geological investigations frequently increase our acquaintance with forces now in action and give us some insight into the hidden operations of nature. It is not indeed too much to say that in many cases our knowledge of such operations is derived in large measure from a study of the effects produced by the work of nature in past ages. The examination, for example, of the fragmentary relics of ancient volcanoes, in this and other countries where volcanic action has long been extinct, has enabled us to picture to ourselves many details of the structure of those interior and basement parts of a volcanic mountain, which otherwise must ever have remained unknown. The long-continued action of the agents of denudation has often removed those superficial rock-masses which gather around volcanic orifices, so as to lay bare, as in a dissection, the interior and basal portions—showing us the fractured and baked strata through which the heated gases, molten matter, and loose ejectamenta were erupted, and the dykes and veins of crystalline rock which were injected into the cracks and fissures of the shattered strata. Nay, a study of those vast masses and sheets of granitic, gneissose, and schistose rocks, of which large portions of the Scottish Highlands, Scandinavia, and other countries are composed, induces the belief that these rocks originally existed as ordinary sedimentary strata, and that their present crystalline condition has been assumed at a time when they were deeply buried underneath other and of course younger strata. And thus we have hints given us as to what may be taking place now throughout extensive areas underneath the surface of the earth, where other sandstones and shales may be undergoing a gradual metamorphism and conversion into crystalline rocks.

(*To be continued.*)

THE SENSES OF BEES

AT the meeting of the Linnean Society on Thursday last, Sir John Lubbock read an account of his further observations on the habits of insects, made during the past year. The two queen ants which have lived with him since 1874, and which are now, therefore, no less than eight years old, are still alive, and laid eggs last summer as usual. His oldest workers are seven years old. Dr. Müller, in a recent review, had courteously criticised his experiments on the colour sense of bees, but Sir John Lubbock pointed out that he had anticipated the objections suggested by Dr. Muller, and had guarded against the supposed source of error. The difference was, moreover, not one of principle, nor does Dr. Muller question the main conclusions arrived at, or doubt the preference of bees for blue, which indeed is strongly indicated by his own observations on flowers. Sir John also recorded some further experiments with a reference to the power of hearing. Some bees were trained to come to honey which was placed on a musical box on the lawn close to a window. The musical box was kept going for several hours a day for a fortnight. It was then brought into the house and placed out of sight, but at the open window and only about seven yards from where it had been before. The bees, however, did not find the honey, though when it was once shown them, they came to it readily enough. Other experiments with a microphone were without results. Every one knows that bees when swarming are popularly, and have been ever since the time of Aristotle, supposed to be influenced by clanging kettles, &c. Experienced apiarists are now disposed to doubt whether the noise has really any effect, but Sir John suggests that even if it has, with reference to which he expressed no opinion, it is possible that what the bees hear are not the loud low sounds, but the higher overtones at the verge of, or beyond our range of hearing. As regards the industry of wasps, he timed a bee and a wasp, for each of which he provided a store of honey, and he found that the wasp began earlier in the morning (at 4 a.m.), worked on later in the day. He did not, however, quote this as proving greater industry on the part of the wasp, as it might be that they are less sensitive to cold. Moreover, though the bee's proboscis is admirably adapted to extract honey from tubular flowers, when the honey is exposed, as in this case, the wasp appears able to swallow it more rapidly. This particular wasp began work at four in the morning, and went on without any rest or intermission till a quarter to eight in the evening, during which time she paid Sir John 116 visits.

INVERTEBRATE CASTS VERSUS ALGÆ IN PALÆOZOIC STRATA

THE distinguished Swedish geologist, Dr. A. Nathorst, having made numerous experiments, has come to the conclusion that invertebrate animals, when creeping over a soft sea-bottom, will leave imprints which are identical with the markings which have hitherto been considered those of fossil Algæ. If these Algæ are examined, it will be found, he states, that the appearance of a great many of them indicate that they have not been organisms at all, but formed in some mechanical way, and that analogous forms may even be found in existing species.

Dr. Nathorst considers that with the exception of three groups, the greatest number of Algæ enumerated in Mr. Schimper-Zittel's work on Palæontology as "undefined," are merely imprints of invertebrate animals.

Some time ago Prof. Martens of Berlin demonstrated that ichthyological members of the genus *Periophtalmus* which he had watched on the coast of Borneo when creeping over a clay bottom, left regular and defined impressions from their body and fins on the surface which would, if preserved, easily be mistaken for cryptogamic fossils, and in a paper on casts of Medusæ in the Cambrian strata of Sweden, Dr. Nathorst further shows that the so-called *Eophyton spatangopsis*, &c., which have been considered imprints of certain zoophytes and mollusks, are traces of Medusæ. These "fossils" are, according to his theory, either traces which Medusæ leave when carried by the motion of the water over a soft bottom (*Eophyton*), or imprints of their belly and adjacent organs when at rest. He further shows, that a more solid kind of Medusæ than the common have left traces in the calcareous slate of Central Germany, which makes it possible, in some measure, to define their relation to existing species.

Hitherto, Medusæ have only been traced back to the Jurassic period, but Dr. Nathorst shows that these organisms have existed from at least Cambrian times. The imprints which the lower organisms leave on mud or sand vary in appearance with the creeping or swimming habits of the animals, as well as with the nature of the bottom, whilst it is particularly interesting to note that certain worms produce imprints and vermiculated holes, which are exactly like the radiant Algæ, and which would not be supposed to be the work of invertebrata, if their formation had not been clearly demonstrated.

In connection herewith it should be mentioned, that imprints may also be made in a soft sea-bottom by stones which are carried along with the tide by floating sea-weeds, regarding which observations have recently been made by the Scottish naturalist, Mr. Symington Grieve. C. S.

BIOLOGY IN ITALY[1]

IN welcoming the appearance of this new journal under the editorship of Prof. Emery, of Bologna, and Prof. Mosso of Turin, it may not be amiss to mention briefly the programme of its originators. They will endeavour each year to give a classified list of all works published in Italy on biology, in its widest sense. The list for 1881, with an index of the names of authors, appears in volume I. They will try and bring together and illustrate original memoirs on subjects which treat of life in every form. In addition to these there will from time to time appear *résumés* and notices of memoirs appearing in other Italian journals; and as far as practicable the *résumés* will be drawn up by the authors of the papers abstracted. The archives will be published in French, "because this language is, without the possibility of contradiction, that one the most universally known among all the living languages."

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[1] "Archives Italiennes de Biologie." tome i., 1882. Tome ii. fasc. i., October 15, 1882. (Rome, Florence, Turin: H. Loescher.)

Most heartily do we echo the following words of the editors :—

"L'Italie a été jadis le berceau de la renaissance des arts et des sciences. D'autres nations nous ont depuis lors dépassés; mais l'unité de la patrie est venue rallumer le foyer du travail, et donner un nouvel essor aux études scientifiques, dont nous constatons chaque année les rapides progrès. Les travaux qui verront le jour dans les *Archives Italiennes de Biologie* seront, pour nôtre pays, nous l'espérons, un nouveau titre à l'estime de tous ceux qui prennent intérêt à l'avancement des sciences de la vie."

Among the chief articles in volume I., are the following :—*Physiology :* On a new element in the blood of mammals, and its importance in thrombosis and in coagulation; on the production of the red globules in extra uterine life; and on small blood discs in mammals, by G. Bizzozero; on the reproduction of the marrow in long bones; and on the regeneration of articular extremities in sub-capsular periosteal resections, by D. Bajardi; on the hæmatopoetic functions, and on the complete reproduction of the spleen, by G. Tizzoni; on hepatic glycogenesis, by Ph. Lussana; on the functions of the bladder, by A. Mosso and A. Pellacani; on the structure of the spinal cord, by J. B. Laura; on varietes in the cerebral circumvolutions in man, by C. Giacomini; critical experimental study of the cortical motor centres, by A. Marcacci; on the caducousness of the ovarial parenchyma and its total rehabilitation, by J. Paladino; origin of the olfactory tract, &c., in mammals, by C. Golgi. *Pathology :* Contribution to the pathology of the muscular tissue, by E. Perroncito; contribution to the study of endocartitis, by V. Colomiatti; contribution to the subject of intestinal cysts, by H. Marchiafava; on the discovery of the specific ferment of malaria in the blood, by the Editors. *Zoology :* On the origin of the central nervous system in annelids, by N. Kleinenberg, of Messina; on the nervous system and sense organs of *Sphæroma serratum*, by J. Bellonci; on a new genus (Distaplia) of Synascidians, by A. Della Valle; on the metamorphoses of some Insecticole Acari, by Ant. Berlese. *Botany :* On the action of ether and chloroform on the sensitive organs of plants, by C. Cugini; on the active principle of *Adonis vernalis*, by V. Cervello; contribution to the study of the genus Cora, Fr., by O. Mattirolo; researches on the anatomy of leaves, by I. Briosi.

Vol. ii. part 1, contains : On the early phenomena of development in Salpa, by F. Todaro; on the anatomy of the compound Ascidians; and on budding in the Didemnidæ and Botryllidæ, and on the enterocœlic type in the Ascidia, by A. Della Vallee; polymorphism and parthenogenesis in some Acari (Gamasidæ), by A. Berlese; on an unobserved organ in some vegetable embryos, by S. Briosi; experimental study of the cortical motor centres, by A. Marcacci; experiments on the formation of uric acid, by J. Colasanti; on the action of oxygenated water (H^2O^2) on animal organisms, by J. Colasanti and S. Capranica; on the toxic action of human saliva, by L. Griffini.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In addition to the Scholarships in Natural Science offered by Balliol and Christ Church this term, of which details have been published in NATURE, a scholarship in Natural Science will be offered for competition next term by Queen's College. Papers will be set in Chemistry, Physics, and Biology. No candidate will be expected to offer more than two of these subjects. Candidates are requested to signify their intention of standing by letter to the Provost, not later than February 1, and to state the subjects they propose to offer.

The Natural Science Scholarship at Exeter College has not been awarded. Mr. H. O. Minty, of the Royal College of Science, Dublin, has been elected to an Exhibition. Mr. Minty was *proxime* at the late examination for the Trinity Natural Science Scholarship, but being over the statutable age, was not eligible for a scholarship at Exeter College.

CAMBRIDGE.—Prof. C. C. Babington, F.R.S., Professor of Botany in the University of Cambridge, has been elected to a Professorial Fellowship at St. John's College. Prof. W. J. Sollas, F.G.S., Professor of Geology at University College, Bristol, has also been elected Fellow of St. John's College.

THE number of students at Dorpat University is vastly increasing from year to year. While in 1867 the number was only 573, it reached 728 in 1872, 858 in 1877, 1105 in 1880, and now stands at 1367 students.

SCIENTIFIC SERIALS

Journal of the Royal Microscopical Society for August, 1882, contains : On some micro-organisms from rain-water, ice, and hail, by Dr. R. L. Maddox.—On the relation of aperture and power in the microscope, by Prof Abbe.—Description of a simple plan of imbedding tissues for microtome cutting in semi-pulped unglazed printing paper, by B. W. M. Richardson.—Note on Rev. G. L. Mills' paper on diatoms in Peruvian guano, by F. Kitton.—The usual summary of current researches relating to zoology and botany (principally invertebrate and cryptogamia), microscopy, &c., including original communications from Fellows and others.—Proceedings of the Society.

THE same journal for October, 1882, contains : On plant crystals, by Dr. Aser Poli (plate 6), and the summary of current researches relating to zoology and botany (principally invertebrata and cryptogamia), microscopy, &c., including original communications from Fellows and others.

The Quarterly Journal of Microscopical Science, No. 87, for July, 1882, contains :—Note on the formation of fibrine, by Mrs. Ernest Hart (plate 21).—On the genesis of the egg in Triton, by T. Iwakawa (plates 22-24).—On the germination and embryogeny of *Gnetum gnemon*, by F. O. Bower (plate 25).—The organ of Jacobson in the dog, by Dr. E. Klein (plate 26).—On Saprolegnia in relation to the salmon disease, by Prof. Huxley.—Notes on certain methods of cutting and mounting microscopical sections, and on the central duct of the Nephridium of the leech.

No. 88, for October, 1882, contains : On the development of *Ostrea edulis*, by Dr. R. Horst (plate 27).—The morphology and life-history of a tropical Pyrenomycete, by H. Marshall Ward (plates 28 and 29).—The thread cells and epidermis of Myxine, by R. Blomfield (plate 30).—The eye of Spondylus, by Sydney J. Hickson.—Note on open communication between the cells in the pulvinus of *Mimosa pudica*, by W. Gardiner.—Notes on the development of Mollusca, by Prof. Haddon.—Note on Echinoderm morphology, by P. Herbert Carpenter (woodcuts).—On the vertebration of the tail of Appendiculariæ, by Prof. Lankester.—Notes on the structure of Seriatopora, Pocillopora, Corallium, and Tubipora, by Prof. Moseley (woodcut).—Note on pacinian corpuscles, by Dr. V. Harris.—Reviews of Strasburger's structure and growth of the cell wall, and of Bergh's researches on the cilio-flagellata.

Proceedings of the Royal Society of Tasmania for 1880, contains :—Algæ of the New Hebrides, by Dr. Sonder, contains new species of Sarcodia, Caulerpa, and Chætomorpha.—On some Australian slugs, by Prof. R. Tate.—On the Unios of the Launceston Tertiary basin, by R. Etheridge, jun. (with a plate).—On a fossil helix, by R. M. Johnston (with a plate).—The lichens of Queensland, by F. M. Bailey.—On some fossil leaves and fruits, by Dr. C. E. Bernard.—On some introduced plants, by Rev. G. E. Tenison Woods.—On some new species of fish, by R. M. Johnston.—On oyster culture, by Capt. Stanley, R.N.

Bulletin de la Soc. Imp. des Naturalistes de Moscou, 1881, No. 4, contains : On new species of European Mints, by M. Gandoger.—On the Amphibia and Reptiles of Greece, by Dr. J. v. Bedriaga.—On new species of Hemiptera from the Aral and Caspian districts, by V. Jakovlev (in Russian, but the diagnoses of the new genera and species are given in German).—Catalogue of the Lepidoptera of the Moscow district, by L. Albrecht (Supplements Dr. E. Assmus's catalogue of 1858, and raising the number of species from 675 to 1172.—On new Lepidoptera from the Amur Land, by H. Christoph.—Meteorological observations (Moscow) for 1881, by J. Weinberg.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, November 2.—F. A. Abel, F.R.S., vice-president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting (November 16).—The following papers were read :—On dihydroxybenzoic acids and iodosalicylic acids, by Dr. A. K. Miller. The author has succeeded in preparing the sixth dihydroxybenzoic acid; five being already known. It was obtained by heating salicylic acid and iodine in alcoholic solution, two iodosalicylic acids were formed, which yielded two distinct dihydroxybenzoic acids when heated with potash.—On crystalline molecular compounds of naphthalene and benzene with antimony trichloride, by Watson Smith and G. W. Davis. By melting three parts by weight of antimony trichloride with two of naphthalene, minute crystals were obtained, $3SbCl_3$, $2C_{10}H_8$; similarly with benzene, a body, $3SbCl_3$, $2C_6H_6$, was prepared.—Additional evidence, by an analysis of the quinoline molecule, that this base belongs to the aromatic series of organic substances, by Watson Smith and G. W. Davis. The authors have studied the effect of exhaustive perchlorination (by heating with antimony pentachloride) on quinolin; perchlorethane, perchlorbenzene, and nitrogen were obtained.—On orcin and some of the other dioxytoluols, by R. H. C. Nevile and Dr. A. Winther. The authors have prepared the dioxytoluol 1 . 3 . 5, starting from the dinitrotoluol 1 . 3 . 5, and have found it to be identical in all its reactions and physical properties with orcin. They have also prepared the dioxytoluols 1 . 2 . 4 and 1 . 2 . 5, and have investigated the preparation of the body 1 . 3 . 4.—On the varying quantities of malt albumenoids extracted by waters of different types, by E. R. Moritz and A. Hartley.—On the derivatives of ethylene-chlor-bromide, by J. W. James. The author gives details as to the preparation of this body, and has studied the action of sodium sulphite upon ethylene chlorobromide, ethylene dibromide, and ethylene chlorothiocyanate; also the action of ammonia upon an ethereal solution of chlorethylsulphonic chloride.

Paris

Academy of Sciences, October 30.—M. Blanchard in the chair.—The following papers were read:—Remarks on the theory of shocks, by M. Resal.—Results of experiments made at the Exhibition of Electricity on machines and regulators with continuous current, by MM. Allard, Joubert, Le Blanc, Potier, and Tresca. Thirteen different combinations are dealt with, and data regarding mechanical work, electric resistance, intensity, luminous power, economical efficiency, &c., tabulated. Another paper, to appear soon, will treat of other systems. In nearly all the experiments the total motor work is very well represented by the corresponding electric work.—Rational conception of the nature and propagation of electricity (continued), by M. Ledieu. Electricity is, no more than heat or light, to be regarded as a special agent under particular mechanical laws. As to the phenomenal cause, it is simply the potential energy of the ether associated with the ponderable matter, especially in the form of atmospheres round the molecules. It has for counterpart the portion of potential energy of the ponderable matter, which constitutes chiefly *latent heat*.—On the efficacy of lightning conductors, by M. Hirn. A very faulty conductor may *sometimes* protect a house. One such near Colmar, on a house 15m. high, consisted (in descending order) of a conical brass point, an iron rod about 8m. long, on which this was screwed, and a wire, hardly 0·007m. diameter, in pieces with terminal rings, passing down to a piece of iron 0·5m. long in a hole in the moist ground. In a violent storm (the thunder of which brought down plaster from ceilings), the rod was struck, and the brass cone fused, but no part of the current left the conductor. During over forty years' observations, M. Hirn has never seen lightning strike any of the forty or fifty lightning rods on the works of Logelbach. Yet, during a thunderstorm, these rods work actively; as he has proved by means of derived circuits from the uninterrupted conductors, yielding currents with magnetising power. He has even drawn currents from a conductor separated by a thin leaf of caoutchouc; the thin copper wire was never fused.—Application of the law of complementary colours to temporary decoloration of diamonds tinted yellow, by MM. Chatrian and Jacobs. The yellow diamond is merely put in a solution of the complementary colour (violet), and it comes out white; but mere washing brings back the yellow.—Chemical studies on the sugar beet called the white beet of Silesia, by M. Leplay.—On certain quadratic forms, and on some discontinuous groups, by M. Picard.—On trigonometric series, by M. Poincaré.—Reply to M. Faye's objections to Dr. Siemens' theory of the sun, by Dr. Siemens.—On an extension of the principles of areas and of movement of the centre of gravity, by M. Lévy.—On the longitudinal vibrations of elastic rods, and the motion of a rod carrying at its end an additional mass, by MM. Sébert and Hugoniot.—New expressions of the work and economic efficiency of electric motors, by M. Deprez.—On a modification required in enunciation of the law of isomorphism, by M. Klein. In the second part of the law, stating that isomorphous bodies have a similar chemical composition, it is necessary to say, instead, that they have either a similar chemical composition, or present a centesimal composition slightly different, while containing a group of elements that are common or of identical chemical functions, and which form much the largest part of them by weight.—Researches on the thorite of Arendal, by M. Nilson.—Rapid process of determination of salicylic acid in beverages, by M. Rémont.—Distribution of ammonia in the air and aqueous meteors at great altitudes, by MM. Muntz and Aubin. On the Pic du Midi (2877 m.), the quantity of ammonia in the air was much the same as on low ground (or 1·35 mgr. per 100 cub. m.); that in rain water considerably less; also that in snow and in mist.—New chemical and physiological researches on some organic liquids (water of sea-urchins, water of hydatic cysts and cysticerci, amniotic liquid), by MM. Mourson and Schlagdenhauffen.—On the evolution of Peridinians and the peculiarities of organisation connecting them with Noctilucæ, by M. Pouchet.—Hypsometric map of Turkey in Asia, published at Tiflis, under direction of General Stebnitzky. Previous maps are shown to need correction in orography.—Action of oil on sea-waves, by M. Virlet d'Aoust. An experience of his in Greece in 1830 shows that the method was practised by seamen there. He also notes the calming effect of petroleum rising in the bed of a Mexican river, and carried into the sea.—On the cultivation of opium in Zambesia, by M. Guyot. This was begun in 1879 at Chaima, near Niopea, about 6 km. from the Zambesi. In 1881 it engaged 300 workers, 250 of whom were blacks and 50 natives of India. In India the opium sells for 50 to 60 francs the kilogramme.

Göttingen

Royal Society of Sciences, June 10.—On the occurrence of cleistogamous flowers in the family of the Pontederaceæ, H. Grafen zu Solms-Laubach.—On Arabian navigation, by S. Gildemeister.—On gradually developing contact-electricity with co-operation of air, by W. Holtz.—Optical studies on garnet, by C. Klein.

August 1.—On the measurement of the winding surface of a wire-coil by the galvanic method, and on the absolute resistance of the mercury-unit, by F. Kohlrausch.—On triazo compounds, by H. Hubner.—On the method proposed by M. Guébhard for representation of equipotential lines, by H. Meyer.—On the neurology of the Petromyzonts, by F. Ahlborn.

CONTENTS

PAGE
A SEARCH FOR "ATLANTIS" WITH THE MICROSCOPE. By Dr. ARCH GEIKIE, F.R.S. 25
THE LIFE OF CLERK MAXWELL 26
OUR BOOK SHELF:—
  Burmeister's "Description Physique de la République Argentine d'après des Observations Personelles et Étrangères" 28
  Scudder's "Nomenclator Zoologicus" 28
LETTERS TO THE EDITOR:—
  "Weather Forecasts"—Rev. W. CLEMENT LEY 29
  The Comet—C. J. B. WILLIAMS; W. J. MILLER; WALTER HIGGINSON and B. MANNING 29
  Two Kinds of Stamens with Different Functions in the same Flower—Dr. HERMANN MÜLLER (*With Illustrations*) 30
  A Curious Halo—Father MARC DECHEVRENS 30
  Habits of Scypho-Medusæ—Surgeon-Major H. B. GUPPY 31
  Prof. Owen on Primitive Man—GRANT ALLEN 31
  Magnetic Arrangement of Clouds—C. H. ROMANES 31
  The Umdhlebe Tree of Zululand.—H. M. C. 32
  The Weather—J. M. FOUNTAIN 32
ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE. By ANDREW GRAY (*With Diagrams*) 32
THE ITALIAN EXPLORATION OF THE MEDITERRANEAN. By Dr. J. GWYN JEFFREYS, F.R.S. 35
WIRE GUNS, II. By JAMES A. LONGRIDGE, C.E. (*With Diagrams*) . 35
BEN NEVIS OBSERVATORY. By CLEMENT R. WRAGGE 39
THE OYSTER INDUSTRY OF THE UNITED STATES 39
NOTES 40
OUR ASTRONOMICAL COLUMN:—
  Comet 1882 *b* 43
  The November Meteors 43
GEOGRAPHICAL NOTES 43
THE AIMS AND METHOD OF GEOLOGICAL INQUIRY. By Prof. JAMES GEIKIE, LL.D., F.R.S. L. and E. 44
THE SENSES OF BEES 46
INVERTEBRATE CASTS VERSUS ALGÆ IN PALÆOZOIC STRATA 46
BIOLOGY IN ITALY 46
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 47
SCIENTIFIC SERIALS 47
SOCIETIES AND ACADEMIES 47

THURSDAY, NOVEMBER 16, 1882

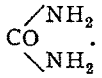
RECENT CHEMICAL SYNTHESES

DURING the Exhibition of Scientific Apparatus at South Kensington a few years ago one of the most interesting exhibits was the first specimen of Urea from Prof. Wöhler.

This substance and specimen may be said to be the first organic compound or product of a living organism built up from its mineral elementary constituents, not certainly directly, but very nearly, by an ordinary chemical operation.

The importance of this discovery, made in 1828, was not, however, recognised for many years. Its importance and signification in a physiological sense were first perceived, but its formation is the earliest and best example of an action that plays an important part, and is probably the most interesting question in modern organic chemistry, namely, what the Germans call the "Umlagerung" of the atoms in a molecule or "intermolecular change."

Urea was obtained by Wöhler by simply heating the compound ammonium cyanate, NH_4CNO, in contact with water, whereby the arrangement of the atoms is so changed that the two nitrogen atoms become directly combined to hydrogen and only indirectly to the oxygen as shown in the chemical formula:



$$CO\left<\begin{matrix}NH_2\\NH_2\end{matrix}\right. .$$

So long ago as 1773 this substance was discovered as a constituent of urine, and since then its importance as a final stage in the retrogressive metamorphosis of the animal tissues, or of albumenoids, has been pretty fully worked out and recognised.

Although albumin has not yet been directly oxidised to urea in the laboratory, the descent takes place in the animal economy through several stages, as for instance, tyrosin, kreatin, xanthin, allantoin, uric acid, &c., probably by a simultaneous oxidation and hydration, or even reduction. Several of these intermediate substances yield, however, urea as a direct product of oxidation not only when taken into the organism but when submitted to ordinary oxidising agents. Even substances like asparagine, leucine, and glycine, which are very near to albumin as products of retrogressive metamorphosis, may be considered as preliminary stages in the splitting up and oxidation of the tissue substance into more simple compounds until a truly *mineral* character is arrived at.

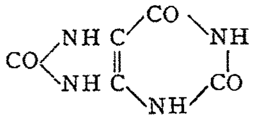
Although urea was synthesized from its mineral elements so long ago, it has until quite recently contributed comparatively little to the syntheses of the more complex members of the class of bodies of which it is almost the final oxidation product. A great number of bodies have, however, been derived from urea by substitution. Probably the most important synthesis obtained by the aid of this body since Wöhler prepared it from its mineral constituents, is the one just announced as having been made by Dr. Horbaczewski in the Vienna Chemical Institute.

This chemist has succeeded in proceeding a step backwards from urea to uric or lithic acid. The method employed, of which the details have been sent to us by Dr. Horbaczewski, is by heating urea with glycocoll at a temperature of 200°–230° C. in a metallic bath until the mass fuses and becomes brown and friable.

Glycocoll is amido acetic acid, $CH_2 . NH_2 . COOH$, and the reaction which takes place may perhaps be represented:—

$$CO{}^{NH_2}_{NH_2} + (CH_2\,NH_2\,COOH)_2 = C_5N_4H_4O_3 + OH_2 + H_6.$$

The action as represented by this equation indicating conditions the reverse of those supposed to exist when uric acid is converted to urea. As to the structure of the group $C_5H_4N_4O_3$ the simplest view is that of Medicus—



It is somewhat remarkable that this reaction and synthesis has not been attempted or attained earlier, for the converse reaction, represented by the equation—

$$C_5H_4N_4O_3 + 5OH_2 = CH_2.NH_2.COOH + 3CO_2 + 3NH_3$$

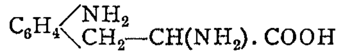
has been known for a considerable time.

The same two substances have previously served as materials for an important synthesis, namely, that of hydantoïn or hydantoic acid (*Ber. Ber.*, p. 36).

This synthesis is mostly important as giving a step backwards towards that very complicated atomic group termed albumen.

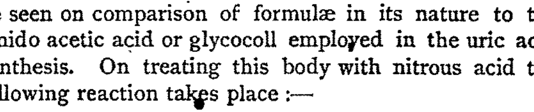
That this will eventually be arrived at is exceedingly probable, and in the near future, for an even more complicated substance than uric acid has also been built up and its structure or intermolecular constitution settled very conclusively by the method of synthesis employed by Erlenmeyer and Lipp. This substance is tyrosine, a product of the decomposition of albumen in the animal system, and also by putrefactive decomposition and by heating with alkalies or acids.

The method is somewhat more complicated than the one employed by Dr. Horbaczewski. Starting with phenethyl-aldehyde they proceeded by conversion into phenylalanin and nitration to the amide compound—



$$C_6H_4\left<\begin{matrix}NH_2\\CH_2\text{—}CH(NH_2).COOH\end{matrix}\right.$$

paranitrophenylalanine, a substance very similar, as will be seen on comparison of formulæ in its nature to the amido acetic acid or glycocoll employed in the uric acid synthesis. On treating this body with nitrous acid the following reaction takes place:—



$$C_6H^4CH_2 : CH . NH_2 . \overset{NH_2 + NO^2H}{COOH} = H_2O + N_2 + C_6H_4\left<\begin{matrix}OH\\CH_2 . CH . NH_2 . COOH.\end{matrix}\right.$$

According to this method of building up, tyrosine is a para-hydroxy phenyl *a* alanine.

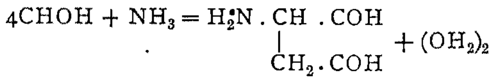
Both reactions are similar in this respect: the end is attained by the splitting away of hydrogen from nitrogen groups NH_2 partly in the form of water.

All these syntheses are really approaches to that of albumen, and in this connection some work lately done and published in brochure form by MM. Loew and Bokorny of Munich gains in importance.

These investigators have proved the presence of aldehyde groups in living plasma, and are of opinion that albumen is a product of the condensation of a relatively simply-constituted molecular group.

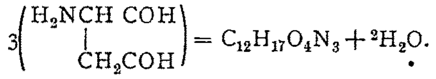
These simple groups are what are termed aldehydic groups and ammonia or amide groups.

Their idea is that a group *CHOH*, which, however, has not yet been isolated, combines and condenses somewhat as shown :—



$$4CHOH + NH_3 = \begin{array}{l} H_2N \,.\, CH \,.\, COH \\ \qquad\quad | \\ \qquad\; CH_2 \,.\, COH \end{array} + (OH_2)_2$$

This more complex group acting again as an independent individual and yielding a still more complex body, $C_{12}H_{17}N_3O_4$, with expulsion of water.



$$3\left(\begin{array}{l} H_2NCH\ COH \\ \qquad\;\; | \\ \qquad CH_2COH \end{array}\right) = C_{12}H_{17}O_4N_3 + {}^2H_2O.$$

A further similar condensation under conditions where it could take up additional hydrogen and some sulphur, conditions easily attainable in living organisms, yield albumen direct :—

$$6 \,.\, (C_{12}H_{17}N_3O_4) + SH_2 + {}^6H_2 = \\ C_{72}H_{112}N_{18}S \,.\, O_{22} + {}^2H_2O.$$

This is the formula for albumen, assuming sulphur as an essential constituent. A simpler would be $C_{72}H_{114}N_{18}O_{24}$, and would be a direct product of such condensation.

Both in the fall of this complicated molecule through less and less complicated groupings of atoms to the so-called mineral groups into which they are finally resolved, and in the so far only partial building-up process, the peculiar aptitude of certain elementary substances to combine into very stable groups or individuals is well shown. Carbon and nitrogen compounds exhibit this *par excellence*, but there is no reason to suppose that such a property is confined to them alone. It may be that the range of existence of these compounds are more within our reach than in the case of other so-called elements.

As elements imprint generally their most characteristic property on the compounds they form, it is perhaps not unreasonable to suppose that these elements whose compounds we see so readily group up or polymerise, may themselves be also, in the condition in which we take them to be elementary, in a state of great atomic complexity.

THE BUTTERFLIES OF INDIA

The Butterflies of India, Burmah, and Ceylon. A descriptive Handbook of all the known Species of Rhopalocerous Lepidoptera inhabiting that Region, with Notices of Allied Species occurring in the Neighbouring Countries along the Border; with Numerous Illustrations. By Major G. F. L. Marshall, R.E., and L. de Nicéville. Part I. Royal 8vo. (Calcutta, 1882.)

THE first part of this anxiously-expected book, by Major Marshall and Mr. de Nicéville, has just arrived, and will, I am sure, be gladly welcomed, not only by the naturalists *in esse* of Europe, but by a great number of naturalists *in posse* of our Indian Empire.

For though, thanks to the labours of Hodgson, Blyth, Jerdon, Hume, Blanford, Godwin-Austen, Day, Theobald, and many others, we have excellent handbooks, and a very fairly complete knowledge of the mammals, birds, reptiles, fishes, and land shells of India, we have absolutely nothing to assist the entomologist or collector in identifying and studying the lepidoptera. How many weary hours of hot weather on the plains, how many dreary evenings in camp, and tedious marches in mountains and forests will be made interesting and profitable by this book no one but residents in India have any idea, but I feel sure that its appearance will give such an impulse to the collection and study of the lepidoptera of India that in ten years we shall have as many working entomologists in India as we have had ornithologists, since the publication of Jerdon's Birds of India.

Considering the magnitude of the work, and the many risks and chances of life in India, it is specially fortunate that the work has been undertaken by two gentlemen, of whom one is already known as an ornithologist of repute, and both of whom have excellent opportunities for bringing together the immense amount of material necessary to bring the work to a conclusion.

The little we know at present of the butterflies of India is gathered from the scattered descriptions of Indian species by old authors and from the numerous papers and descriptions in various publications by a few modern entomologists, of whom Mr. F. Moore holds by far the most distinguished place. Unfortunately, however, many of these papers are of a bare and misleading character, and so far from making the work of discriminating the species easier, only confuse it.

The carelessness which has been shown by some writers about the habitat and distribution of species, and about their allied forms, is deplorable in many ways, and shows an entire want of appreciation of the physical geography of India, and of the vastly different zoological regions which it includes; but new light is sure to be thrown on the subject by men who understand and appreciate these facts, and who have personal and local knowledge of the country whose insects they describe. The form of the work, which is printed and published by the Calcutta Central Press Company, 5, Council-House Street, Calcutta, is a large octavo; both print and paper are good, and likely to stand the hard wear to which no doubt the work will be subjected. The price is not mentioned in the first part, but will no doubt depend on the number of illustrations which are found necessary. These are of three kinds, viz. chromolithographs, of new and remarkable species by West, Newman, and Co., London, of which one appears as frontispiece, and is very superior to some illustrations of a similar character; autotypes, by the Autotype Company of London, of which nine are given in the first part, illustrating dissections, typical larvæ, and pupæ, and fifteen species of Danainæ; these are well executed, and suitable to their purpose, though perhaps they will hardly be suitable to illustrate the Lycenidæ. The woodcuts, by George Pearson, of which three are given with the text, are not quite so good, but will serve their purpose very fairly. The illustrations are drawn by Babu Behari Lall Dass, and Babu Cris Chunder Chuckerbutty, of Calcutta, under the superintendence of Mr. Wood Mason, and seem to be faithful to nature, as the drawings of good native artists generally are.

The preface and introduction show that the authors thoroughly appreciate the difficulties before them, and are determined to spare no pains to make their work as useful as possible; and though they have, from their inability to examine the types, been obliged temporarily to adopt many species about which they evidently have grave doubts, yet a new edition will no doubt enable these supposed species to be relegated to their proper position. The authors' opinion on this important question may be quoted as follows:—

> "With regard to species and varieties we have found it convenient to describe where there is any room for doubt under its own distinctive name, every form that has been separately characterised, the question whether any particular form represents a species or a variety of a species can at present be decided in this country only as a matter of conjecture; for a knowledge of the life-history in all its stages is essential to the authoritative settlement of such questions; at the same time the evidently or apparently allied species are carefully grouped together, and the nature of the variety is indicated as closely as our present knowledge will allow."

With regard to the scope of the work we may again quote the preface as follows:—

> "This book does not attempt a life-history of each or any of the insects. The time has not arrived for such a work. The details required for a life-history cannot be gathered until a knowledge of the nomenclature is far more widely diffused. It is simply designed as a handbook of reference, as complete as possible in itself, for the convenience of naturalists in the field, who have no access to libraries. Where necessary full extracts from the works not generally available are given, and where possible and advisable the description of the species are given in the words of the original describers, supplemented by any further details necessary to complete them. For the genera the admirable descriptions by Westwood in the 'Genera of Diurnal Lepidoptera' have been followed as closely as possible.
>
> "The book will comprise detailed descriptions of every genus and species known to occur within the limits of India, British Burmah, and Ceylon, and short descriptions will be added in smaller type of species from neighbouring countries on the border, such as Malacca, Siam, Yunnan, Tibet, South Turkestan, Afghanistan, and Beluchistan, which, though not yet recorded from within Indian limits, may very probably subsequently be found to occur within our border."

If the authors mean to follow out this course it is to be hoped that their descriptions will be of a comparative and not of a general nature. Nothing can be more laborious, more unsatisfactory, and often more useless than wading through long descriptions, when a few words indicating in what character the species in question differs from its nearest allies, are often far more useful. It is just because authors have in many cases been unwilling or unable to make this comparison that they have described species without good cause, and it is frequently found that when such comparison is attempted, the want of distinctive characters is shown at once, whereas in a long wordy description it may easily be concealed. In conclusion, we wish the book success, and plenty of supporters, so that it may be completed quickly, and mark the commencement of a new era in Indian entomology.

H. J. Elwes

OUR BOOK SHELF

Winners in Life's Race; or the Great Backboned Family. By Arabella Buckley, Author of *Life and her Children*, &c. With Numerous Illustrations. (London: Edward Stanford, 1882.)

Life, the title of Miss Buckley's thoughtful work now before us would suggest, once it became materially existent, went ever forward, striving after diverse fashions to adapt her children to the best methods of fighting and winning. She felt her way onward in several directions, and in several of these she attained to a fair share of perfection, from shapelessness to symmetry, from a simpleness in structure to a wonderful differentiation thereof; from a mere manifestation of vitality to a high state of instinct, almost of intellect; but there was to all of these a limit all too speedily attained—and it is now plain that no arrangement of epidermis, or muscle, or nerve, no alteration of blood, or alimentary system could get the uppermost in the struggle. It was only with the appearance of a quite new structure—the back-bone of this volume—that Life felt she had acquired a new power, and those of her children who were thus endowed went on gallantly until, Winners in the race, they were left without a rival. The record of their humble beginning was still very incomplete but a few years ago, and there was no clue thereto. Now as the reader will learn in the clearest manner from chapter I., we know of such forms as the Lancelet, and those strange Ascidia who "once tried to be backboned, and yet as they grew fell back into the lap of Invertebrates."

Commencing with these Ascidia, this new volume of Miss Buckley proceeds to tell of those "Winners in Life's Race," which are supposed to culminate in our very selves. It does this in a way that most young people and every fairly educated person can understand as well as with a carefulness in detail and a caution in the statement of facts, most pleasing and grateful to the advanced student of Nature. Ably as this little volume is written, and admirable as, in our mind, is the judgment shown in the selection of details, yet it hardly comes to us with that captivating freshness that made the author's story of "Life and Her Children" so welcome. Why this is so, we can scarcely suggest; but this record of the battle over, of the fight won, seems to have been the result of a more tiresome labour than the author's previously published records of those other legions which led on so steadily to what was but a forlorn hope. Perhaps this is because there is a wondrous charm surrounding the mysterious beginnings of life which is not felt in the same degree as we approach the consideration of those beings who would seem to be the final product of life's genesis. Still, nothing that we thus write about the contrast between these volumes can lead us for a moment to overlook the fact that we know of no book in our language, which for the general reader approaches this, as an introduction to those animals (fish, reptiles, birds, and mammals) to whom the victory in life's race has been vouchsafed.

E. P. W.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Weather Forecasts

I am glad that my letter on this subject has been the means of eliciting the letter of the Rev. W. Clement Ley, printed in your number of November 9. I have also received more than one private communication; and by the courtesy of Mr. Scott I have been permitted to see all the evidence received at the Meteorological Office on the day preceding the great storm of October 24.

The painful conclusion is forced upon my mind that some of the difficulties which lie in the way of a surer and safer forecast of dangerous storms might be removed by a simple increase of expenditure upon the machinery available for meteorological purposes.

Mr. Ley writes: "On the whole, to the minds of many students of the subject it will appear rather strange that the Office, *with the materials at its disposal* [the italics are used by Mr. Ley] does not more often fail to furnish satisfactory warnings of the more serious of our gales. It is easy to say, in view of occasional failures, 'the system itself must be at fault;' it is still easier to reply 'Better it!' If the country cares enough for the welfare of 'fishermen and others' to do so, let it provide the necessary funds for a system of night telegrams, and, if possible, for a system of oceanic stations. If it does not, it must be content with things as they are."

Again, a private correspondent writes to me:—"The weather cannot be treated as though it went to bed at night and tucked itself in under a blanket of cumulus. It unfortunately does nothing of the kind; and while the director and his subordinates are quietly sleeping, atmospheric changes are going on with a rapidity which a constant influx of telegrams must afford the only means of meeting. Yet still in spite of all these we go on, satisfied with having only *two* reports sent in every twenty-four hours. The result is that every now and then a disastrous failure such as that of Tuesday *must* occur."

It must seem, therefore, that without the important assistance of a new class of observations, dependent upon the motions of the higher clouds, much might be done by such an extension of an existing system as common sense seems to demand. At present we appear to be acting upon a method somewhat parallel to that which would be adopted upon our railways, if the companies should send their signalmen to bed at 8 p.m., but with the night-traffic all the same. Collisions of the first magnitude would probably abound under such a system.

I write not as a meteorologist, but as a citizen. Surely if the position of things be such as has been described, and if an important improvement in the forecasting of storms could be ensured by the expenditure of a somewhat larger sum of public money, there could be no difficulty in bringing the matter in such a way under the notice of the Government as to secure the necessary funds.

H. CARLISLE

Rose Castle, November 13

The Comet

IT may perhaps interest you to know that a most brilliant comet has been visible here for about three weeks. I saw it for the first time on the morning of September 29; at 4.40 a.m. of that day it bore from a house on the ridge overlooking Victoria E. ¼ N. true (nearly), the nucleus being then about three degrees above the horizon; an imaginary line drawn from Rigel through Sirius met the nucleus.

The approximate length of the tail was nearly equal to the distance between Rigel and Betilguex, and its greatest breadth nearly equal to the distance between the two outer stars in the belt of Orion. The tail appeared brighter on the southern than on the northern side.

The following particulars, which may also prove of interest, were communicated to me by Capt. Metcalf, of the White Star Company's steam ship *Oceanic*.

"Monday, September 25—observed a large comet rise about 4.30 a.m., position at the time lat. 30° 18′ N., long. 128° 40′ E. September 26, at 5h. 17m. a.m., apparent time at ship (September 25, 9h. 01m. 13s. G.M.T.), altitude of comet, 7° 20′, distance from Sirius 63° 21′, tail extending nearly in a line from sun to Orion's belt., lat. 27° 52′ N., long. 124° 10′ E. September 27 (at 9h. 02m. G.M.T. September 26), comet's distance from Sirius 62° 32′. At 5h. 32 a.m., altitude of comet 13° 22′, bearing true S. 80° E., lat. 25° 16′ N., long. 119° 56′ E., tail about 7° to 8° long. Comet rose bearing S. 86° E. (true). September 28—distance, from Sirius 61° 49′. October 3 (in the Victoria Harbour)—distance of comet from Sirius 58° 43′."

As the comet is still visible I may possibly be able to give you some further information about it by next mail.

The following extract is from the *China Mail* of the 7th inst.:—

"A Melbourne despatch, dated September 16, says—"The comet is now extremely bright, being visible through the telescope at noon, a circumstance unprecedented in the experience of the officers of the observatory."

If it is not trespassing too much on your time I should be very much obliged if you would kindly inform me what observations would be useful (which could be taken by an ordinary sextant) should we be visited by another comet.

In conclusion I may add that for the past week the weather has been unusually hot, and although the barometer has fluctuated considerably, no atmospheric disturbance has taken place.

According to M. Dechevrens, S.J., Director of the Zi-ka-wei Observatory, no less than *twenty* typhoons visited the China and Japan seas last year, but up to date of the present year only *three* have been reported.

J. P. MCEWEN, R.N.,
Assistant Harbour Master

Hong Kong, October 9

I THINK there must have been some mistake about Major J. Herschel's observation, as recorded in NATURE, vol. xxvii. p. 5. As other observers have shown, the comet appears quite bright in moonlight. On the morning following his observation I was perhaps as much astonished as he was, only in the opposite direction; for I was very much surprised to find then (the 31st ult., at 5 a.m.), that the tail was longer than on any other occasion when I have seen it, viz. 33°. The following observations will also show the brightness of the comet in moonlight and twilight. On the 26th, at 5.25 a.m., nine hours before full moon, and in brightish twilight, the tail was visible through fog and thin cloud to a distance of 13½°. On the 29th, at 5.37 a.m., it was fully 23° long. This morning, at 6.9 a.m., in bright twilight, it was very faint, but still above 18° long. I think Major Herschel cannot have looked low enough down, or his view must have been otherwise obstructed.

The wisp, or horn, that he represents on the 23rd, was certainly a very striking feature at that time. Though not exactly like the drawing in vol. xxvi. p. 622, it was nevertheless very definite, and part of it was brighter than the adjacent part of the comet between it and the head. There appeared that morning to be also two other knots of light, much less conspicuous—one almost a continuation of the "horn," following it, and a little further north; the other, in the *n* branch of the "fish-tail."

Sunderland, November 7 T. W. BACKHOUSE

P.S. November 8.—The "horn" was this morning still a marked feature of the comet, though much less definite than formerly. Its origin (at the point where it begins from the northern branch) is still brighter than the neighbouring portions of the tail for a considerable distance in all directions. It occurs to me that this comet offers a very favourable opportunity of testing theories of the motions of tails; its features are so definite that if careful observations are, and have been, made of the positions of different points, they must throw much light on the subject.

T. W. B.

ON the 10th inst., at 5 a.m., the length of the nucleus was 110″, its breadth 12″, and its position angle 112°·5. The length seems fast increasing judging from previous measures. The tail was still about 15° long, but the first glimpse of daylight completely masked it.

GEO. M. SEABROKE

Temple Observatory, Rugby, November 14

THE great comet was again a magnificent object in our south-eastern sky, at 5.10 this morning. The startlingly sharp definition of October 23 had given place to a softer outline; but the apparent length of the tail was at least as great, and the nucleus surprisingly bright, with a distinct scintillation, even in comparison with two near stars. Major J. Herschel's experience of finding himself gazing at the comet without seeing it, in a clear sky, differs from mine, as I have on two occasions, since October 23, seen it perfectly visible, though wan, in bright moonlight, and when the sky at that elevation above the horizon was not free from haze. On October 23 I saw the strong apparent shadow spoken of by one of your correspondents, but here it was much blacker below the bright convex line of the tail than between the cleft at the end of clear definition. It did not seem to me the effect of contrast merely; but like that blackness through which the stars shone darkly in the recent southward display of aurora borealis.

HENRY CECIL

Bregner, Bournemouth, November 10

Magnetic Arrangement of Clouds

THERE is a small literature on the above subject (dating back to the time of the publication of Humboldt's *Cosmos*) which seems to have escaped the attention of Mr. Romanes. He will find a large number of observations similar to those mentioned in NATURE, vol. xxvii. p. 31, recorded in a paper in the *Phil. Mag.* for July, 1853, by Mr. W. Stevenson, of Dunse. Similar observations have been made by Mr. Birt, *Met. Mag.* January, 1876, and by several others in this country. M. André Poey also deals with the subject at some length in his work, "Comment on observe les Nuages," chap. iv.

The apparent arrangement of cirri-form clouds "round two opposite poles" is simply the optical effect of the parallelism of the belts of ice-cloud, or "cirrus-bands," as Humboldt designated them. These belts are coincident in direction with what were, at the time of the formation of the clouds, lines of equal pressure in that horizontal plane in which the clouds float; or, in other words, their direction is normal to that of the atmospheric gradient at the cirrus-level. Their position, and therefore that of their vanishing points, has never been proved to have any relation to the position of the magnetic poles. It is true that in Europe a direction coincident with the magnetic meridian is slightly more common than a direction transverse thereto. But this is explained by the fact that the formation of the bands requires somewhat steep gradients in the regions of the cirrus, and that, with us, the steepest gradients in those regions are commonly the north-eastward, being those which prevail in front of and between the cyclonic disturbances at the earth's surface, which travel towards north-east. Thus, the best defined cirrus-bands most commonly stretch from north-west to south-east.

A detailed explanation of the formation of the belts, which bears some similarity to that given by Lamarck, and which is in many, but perhaps not in all points satisfactory, will be found in a paper by Max Möller in the "Annalen der Hydrographie und Maritimen Meteorologie. Organ des Hydrographischen Amtes und der Deutschen Seewarte," 1882, heft iv. pp. 212-226.

The attempts which have been frequently made to apply the terms "polarisation," "polar bands," &c., to the cirrus belts have proved unsuccessful, and will not, it is to be hoped, be renewed.

W. CLEMENT LEY

November 11

"A Curious Halo"

THE phenomenon described in NATURE (vol. xxvi. pp. 268, 293, xxvii. p. 30) is far from being unknown in Europe, where it generally receives the title of "*Rayons du Crépuscule*"; although I do not think that it ever presents the brilliant appearance described by Father Marc Dechevrens as noticeable in China. In England it is more common in the winter than in the summer months, and does not appear to occur especially in warm weather, although I do not know that it has been noticed during frost. The furrows between the bands of light are not, so far as I have observed, rapidly movable in the sky in England, and they seem to be traceable to hills beneath the horizon, rather than to cumuli. I have never noticed them where the sun sets beneath a sea horizon.

W. CLEMENT LEY

The phenomenon described by M. Dechevrens as often witnessed in China, I have several times seen in this country, namely, beams or spokes in the eastern sky about sunset, springing from a point due opposite to the sun. The appearance is not very strongly marked, and I used to think I must have been mistaken, till I came to see the true explanation, which was the same as that furnished by your correspondent.

There seems no reason why the phenomenon should not be common, and perhaps if looked out for it would be found to be. But who looks east at sunset? Something in the same way everybody has seen the rainbow; but the solar halo, which is really commoner, few people, not readers of scientific works, have ever seen at all. The appearance in question is due to cloud-shadows in an unusual perspective and in a clear sky; now shadow may not only be seen carried by misty, mealy, dusty, or smoky air near the ground, but even on almost every bright day, by seemingly clear air high overhead. Therefore, if this sunset phenomenon is much commoner in China, there must, one would think, be some other reason for it than that the sky of England is not heavily charged enough with vapour to carry shadow. Rather it is too much charged, and the edge of the shadow becomes lost with distance and with the thickening of the air towards the horizon before the convergence of the beams eastwards is marked enough to catch the eye.

I may remark that things common at home have sometimes first been remarked abroad. The stars in snow were first observed in the polar regions; it was thought that they only are there, but now everyone sees them with the naked eye on his coatsleeve.

GERARD HOPKINS

Stonyhurst College

Priestley and Lavoisier

I AM sorry that Mr. Rodwell should have thought it necessary to revive the old oxygen quarrel, and the more so, as he has taken an unpatriotic part against Priestley, and indorsed the complacent statement of Wurtz, that chemistry is a French science founded by Lavoisier; forgetting, perhaps, that the title, "La Chimie Française," was invented by Fourcroy, and objected to by Lavoisier.

The fact is, that chemistry has no nationality. It belongs to the universal republic of Nature, and had no proper existence for us until Dalton discovered its laws.

In the scientific democracy, to use Lord Bacon's expression, discoverers are mutually dependent, and it would perhaps be impossible to find any one capable of standing alone. It has even been charged against our great Newton that his astronomical discoveries are to be found in Kepler; but, as Dr. Whewell well remarks, it required a Newton to find them there.

That the compound is always equal to the sum of its elements, was known long before Lavoisier, and so early as 1630 Rey gave the true explanation of the increase of the weight of metals by calcination. Lavoisier's note of 1772 was, as he admitted, based upon Priestley's earlier experiments, begun in 1744; while the acceptance of Lavoisier's doctrine was mainly due to the capital discovery of the composition of water by Cavendish, in 1784.

If at this advanced period we are required to put in national claims, then surely our own countrymen must share largely in the honours which Mr. Rodwell reserves for Lavoisier alone. Black, Priestley, and Cavendish are the founders of pneumatic chemistry. Priestley discovered oxygen in 1774, Cavendish discovered hydrogen in 1784, while Davy abjured Lavoisier's *principe oxygène*, and by his numerous discoveries gave the chemical edifice so rude a shake, that it had to be taken down and rebuilt.

C. TOMLINSON

Highgate, N., November 4

Wire Guns

IN the last number of NATURE there is an interesting paper on "Wire Guns," and incidentally various methods of manufacturing guns is mentioned. *Apropos* of this permit me to relate a curious fact regarding gunmaking which came under my notice many years ago, and which supports the adage that there is nothing new under the sun. In the autumn of 1841 Sir H. Gough took the batteries of Chusan by a turning movement and thus spoiled the Chinese preparations. The force captured a large number of guns, some very fine bronze ones, but there were also a good many smaller iron ones, and as these were of no value they were ordered to be destroyed. The Royal Artillery tried to burst these without success at first, and only after sinking the muzzles in the ground did they succeed. It was then ascertained that the reason of the extreme strength of the gun arose from its strange manufacture. It had an inner tube of wrought iron, over which the gun was *cast*, anticipating by many years a somewhat similar plan by Palliser.

Cheltenham, November 3

W. H. C. B.

Palæolithic River Gravels

MR. C. EVANS, in NATURE, vol. xxvii. p. 8, wishes our anthropologists to furnish an explanation why the mortal remains of palæolithic man are not to be found amongst his "so-called 'flint implements.'"

The question is one that naturally occurs to any one whose practical acquaintance with anthropological "finds" is of a limited character; and it may fairly be presumed that the inquirer has not himself seen and handled such relics, else he would scarcely have imagined it within the range of possibility that they could have been "formed by natural causes," by which, I suppose, he wishes to infer that they were not made by *man*.

As I am a mere tyro myself, and therefore unbiassed in the matter, I beg leave to state, for the benefit of any whose acquaintance with the subject is of only a rudimentary nature—or less—what appears to be a reasonable explanation of the case.

1. The implements of foremost scientific interest are probably those which are found in the various well-known caves, in that they retain in the highest degree all the original sharpness of edge possible only under the slow and undisturbed circumstances of the formation of the stalagmitic rock, or silt deposit, in which they have become embedded above the surface of the ancient floor. All such specimens bear clear and unmistakable testimony to their nature and use as weapons.

2. The alternative hunting-grounds for flint implements are the wide-spread gravels which formed the beds and older banks of the ancient rivers, and which have been of late so thoroughly explored by Mr. Worthington Smith, as recorded by him in this Journal, in so many interesting and valuable communications. Respecting these it is only natural that in some cases the specimens have been subjected to much detrition; but then a special value attaches to them on that very account. Of the river gravels as localities from which such evidences are obtainable it is quite unnecessary for me to use space in emphasising the importance of river-sides as a habitat of primitive man.

3. "The entire absence of the bones of man," is simply due to the rapid decomposition of the osseous frames of small-boned animals, and the speedy annihilation of which in the case of man—cremation and other means of disposal apart—is particularly noticeable.

Perhaps the position will be best understood by suggesting the question, "Do you imagine it at all probable that you could unearth any trace of a single bone of one of your pedigree ancestors, say only your great-great-grandfather?" If any of you should doubt the impossibility of such a thing, let proof be given by employing the first grave-digger—*out* of "Hamlet"—to bring the treasures to the light of day, and let the facts of the case be placed on careful record.

4. Any connoisseur can at once tell by the touch of a flint flake whether it has been worked or not, and the fracture always bears certain signs by which the operation may be known to have been performed.

It is somewhat remarkable that there should be any so faithless as to seek after signs so easily to be discerned, in opposition to the testimony of reliable authorities; and *it is surely time* that surrounded as we are with national museums and libraries full of patent facts appealing to all who cannot work for themselves, we should cease to throw discredit upon the evidence of many careful observers and honourable truth-seekers.

Highbury WM. WHITE

YOUR correspondent, Mr. C. Evans, raises the question, in your issue of November 2, whether the peculiarly-chipped flint found in the palæolithic gravels, and accepted as the work of man, may not be the result of natural causes.

Mr. Evans mentions "the presence of bones of *recent* and extinct Mammalia." If your correspondent has clear evidence of the presence of bones of *recent* mammalia with the chipped flints that evidence would prove that the flints in question have not been so chipped by Palæolithic man, but are either nature's work, or the product of man of more recent times, and the gravels in such case should not be called Palæolithic gravels.

St. John's Wood, November 7 T. KARR CALLARD

Aurora

A MAGNIFICENT aurora was observed here last night. I first detected quivering sheaves on the northern horizon about 5.40 G.M.T. About 5.47 a dull indigo base, on or against which "sheaves" and "streamers" were playing with great beauty, was noted, surmounted by an arch of light. Soon afterwards, sharply-defined "spines" and "spikes" of great brilliancy and in patches became developed, followed by five great tongues of light stretching towards the zenith. I especially noted streamers reaching towards Vega, and passing over Mizar in Ursa Major, and some of exceptional brilliancy to north-north-east. At 6.50 irregular horizontal belts of a dull indigo tint, alternated with *horizontal* tongues of light, the streamers having generally disappeared, except to north-north-east. At 8.6 p.m. a low indigo belt, surmounted by a bright golden band, fringed the horizon, o'ertopped again by belts of paler tints respectively, while detached brilliant streamers shot up fitfully towards Cassiopeia. At 11 p.m. auroral lights were still seen.

To-day I intend to examine the sun's disc, and expect to see signs of disturbance.

Fort William, November 14 CLEMENT L. WRAGGE

A Dredging Implement

I WAS much interested in reading, in the last number of NATURE, Prof. Milnes Marshall's account of his successful trial of a new dredging implement.

A few summers ago I constructed and used in Lamlash Bay, Arran, a somewhat similar machine, suggested, like Prof. Marshall's, by the Philippine Islander's dredge used in the *Euplectella* fishery. My implement was a rough copy of one brought from Cebu which I had seen at the *Challenger* office in Edinburgh. It had two slight wooden bars, 5 or 6 feet each in length, meeting at about a right angle to form the front of the apparatus, and having several cross-pieces connecting them further back. I attached large fish-hooks, not to cords hanging from the frame, as in Prof. Marshall's instrument, but to the long bars themselves (as in the Philippine Islanders' machine), and also to the cross-pieces. One weight was tied to a cross-piece near the centre of the frame-work, and a second was attached to the rope a few feet from the front of the instrument, so as to make the pull more horizontal, and so prevent the front end from tilting upwards.

The apparatus worked well and brought up quantities of Hydroids and Polyzoa; but as I was not dredging for Giant Pennatulids, after a few trials I gave it up and returned to the ordinary naturalist's dredge. In one case, however, I found my fish-hook apparatus serviceable. I wished to search a remarkably sea-weedy region, in a few fathoms of water, chiefly for Ascidians attached to the sea-weeds. The ordinary dredge I found almost invariably soon after reaching the bottom, got foul of a large *Laminaria* or some other Algæ, which stretched across the mouth and prevented anything entering. The frame-work with hooks, on the other hand, always brought up enormous masses of stuff, in many cases dragging the *Laminaria* up by the "roots," and hoisting also sometimes stones and shells to which the Algæ were attached, and on which were very frequently the Ascidians I was in quest of.

I should think this kind of apparatus would be most useful for obtaining Algæ on rocky ground, and its value in dredging Pennatulids is sufficiently shown by Prof. Marshall's experience at Oban. W. A. HERDMAN

University College, Liverpool

Forged Irish Antiquities

UP to the present we have had little reason to complain of forgeries among Irish antiquities. Shams have frequently been offered for sale, but they could scarcely be called forgeries, as they were so unlike genuine articles that persons of ordinary experience could scarcely be deceived by them. Lately, however, some very clever imitations have come under my notice. The objects imitated are those known as oval tool-stones, which were formerly very rare but are now offered in lots of two or three together. I believe the fabricated articles are produced somewhere about the Giant's Causeway, the ordinary black shore pebbles being used for the purpose. W. J. KNOWLES

Flixton Place, Ballymena, November 11

THE NEW NATURAL HISTORY MUSEUM

SINCE our previous notice of the great building which has been erected at South Kensington for the reception of the Natural History Collections of the British Museum (NATURE, vol. xxiii. p. 549, April 14, 1881), eighteen months have elapsed, and during that period great progress has been made in the transfer and arrangement of specimens. It may not be uninteresting to the readers of NATURE to receive some information concerning the present condition of affairs and the prospective arrangements in connection with the housing and exhibition of the priceless treasures of the national collections.

The first point which strikes a visitor at the present time is that a serious mistake has been made in the erection of a building with such elaborate and ornate internal decorations for museum purposes. Now that the cases are nearly all in position and the specimens are gradually being arranged in them, this incongruity between the style and objects of the building becomes more and more apparent. On the one hand, it is clear that the form, position, and illumination of the cases has in many instances been sacrificed to a fear of interfering with the general architectural effect; and on the other hand it is equally manifest that it will be impossible to make full use of the floor space, and especially the best-lighted portion of it, without seriously detracting from the artistic effects designed by the architect.

Thus we find the beautiful arcade formed by a series of pierced wall-cases in the Coral-gallery has its effect totally destroyed by the floor-cases, which it has been found necessary to place along the central line; and in the British gallery the vistas designed by the architect have been completely marred by the insertion of large cases in some of the arches. Again and again we find massive columns, beautiful in themselves perhaps, breaking up a line of cases, or throwing their contents into deep shade. The peculiar tint of the terra-cotta, too, is far from being suitable for making the objects of the Museum stand out in relief, and this is particularly manifest in the case of the palæontological collections, where a great majority of the specimens have a very similar colouring. When an attempt has been made to remedy this by giving the walls near the objects other tints; it is found that such tints do not harmonize well with the general colouring of the building. Nor is the wisdom apparent of bringing into close proximity natural-history objects with the conventional representations of them adopted by architects. The crowding together, on the same column or moulding, of representations on the same scale of microscopic and gigantic organisms, of inhabitants of the sea and of the land, and of the forms of life belonging to present and those of former periods of the earth's history, seems to be scarcely warrantable in a building designed for educational purposes.

Greatly as we admire the spacious hall, the grand staircase, the long colonnades, and the picturesque colouring of the whole building, we cannot but feel that the adoption of such a semi-ecclesiastical style was a mistake. We fear that in the future there will be a perpetual conflict between the views of the keepers of the Museum-collections and those of the architect of the building; for the erection of cases as they may be required in the most convenient and best-lighted situations cannot fail to detract from the striking and pleasing effects of the architecture.

Apart from this fundamental objection, however, we find nothing but what is praiseworthy in the arrangements which are being made to worthily exhibit to the public these grand collections, of which such large portions have been long buried at Bloomsbury. In a few months the whole of them will have been removed from their old places of exhibition (or more often of sepulture) to the new galleries, where the space available for their arrangement is so much greater. The cases in the Zoological Galleries are now almost completed and fitted, and the collections of osteology and shells with some of the stuffed animals, have been already removed to their new home—so that the public may hope to see the transfer of the whole of the specimens completed by next spring.

The keepers of the geological, mineralogical, and botanical collections, which are housed in the eastern wing and annexes of the building, have had a very difficult task to perform. They were called upon to remove these collections before the fitting of cases in the new buildings was completed, and in consequence of this the re-arrangement of the specimens, with the incorporation of the valuable material long packed away in the cellars at Bloomsbury owing to want of space, was rendered additionally laborious and troublesome. These difficulties have now, for the most part, however, been happily overcome.

The Geological collections, in spite of their vastness have been to a great extent arranged. The Mammalian and Reptilian Galleries are indeed almost completed, and much progress has been made with the Fish Gallery and the several rooms devoted to the exhibition of the invertebrata and the stratigraphical collections. The trustees have been fortunate in securing the services of such an experienced palæontologist as Mr. Etheridge to second the energetic efforts of Dr. Woodward in this department. By the insertion of drawings and tables, illustrative of the structure and classification of the fossil forms, the value of this part of the collection to students has been greatly enhanced.

In the Mineralogical Gallery everyone must be struck by the improvement in the cases, now that the specimens are no longer crowded together, as was the case in the old museum. At the end of the general gallery, and in the adjoining pavilion, there are a number of interesting special collections. First and foremost among these is the unrivalled series of meteorites, which is now displayed to much greater advantage than at Bloomsbury; with these are collections of crystals, both artificial and natural, of pseudomorphs and of rocks, or mineral aggregates—the latter being an entirely new feature in this department. Large specimens, illustrating the abnormal development, the mode of association, and the economic uses of minerals are here being arranged, and they make a very fine display. Working mineralogists will be thankful to Mr. Fletcher for his capital design of setting apart a case, in which new acquisitions to the collection are exhibited for awhile, before being incorporated with the general series.

The portion of the Botanical collection available for public exhibition is small, but Mr. Carruthers, the keeper, has brought together a capital series of examples of all the great divisions of the vegetable kingdom—illustrating the dried specimens, where necessary, by drawings and models.

There are two points, however, in connection with the establishment concerning which the readers of NATURE will naturally be especially desirous of information—first, as to the facilities to be afforded to students for examining the valuable types and rare specimens in which the collections are so rich, and secondly, with respect to the improvements which are sought to be made in the Museum, regarded from the point of view of an educational institution. The surest test of the efficiency of the administration of such a museum as this will be found in the manner in which these two great objects are attained by its keepers.

Close days for students having been now entirely abolished, the trustees of the Museum have provided galleries in each of the departments where scientific workers can pursue their studies undisturbed. We cannot help thinking that this plan is far better than the old one, which required original investigators to attend on those days of the week when the public were not admitted to the galleries—a restriction keenly felt by busy men in this country, and more especially by foreigners, who had perhaps come to this country with the sole object of devoting their time to the study of our national collections. As there are valuable reference libraries in each of the departments, and a general library of scientific journals for the whole establishment, the student has much greater facilities than formerly for carrying on his work, and nothing can exceed the courtesy with which persons actually engaged in scientific research are received and aided by the keepers and their assistants.

The publication of the series of well-known and valuable scientific catalogues is still proceeding. During the pressure of work caused by the removal of these vast collections, the trustees of the Museum have done wisely to avail themselves of the aid of specialists from outside, in connection with certain of the collections. Thus the collection of the fossil foraminifera has been arranged by Prof. T. Rupert Jones, whose catalogue of the same has been recently published. Dr. Hinde has in the same way dealt with the grand collection of fossil sponges; and his illustrated catalogue of them is now in the press.

But while the purely scientific objects of the Museum are not being lost sight of, we are glad to find that the greatest efforts are being directed by the keepers to the development of the institution as a means of popular education. In addition to the three admirable guides, published at the low price of one penny each, other popular works in illustration of the collection are being prepared. Thus Mr. Fletcher has written a penny guide to the collection of meteorites, in which he has drawn up one of the best statements concerning the nature of these bodies, and of the grounds on which they are so greatly valued by scientific inquirers, that we ever remember to have read. Simple in its language and mode of treatment of the subject, this little guide is replete with the most valuable information—information which the student of the collection might ransack a library in vain to find.

Still more interesting is Dr. Woodward's venture in the same direction—an illustrated guide for the department of Geology and Palæontology. The woodcut illustrations of this work are in part original, and in part borrowed from various scientific manuals, the publishers of which have generously granted the use of them to the Museum authorities. By the aid of these woodcuts Dr. Woodward is able to call attention to the chief facts concerning the structure of some of the most remarkable fossils in the collection, and the guide forms an excellent introduction to the study of palæontology. At present the only part of this guide which is illustrated by woodcuts is that which deals with the fossil vertebrates, for these only are as yet fully arranged; but in subsequent editions, no doubt, Dr. Woodward will give equal attention to the description of the most important forms, among the invertebrates. The design is an excellent one, and there is every promise in the present instalment of the work of its being admirably carried out. Such work cannot fail to be the means of diffusing in the widest possible manner accurate notions on the subject of natural history among the people. We hope that its circulation may be as large as that of Prof. Oliver's admirably illustrated guide to Kew Gardens, which we are glad to see has passed through twenty-nine editions.

While on the subject of the means adopted by the Museum authorities to make the collections a means of diffusing correct ideas among the people, we cannot avoid referring to Prof. Owen's design of surrounding the great central hall of the building with an "Index Museum." The idea is most praiseworthy, but its execution will, we fear, be attended with serious difficulties. Prof. Owen proposes to devote the first of the six recesses on the western side of the central hall to the illustration of man, the two next to the other mammalia, the fourth to birds, the fifth to reptiles, and the sixth to fishes. On the other side three recesses are to be devoted to the invertebrata, and one each to botany, mineralogy, and geology. Few naturalists will agree with Prof. Owen that the points which distinguish man from the rest of the animal kingdom, are to the zoologist, of such importance as to necessitate the setting apart of a division of the Index Museum for their illustration; and the limited portion of the available space assigned to botany and geology will occasion much surprise. As structural alterations have interfered with the use of two of these recesses, and the lighting of some of them is far from being satisfactory, the project may perhaps have to be greatly modified. One of the recesses, that devoted to the birds, has been already arranged with instructive diagrams and well-selected specimens, and a penny guide to it, written by Prof. Owen in his well-known clear and attractive style, has been published. If the design is carried further, we hope the greatest care will be taken to make the classification and arrangement adopted in the Index Museum harmonise with that employed in the several galleries, for otherwise such a museum will not serve as an index to the great collection, but will be a source of confusion rather than of assistance to students.

Of the zoological collections we can say little at present. The birds will occupy the ground floor of the western wing of the building, and the mammals the floor above. The osteological collections belonging to these two departments are already arranged in the upper floor, and form a new and most valuable feature of the Museum. The articulated skeletons are exhibited on the floor and in glass cases, behind which cupboards are constructed for the reception of unarticulated skeletons. The Pavilion contains a special series of bones, which are reserved for purposes of study. The skeletons of whales are to be housed in the basement of the building.

Generally we find that the convenience of the public has been fully consulted in the arrangements of the building. The lavatories and cloak-rooms are all that can be desired, but we suspect that much disappointment will be felt with regard to the refreshment department as at present constituted. Small and inconvenient counters are being erected on the highest story of the building, outside the Botanical and Osteological Galleries respectively. The obstacle thus created to the ingress and egress of visitors to those departments, and the fact that mice will infallibly be brought to them, is enough to ensure condemnation of such a plan. We hope that the trustees may yet reconsider the question, and find themselves able to devote to the purpose of refreshment, a room in the building which is centrally situated, and at the same time entirely cut off from the collections.

THE COMET

WE take the following from the *Sydney Morning Herald* of September 19:—

Mr. H. C. Russell, Government Astronomer, sends us the following interesting particulars respecting the comet, under yesterday's date:—

The comet discovered on the 7th has developed in brilliance rapidly. When I first saw it on the 8th, the nucleus was equal to a bright star of the second magnitude; by the 11th it was brighter than a first magnitude star, and I was able to see it for eight minutes after sunrise on that day. Subsequently, the mornings were cloudy, and I could not see the comet either then or during the daylight, probably because of the sea haze, which is more or less part of the N.E. wind. The comet has, however, increased in brilliance so rapidly that Mr. Ellery was able to see the comet at noon, and telegraphed to me to that effect, and the air being clear it was found at once. I had not anticipated such a wonderful increase in its light, for now it is easily seen in the full glare of the sunshine, like a star of the first magnitude, even when viewed without a telescope, and it must be many times more brilliant than Venus when at maximum. In the large telescope the nucleus appears round and well defined, and measured three seconds in diameter; from it, extended on each side, the first branches of the coma, like two little cherub wings, and in front, the great body of the coma, forming a brilliant and symmetrical head, and thence turning to form the tail six minutes long. Under close scrutiny it was evident that the coma had one or more dark bands, curved like the outline, which made the form very interesting, but the glare of the sunlight made it very trying to the eyes. It is a splendid object, and it is to be regretted that no stars can be seen by means of which to fix accurately the comet's position; but should the weather continue fine, it will be possible to do this with the transit instrument. My observations this afternoon show that the comet was moving away from the sun again, and should this be maintained, it will become a morning, not an evening object. At 1.15 p.m. to-day the comet was only 9m. 45s. west from the centre of the sun, and 7m. of declination south; by 5 p.m. the distance in right ascension had increased three minutes; the declination was slightly less. Unless some rapid change in the direction of the motion takes place before to-morrow (and now that the comet is so near the sun this may result), the comet will be seen without the aid of a telescope, about seven degrees west of the sun. History tells us of wonderful comets which outshone the sun; but it is usual to receive these statements after liberal discount. Nevertheless the great comet of 1843 was easily seen by spectators when it was only 1° 23′ from the sun (that is, about half the distance between the comet and sun to-day at 1 p.m.); and at Panama the observers standing in the shade of a wall saw the comet with a tail four or five degrees in length. In Mexico, also, the comet was seen near the sun like a star of the first magnitude. It is probable, therefore, that the comet of 1843, the brightest of this century, was brighter than the present one.

We are indebted to Mr. John Tebbutt, of the Private Observatory, Windsor, for the following communications respecting the comet:—

September 16.—I succeeded in obtaining pretty good observations of the comet on the mornings of the 9th and 10th instant, but since the latter date fog and cloud have prevented observation. The following are the positions secured:—September 8d. 17h. 54m. 52s., R.A.=9h. 37m. 7·50s., Declination S. =0° 57′ 46″·4; September 9d. 17h. 49m. 45s., R.A. 9h. 45m. 47·81s., Declination S. = 0° 53′ 36″·2. A third position will, of course be necessary for the approximate determination of the orbit. In the absence, however, of such a determination it may safely be stated that the comet is rapidly coming into conjunction with the sun, and near its ascending node. It is not at all improbable that the comet is passing between us and the sun, and that in consequence its tail will be pointed approximately towards the earth. As we do not at present know the exact apparent track of the stranger, it would be advisable to watch the sun's disc at intervals during the next few days for a possible transit, and to look out at night for any indications of the aurora consequent on a possible near approach of the earth to the tail. It will be remembered that our passage through the tail of the great comet of 1861 was marked by a general exhibition of auroral phenomena. It is highly probable that the comet will, towards the close of next week, become an imposing object in the west during the evenings. Like the recent Wells comet, this body will doubtless be well observed with the transit circle in full sunlight.

September 18.—The extraordinary interest which attaches to the comet now visible will, I trust, afford a sufficient apology for my again trespassing so soon on your valuable space. Supposing, from the rapid increase in the brilliancy of the comet that it would probably be seen in full daylight, I turned my attention to the immediate neighbourhood of the sun about 10h. a.m. yesterday. I at once found the comet without a telescope; it was visible about four or five degrees west of that luminary as a brilliant white dagger-like object. The head was beautifully distinct, and the tail could be readily traced for about twenty minutes of arc. I succeeded in obtaining eleven absolute determinations of position with the equatorial, the approximate right ascension and declination of the last observation, 11h. 25m. a.m., being respectively 11h. 22m. and 1° 10′ north. I attempted to observe with the transit instrument. The comet entered the field of the telescope and was at once bisected by the declination wire; but, unfortunately, just before it reached the first transit wire it was obscured by a passing cloud and remained so till just previously to its quitting the field, when it was still found to be bisected. I trust the Melbourne observers will not fail to avail themselves of every opportunity to observe with the transit circle. If my memory serves me well I believe the history of astronomy does not furnish any previous instance of a comet being seen near the sun with the unassisted eye since the appearance of the extraordinary and well-known comet of 1843. That body was seen at 3h. 6m. p.m. at Portland, U.S., by a Mr. Clark, and consequently in full sunlight, and its distance from the sun measured by him with an ordinary sextant. The present comet was still plainly to be seen without the telescope at 5h. p.m. yesterday. To-day it will probably be too nearly in a line with the sun to be seen; but on Tuesday and Wednesday it will, I think, again be visible. In the absence of any calculation I will here venture to offer one or two remarks. The comet appears, from a rough inspection of its apparent path, to be moving in a track somewhat resembling that which would be followed at this time of the year by the great comet of 1843 on its way to perihelion, and it is a significant fact that the earth is to-day almost exactly on the line of the comet's nodes, and on the ascending side of the sun. At Greenwich mean noon to-day the longitude of the earth will be 355½°, while that of the ascending node of the great comet of 1843 is about 358°. It will be remembered that at the time of the appearance of the great comet of 1880 the parabolic elements of that body were found to be almost precisely those of the great comet of 1843 (see my paper read before the Royal Society of New South Wales in July, 1880)—and it was therefore considered that the two bodies were identical. It will be remembered, too, that at a discussion at one of the Royal Astronomical Society's meetings it was suggested that although the period between the returns of the comet in 1843 and 1880 was 37 years, the time of revolution might be greatly shortened by the comet's passage through the sun's coronal atmosphere. The question therefore arises—Is the appearance of the present comet a return of the same body? Should the comet make its appearance in the west after sunset, it is quite certain that it cannot be identical with that of 1843 and 1880; but if it should now rapidly revolve round the sun, and make its appearance again west of that luminary, it must certainly be a comet of very small perihelion distance. Whether it is the comet of 1843 and 1880 time alone will decide. I dare say your readers will call to mind the speculation of Mr. Proctor on the probable return of the comet of 1843 and 1880.

P.S.—At 11h. 35m. a.m. to-day (September 18) I again detected the comet with the unassisted eye. It was then about three-quarters of a degree west of the sun's western limb, and apparently moving west. In this case the comet in a few days must be again looked for in the morning sky.

The *Herald* writes:—The comet discovered on the 7th instant has increased so greatly in brilliancy that it can be discerned in daylight with the naked eye. The fact was discovered by Mr. Ellery, Government Astronomer in Melbourne, at noon, and by him communicated to Mr. Russell; but the unusual phenomenon was observed by Mr. Tebbutt, of the private observatory, Windsor, at about 10 o'clock. The authorities seem to agree that the history of astronomy does not furnish any previous instance of a comet being seen near the sun, as this is, since the extraordinary and well-known visitant of 1843. It is probable, Mr. Russell states, that the comet may be seen about seven degrees west of the sun, from which luminary it is apparently, however, moving away; and, should this movement be maintained, it will become a morning and not an evening object.

So far the Sydney journal.

We are indebted to Sir H. Lefroy for an extract from the *Eastern Star*, published at Grahamstown, Cape Colony, in which Mr. L. A. Eddie, F.R.A.S., draws attention to the duplication of the nucleus which appears to have been first remarked at the Royal Observatory, Cape of Good Hope, on September 30, and on the same date in the United States: a day or so later European observers very generally perceived it. On the morning of September 24, at 4h. 30m., Mr. Eddie, says: "A most glorious sight presented itself. The head of the comet had not yet risen, but a broad belt of golden light, about two degrees in breadth, streamed upwards from the horizon to about ten degrees; and from the northern margin of this again, a thin streak of less brilliant light extended upwards to about another twelve degrees, and when the head had fully risen above the horizon at 4h. 43m. a.m., there were about twenty-five degrees in length of intensely luminous matter, stretching upwards from a still more luminous head, and inclined to the horizon at an angle of 70°. . . . The head appeared as before, to consist of an apparently very solid though not very large nucleus, surrounded by a dense coma of no great extent, especially preceding the nucleus, and possessing no dark intervals, &c." The weather prevented further observation at Grahamstown till the morning of October 3, when, on directing his 9½-inch Calver upon the nucleus, Mr. Eddie saw not one round planetary disc, as he had last seen it, but "two distinct ellipsoidal nuclei in juxtaposition, each of them brighter on the interior edge, and drawn out, as it were, towards the comet's ulterior boundary, so that their conjugate axes were about double the transverse. They closely resembled, in the inverting telescope, the flames of two candles placed the one above the other, so that the uppermost part of the lower flame almost overlapped the lower portion of the other. There was a dark rift the breadth of the transverse axes of these nuclei, extending from the hindermost one into the tail. These two nuclei were not parallel with the axis of the comet, but the foremost was drawn, as it were, to the south, or nearer to the direction in which the comet is moving." Mr. Eddie further compares the two nuclei to the double-star *α* Centauri when viewed through a cloud with a low power. When daylight had advanced, they could be seen in the telescope perfectly free from the light of the surrounding coma. On the following morning the nuclei were distinctly divided with powers of 60 and 100 on the reflector: the preceding nucleus was larger and brighter than the other, but both were, if anything, smaller than previously.

The *Natal Mercury* of October 6 describes the imposing spectacle which the comet presented as it rose apparently from the Indian Ocean. The nucleus shone with a brilliancy rivalling Sirius, or even Venus, and the tail was slightly curved, and though, as dawn approached, a little diminished in length, appeared more concentrated and magnificent.

Observers who remember the great comet of 1843, as it presented itself in the southern hemisphere, are somewhat divided in opinion as to which body to give the palm on the score of brilliancy, though most of them appear inclined to favour the former. The Emperor of Brazil, who observed the comet of 1843 close to the sun on February 28, and on the following evenings, considers it was not so remarkable for the brightness of the nucleus as the present comet, but that the tail had a much greater extent.

At Santiago, Chile, the comet was visible on September 17, some minutes before sunrise, and on the next morning could be followed until 11h. 30m. with the greatest facility without the telescope; part of the tail near the nucleus was also visible, the northern border being much brighter than the other. On September 20, though the light of the comet had somewhat diminished, it was seen with the naked eye till 10h. 30m. M. Niesten, Chief of the Belgian expedition for the observation of the transit of Venus, observed the comet in Chile: he found the length of the tail (northern branch) 25° on September 22, and 22° on the following morning.

By the kindness of the Astronomer Royal, we learn that the comet was observed on the meridian at Melbourne on September 15, 16, and 17 civil reckoning; equatorial observations commenced on the morning of September 10: Mr. John Tebbutt observed the comet the previous morning at his private observatory, Windsor, N.S.W. The Melbourne meridian observations will be of great value in the determination of the elements of the orbit prior to the comet's rush through the solar coronal region, the last one having been made only fifteen hours before the perihelion passage.

Subjoined is an ephemeris of the comet for 18h. M.T. at Greenwich. It will be seen that it is now well observable on the meridian.

	Right Ascension.	Declination.	Distance from Earth.	Distance from Sun.
	h. m. s.	° ′		
Nov. 16	... 9 25 58 ...	− 25 3·1 ...	1·496 ...	1·688
18	... 9 21 25 ...	25 39·0 ...		
20	... 9 16 40 ...	26 13·5 ...	1·500 ...	1·762
22	... 9 11 43 ...	26 46·4 ...		
24	... 9 6 35 ..	27 17·6 ...	1·510 ...	1·835
26	... 9 1 16 ...	27 46·9 ...		
28	... 8 55 47 ...	− 28 14·2 ...	1·514 ...	1·906

The latest investigations on the motion of this comet tend to indicate, contrary to the expectation that was at first entertained by many astronomers, that it is not identical either with the great comet of 1843, nor with that which appeared with so great a resemblance in the elements of the orbit in 1880. Calculations by Messrs. Chandler, Wendell, and Hind, are so far in accord upon this point.

RECENT DYNAMO-ELECTRIC MACHINES

ELECTRICAL inventions of innumerable kinds have of late followed one another with bewildering rapidity; and the impetus to invention afforded by the present development of electric lighting, and by recent electrical exhibitions, is making itself felt in many ways. Most important, perhaps, of these is the production of improved types of machines for generating electric currents. Dynamo-electric machines, in fact, appear to be undergoing the same kind of evolution which the steam-engine has undergone; and just at present the tendency appears to be in the direction of producing larger and heavier machines than heretofore.

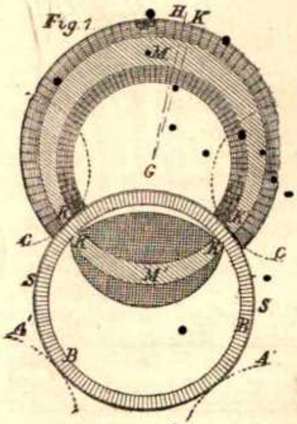
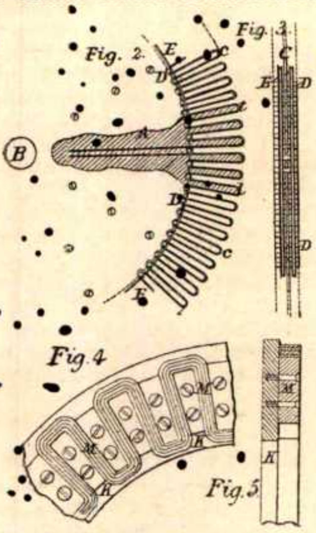


FIG. 1.—Sir W. Thomson's Roller Dynamo.

The readers of NATURE will be familiar with the description of Edison's large steam-dynamo, which first made its appearance in Paris in 1881, and of which two examples are now at work in the Edison installation at Holborn Viaduct. These monster dynamos, each requiring from 120 to 150 horse-power to drive it, are capable of lighting from 1000 to 1300 incandescent electric lamps.

Six such machines have been also erected in New York to supply the central station of the Edison Light Company. Here the unexpected difficulty has arisen that if one of the machines drops in speed the currents from the other machines short-circuit themselves through the one, and overpower the steam-engine that is driving it; a fault which will probably be remedied by a rearrangement of the governors supplying the steam to the engines.



FIGS. 2–5.—Sir W. Thomson's Disk-Dynamo

New forms of dynamo-electric machine have been designed by Sir William Thomson, some of these being for direct currents, others for alternate, but all of them of peculiar construction. The first of them, shown in Fig. 1, may be described as a modification of Siemens' well-known machine, the drum-armature being, however, made up like a hollow barrel, of which B B is a sectional view, the separate staves being copper conductors insulated from one another. They resemble the longitudinal bars used by Siemens in the armatures of his electro-plating machines, and by Edison in his steam-dynamo. At one end of the hollow drum these copper bars are united to each other in pairs, each to the one opposite it. At the other end their prolongations serve as commutator bars. A similar mode of connecting to that adopted by Edison, is also possible. Inside this hollow drum armature is an internal stationary electro-magnet, K M′ K, whose poles face those of the external field magnets. This internal magnet answers the purpose of intensifying the magnetic field, and making the magnetic system a "closed" one, as suggested long before by Lord Elphinstone and Mr. Vincent. This hollow armature Sir W. Thomson proposes to support on external antifriction rollers A A′ C C′, the lower pair A A being of non-conducting material, the upper pair being made up of conical cups of copper split radially, and serving, instead of the usual commutator "brushes" to lead away the current. The hollow armature may be driven either by the tangential force of one of the bearing rollers, or by an axle fixed into the closed end of it.

Another machine devised by Sir W. Thomson, and illustrated in Figs. 2, 3, 4, and 5, is a disk-dynamo for generating alternate currents, and is therefore allied in certain aspects to Mr. Gordon's machine, described below. The rotating armature has no iron in it; it consists of a disk of wood having upon its sides projecting wooden teeth, as shown in Figs. 2 and 3, between which a wire or strip of copper is bent backwards and forwards, and finally carried to the axle B. This disk is rotated between field-magnets

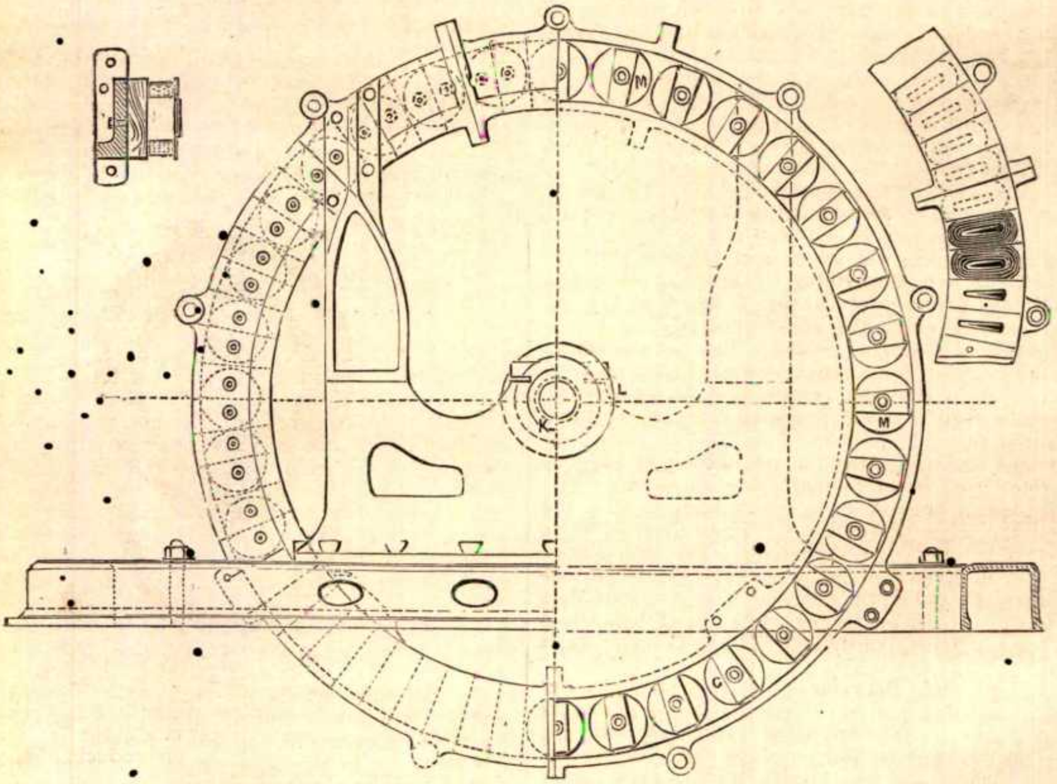


FIG. 6.—Elevation of Gordon's Dynamo, showing the rotating coils. The "taking-off" coils are shown in the top right hand corner.

having poles set alternately all round a circular frame. Figs. 4 and 5 show how this is carried out. A cast-iron ring having projecting iron pieces screwed into it is surrounded by zig-zag conductors which carry into it the current from a separate exciter. These currents pass up and down between the projecting cheeks, and excite those on both sides of them.

A still more recent, and still larger generator, is that designed by Mr. J. E. H. Gordon, whose "Physical Treatise on Electricity and Magnetism" is known to most of our readers. This machine, which is given in elevation in Fig. 6, and in end-elevation in Fig. 7, is more than 9 feet in height, and weighs 18 tons. It possesses several points of interest. The rotating armature differs from those of the well-known Gramme or Siemens' armatures, being in form a *disc*, constructed of boiler-plate, upon which the coils are carried. The machine, therefore, resembles in some respects the Siemens' alternate-current machine, though there are notable points of difference, the most important being, that whereas in most dynamo-machines the inducing field-magnets are fixed, and the induced coils rotating, in Mr. Gordon's new machine the rotating coils are those which act inductively upon the fixed coils between which they revolve. The machine furnishes alternate currents, and therefore requires separate exciters. These exciters, two Bürgin machines, send currents which enter and leave the revolving armature by brushes pressing upon rings of phosphor bronze placed upon the axis at either side. There are 64 coils upon the rotating disc, and double that number upon the fixed framework. These 128 "taking-off" coils, the form of which is shown in Fig. 8, are alternately connected to two circuits, there being 32 groups in parallel arc, each parallel containing 4 coils in series; thus bringing the total electromotive force to 105 volts when the machine is driven at 140 revolutions per minute. At this speed it actuates 1300 Swan lamps, but is calculated to actuate from 5000 to 7000 if the driving power is proportionately increased. The machine is now in operation at the Telegraph Construction and Maintenance Company's Works, East Greenwich.

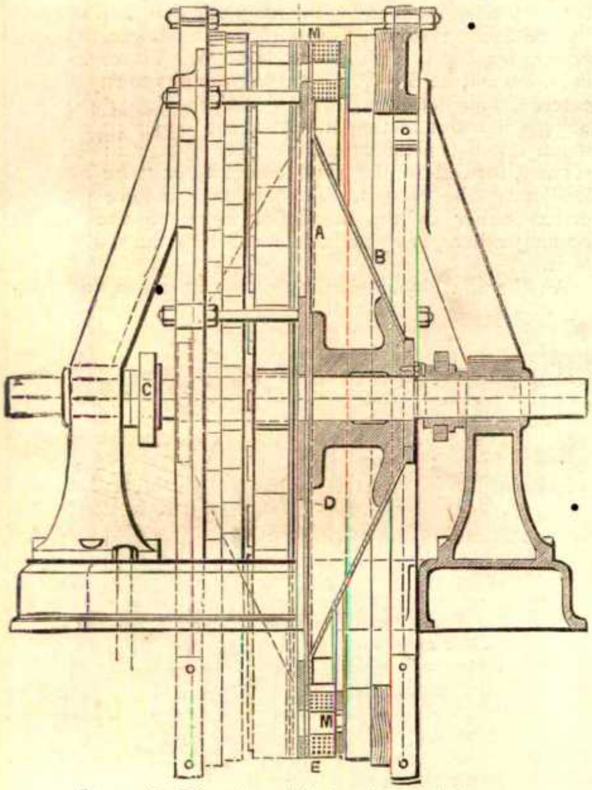


FIG. 7.—End Elevation and Section of Gordon's Dynamo.

A great deal has been said in certain quarters of late about another new dynamo, the invention of Mr. Ferranti, which, with one of those unscientific exaggerations which cannot be too strongly condemned, was pronounced to have an efficiency five times as great as that of existing dynamos. The construction of this machine has not yet been made known, but it is understood that it has no iron in the rotating armature. This is, however, no novelty in dynamos. It appears, also, that Mr. Ferranti has invented an alternate-current machine almost identical with that of Sir William Thomson described above.

Lastly, M. Gravier claims to have designed a form of dynamo in which there are neither commutators nor separate exciters, but in which continuous currents of electricity are produced in stationary coils by the passage near them of a rotating series of iron bars whose magnetism is changed, during their passage, by the reaction of the cores of the stationary coils themselves. M. Gravier has also designed a machine in which a Gramme-ring is wound with two sets of coils, a primary and a secondary, each set having its own commutator on opposite ends of the axis. A current from a separate exciting machine passes into the primary coils of the ring by one pair of brushes, and the secondary current is taken off by a second pair of brushes at the other commutator placed at right angles to the first pair. We are not aware that any practical machine thus constructed has yet been shown in action.

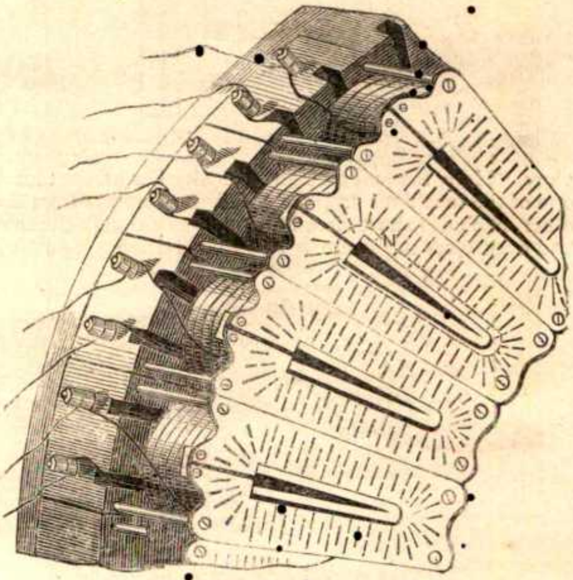


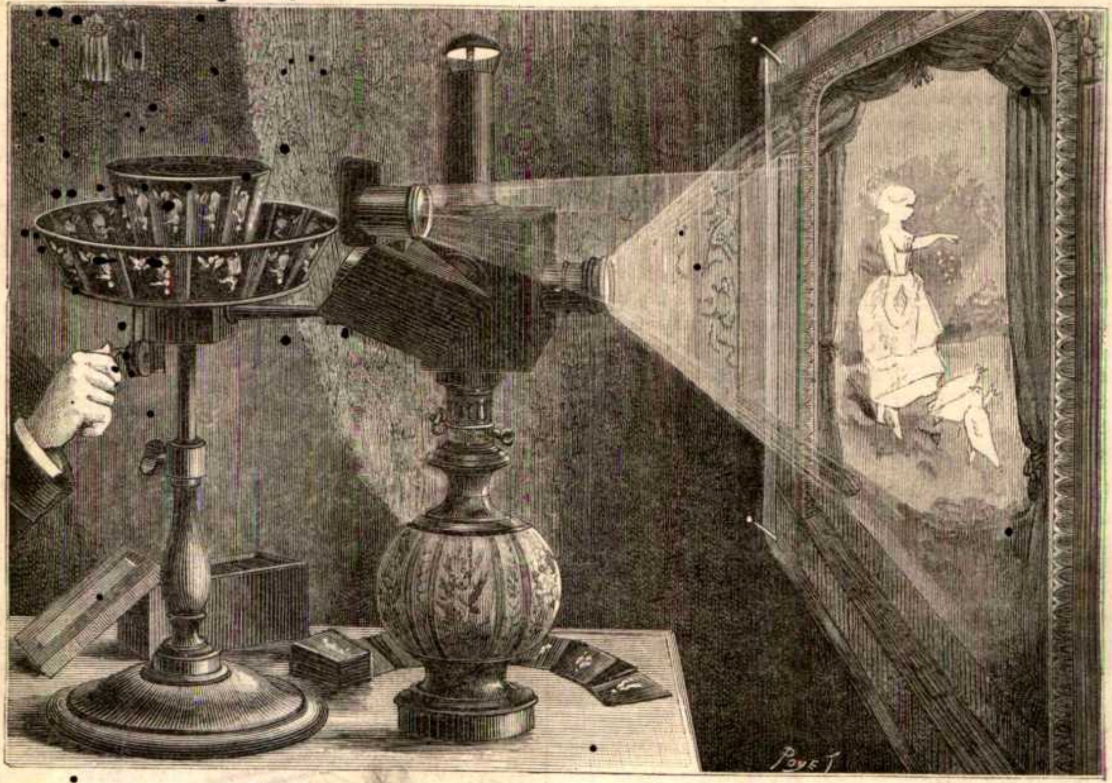
FIG. 8.—The Fixed Coils of Gordon's Dynamo.

It is certain that there is yet abundant room for great improvement in the construction of dynamo-electric machines. But the inducements to improvement at the present time are so great that rapid progress toward the desired goal of perfect efficiency and simplicity of structure is more than assured.

THE PROJECTION PRAXINOSCOPE

M. GASTON TISSANDIER describes in *La Nature* an ingenious adaptation of the praxinoscope, under the above name, by means of which the images are projected on a screen, and are visible to a large assembly.

Our engraving will give an idea of the arrangement and the effect produced. By a modification of the "lampascope," M. Reynaud, the inventor, obtains by means of an ordinary lamp, at once the projection of the scene or background—by the object-glass which is seen at the side of the lantern—and of the subject, by another object-glass which is shown in front of and a little above the same lantern. For this, the positions or phases which form a subject are drawn and coloured on glass, and are connected in a continuous band by means of any suitable material. One of these flexible bands is placed in the wide crown of the praxinoscope, which is pierced with openings corresponding to the phases of the subject. To understand the course of the luminous rays which go to form the image, it is necessary to bear in mind the condensing lens which, placed near the flame of the lamp, is not visible in the figure; then a plane mirror inclined 45°, which reflects the rays and causes them to traverse the figures filling the openings of the crown. These rays, reflected once more by the facets of the prism of mirrors, finally enter the object-glass, which transforms the vertical central image into a real image magnified on the screen. In making the two parts of the apparatus converge slightly, the animated subject is brought into the middle of the background, where it then appears to gambol. A hand-lever on the foot of the instrument allows a moderate and regular rotation to be communicated. This apparatus, with an ordinary moderator lamp, supplies well-lighted pictures and curious effects. It enables us to obtain, with the greatest ease, animated projections, without requiring any special source of light, by simply utilising the lamp in daily use.



M. Reynaud's new projection-praxinoscope.

NOTES

We take the following from the *Times*:—The council of the Royal Society have awarded the medals in their gift for the present year as follows: The Copley Medal to Prof. Cayley, F.R.S., for his researches in pure mathematics; the Rumford Medal to Capt. Abney, F.R.S., for his photographic researches and his discovery of the method of photographing the less refrangible part of the spectrum, especially the infra-red region; a royal medal to Prof. W. H. Flower, F.R.S., for his contributions to the morphology and classification of the mammalia and to anthropology; and a royal medal to Lord Rayleigh, F.R.S., for his papers in mathematical and experimental physics; the Davy Medal (in duplicate) to D. Mendelejeff and Lothar Meyer for their discovery of the periodic relations of the atomic weights. These medals will be presented at the anniversary meeting of the society on St. Andrew's Day.

The President and Council of the Geological Society hold a *conversazione* in the Society's rooms on Wednesday, the 29th inst. Fellows of the Society who have objects of interest suitable for exhibition are asked kindly to lend them for the occasion.

It is announced that General Pitt Rivers will be appointed Inspector of Ancient Monuments under the recent Act.

We announced last week the death, at the age of sixty-six years, of Prof. Johannes Theodor Reinhardt, Inspector of the Zoological Museum of the University of Copenhagen. Prof. Reinhardt was a well-known zoologist, author of an excellent memoir on the Birds of the Campos of Brazil, and of numerous papers in the scientific periodicals of Copenhagen, and will be regretted by many friends and correspondents in this country.

At the sitting of the Paris Academy of Sciences on November 13, M. Faye read letters from the captain of the *Niger*, French war steamer, on the comet, stating that it was seen at Buenos Ayres, in the streets, on November 18 in close vicinity to the sun, and that the tail was seen for the first time on board the *Niger* on September 26. The expanse of the tail was then 28°, and its transversal dimension 26°. The quantity of light was so great that when the end of the tail began to become visible the officers and sailors witnessing the phenomenon were quite unable to understand the real nature of this splendid illumination.

MR. B. J. HOPKINS, of Dalston, sends us a drawing of the head of the comet, which he saw on November 8, 16h. 50m. Viewed with the naked eye, Mr. Hopkins states, the nucleus appeared equal to a second-magnitude star; the tail was distinctly visible, having a length of about 19°; it was straight for four-fifths its length; it then abruptly curved upwards and spread itself out in the shape of a fan, with a breadth of 4°. It was still brightest on the southern side. Observing at 17h. 30m. the nucleus—as seen with a 5-inch refractor—had the appearance of being double, there being two portions of equal brightness separated by a narrow space of less brightness, the whole being surrounded by a circular nebulosity. The line joining the two bright portions of the nucleus formed an angle with the axis of the tail; and the tail immediately following the nucleus was most clearly and sharply divided into two portions of unequal brightness, the southern, as before mentioned, being by far the most brilliant. The dark rift in the tail was not so conspicuous as on the 5th inst.

M. TRESCA presented to the Academy of Sciences on Monday the third part of his great work on measures taken during the Paris Electrical Exhibition. It relates to the analysis of electric candles, and will be followed by a similar work on incandescent lights. M. Mascart sent a paper on measures taken with the registering electrometer in compliance with the wish expressed by Sir William Thomson to test the relations of the state of the weather and the electrical properties of the air.

AT the same meeting M. Janssen read in the name of the Bureau des Longitudes a report on the observations which will be made during the total eclipse of the sun of May 6, 1883, which will be observed in the Pacific Ocean. He also read a paper on his work on solar spectroscopy, and on the observation of telluric rays. Admiral Mouchez read a letter from M. Henry, who has been sent to the Pic-du-Midi to observe the forthcoming transit of Venus and determine the possibility of establishing an astronomical observatory on the top of the mountain.

THE French *Journal Officiel* has published a decree of the President establishing a council for the Observatory of Mentone.

WE are informed that the contract for the construction and erection of the Forth Bridge has been let to Sir Thomas Tancred, Bart., Mr. J. H. Falkiner, and Mr. Joseph Phillips, Civil Engineers and Contractors, of Westminster, and Messrs. Arrol and Co. of the Dalmarnock Iron Works, Glasgow. Messrs. Tancred and Falkiner have already carried out about seventy miles of railway for Mr. Fowler, and are at present constructing the new line to Southampton. Mr. Phillips has had a very wide practical experience in bridge construction and erection, and Messrs. Arrol and Co. are contractors for the new Tay Bridge, so the works are in good hands. The contract sum is 1,600,000*l.*, which is within 5000*l.* of the engineer's parliamentary estimate. The tenders received ranged from 1,485,000*l.* to 2,300,000*l.*, most of the leading firms being represented.

AT the annual general meeting of the Cambridge Philosophical Society, a resolution recording the deep regret of the Society at the lamentable event which deprived them of their late president, Prof. F. M. Balfour, was carried unanimously, and a letter expressive of their feelings was directed to be sent to Mrs. Henry Sidgwick (Prof. Balfour's sister). The officers for the ensuing year were appointed as follows:—President, Mr. J. W. L. Glaisher, F.R.S.; Vice-Presidents: Profs. Babington, Newton, and Cayley; Treasurer, Dr. Pearson; Secretaries: Mr. J. W. Clark, Mr. Trotter, and Mr. W. M. Hicks; new Members of Council: Dr. Campion, Mr. K. Hill, and Mr. J. N. Langley.

WITH regard to the recent sad suicide of a girl by leaping from one of the towers of Nôtre Dame, Dr. Bronardeli's expressed view that asphyxiation in the rapid fall may have been the cause of death, has given rise to some correspondence in *La Nature.* M. Bontemps points out that the depth of fall having been about 66 metres, the velocity acquired in the time (less than four seconds) cannot have been so great as that sometimes attained on railways, *e.g.* 33 metres per second on the line between Chalons and Paris, where the effect should be the same; yet we never hear of asphyxiation of engine drivers and stokers. He considers it desirable that the idea in question should be exploded, as unhappy persons may be led to choose suicide by fall from a height, under the notion that they will die before reaching the ground. Again, M. Gossin mentions that a few years ago a man threw himself from the top of the Column of July, and fell on an awning which sheltered workmen at the pedestal; he suffered only a few slight contusions. M. Remy says he has often seen an Englishman leap from a height of 31 metres (say 103 feet) into a deep river; and he was shown in 1852, in the island of Oahu, by missionaries, a native who had fallen from a verified height of more than 300 metres (say 1000 feet). His fall was broken near the end by a growth of ferns and other plants, and he had only a few wounds. Asked as to his sensations in falling, he said he only felt dazzled.

DR. SLUNIN has published in Russian a work—"Materials for the Knowledge of Popular Medicine in Russia"—which will be received with interest, not only by medical men but also by ethnographers. Dr. Slunin gives a detailed account of all plants and drugs used not only in Russian popular medicine in the governments of Saratoff and Astrakhan, which he knows from many years' residence, but also in all Persian, Tartar, and Central Asian medicines (with their Arabian names) that have come to his knowledge. His remarks on popular pharmacies and on the popular medical literature which goes as far back as the epoch of the flourishing times of Arabian civilisation are of great interest.

THE Catalogue of the Reference Department of the Derby Free Library is of a handy size and excellent type. We are told it contains 60,000 references to works upon the library shelves; and, upon dipping into it, the minuteness of connection which will lead to a reference to publications of scarcely higher standing than a newspaper, is imposing. We grieve to add, however, that this holds good in both senses of the word. For looking more closely we find most important references are absent. As a sample, eight references are given to the name of Garrick, but neither is his life by Murphy or Davies quoted, nor is any reference made to Boswell's "Johnson," or Goldsmith's Poems; and the extraordinary explanation of this is found in the fact that neither of these works is in the library! And this absence of important works seems to be the rule rather than the exception, carried out also with the most even-handed fairness to all subjects. Looking through the letter B as a sample, we find no works of Babbage, Back, Barbauld, Barry (Sir C.), Baxter, Beale, Baden Powell, Brewster, Barrow (Isaac or Sir Jno.), Bayne, Beckmann, Blackie, Blackstone, Borrow, Boswell, Bowring, Bridgewater Treatises, Browning (Mrs.), Buckmaster, Buxton, Butler (Bp.), or Butler (S.). Among Dictionaries neither the Penny nor the English Cyclopædia is to be found.

Nor is it that a selection of certain writers has been made, for numerous authors of many well-known works are only credited with one or two in the Derby Library Catalogue. The letter 3 is not a specially unfortunate one. Ancient Geography refers only to *Nature* and the *Quarterly Review* (one reference each). Gladstone and Hugh Miller are equally unknown. Less than a column contains all the references to Geography, while Geology has nine columns allotted to it. Under Astronomy the inquirer is referred to numerous papers where notices may be found of each of the planets and of many of the planetoids, but only fifteen *works* on Astronomy are catalogued. There is no work at all upon the Moon! Moreover, the references to works which are in this library are made with no discretion. "Barbarossa" does not refer the reader to Gibbon; "Borgia" only refers him to one article—on Lucrezia—in the *Nineteenth Century*! The spelling is not only unscholarly, but the correcting of proofs is careless. It were endless to point out the blunders everywhere; we need only refer to the name of Prof. Haeckel, spelt in four different ways upon pp. 41, 42 only! If some little town struggling against the smallness of the 1*d.* rate wishes to draw as much as possible from its Free Library with its motley collection of books contributed from various quarters, we can strongly recommend the *system* upon which this catalogue is drawn up. But that a place of the size and importance of Derby, whose rate also has been so helped by the munificence of Mr. Bass and others, should think it worth while to print and distribute a catalogue, displaying a knowledge and a collection of books in this rudimentary state, is beyond our comprehension.

THE population of Cascia (Italy) is being constantly disturbed by repeated subterranean shocks.

A VOLCANIC eruption is reported to have taken place from a mountain in the Caucasus, which has not shown any volcanic phenomena during historic times. It is the Karabetow mountain, near Temrink, in the government of Jekaterinodar (Caucasia). The subterranean noise was heard 4 versts away, the lava flowed for a distance of half a verst, and a large crater was formed.

NEWS from Belgrade states that some railway workmen have discovered a nearly perfect mammoth skeleton. It is being photographed on the spot, and will be handed over to the National Museum at Belgrade.

A NATURAL intermittent spring has recently formed in the Jachère (Hameau de l'Argentière, Hautes Alpes). At regular intervals of five and seven minutes it yields 10 litres of water each time. It is very remarkable that the first time it consists of lukewarm and colourless water, but the second of cold but wine-red water. MM. Chester and Hadley are now studying the phenomenon.

M. J. OLLER, the proprietor of the St. Germain racing establishment, is preparing to organise night races. He intends to build a central lighthouse, of which the rays will be directed on the contending horses, so that spectators sitting in the centre may follow the proceedings with as much accuracy as in open day.

AT the annual meeting for the distribution of prizes in Mason College, Birmingham, Prof. Tilden gave a sensible and interesting address on Technical Education, which has been published in a separate form.

THE Captain-General of the Philippines reports another destructive hurricane on November 5, and it is worthy of remark that since the previous hurricane, a few weeks ago, the cholera, which had been very bad, has nearly disappeared from Manila.

MESSRS. SONNENSCHEIN AND CO. announce the forthcoming publication of Dr. Coppinger's Notes of the four years' voyage from which the *Alert* has recently returned.

MR. MURRAY has issued a cheap edition of Dr. Blaikie's "Life of David Livingstone."

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus* ♂ ♂) from India, presented respectively by Mr. J. Knight and Mrs. Snell; a Sooty Mangabey (*Cercocebus fuliginosus* ♂) from West Africa, presented by Lady Stafford; two Globose Curassows (*Crax globicera* ♂ ♀) from British Honduras, presented by Mr. R. W. Ryass; a —— Buzzard (——) from Demerara, presented by Mr. G. H. Hawtayne, C.M.Z.S.; three Common Chameleons (*Chameleon vulgaris*) from Egypt, presented by Mr. W. J. Ford; a Hawk's-billed Turtle (*Chelone imbricata*) from West Indies, presented by Mr. W. Cross; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, a Black Wallaby (*Halmaturus ualabatus* ♀) from New South Wales, a Greek Land Tortoise (*Testudo græca*), South European, deposited; an American Bison (*Bison americanus* ♀) from North America, a Capybara (*Hydrochærus capybara* ♀) from South America, two Eastern Goldfinches (*Carduelis orientalis*) from Afghanistan, two Brent Geese (*Bernicla brenta*), a Red-throated Diver (*Colymbus septentrionalis*), British, purchased; three Capybaras (*Hydrochærus capybara* ♂ ♂ ♀), a Bluish Finch (*Spermophile cærulescens*) from South America, received in exchange.

GEOGRAPHICAL NOTES

AT the opening meeting of the Geographical Society on Monday Mr. A. R. Colquhoun gave an account of his recent adventurous journey, in company with the late Mr. Wahab, from Canton through Yunnan to Bhamo. Mr. Colquhoun's object was mainly to discover trade-routes between Burmah and China, but he collected some interesting information on Further Yünnan, parts of which have not before been visited by European travellers. Mr. Colquhoun describes Yünnan, which is the most westerly of the eighteen provinces of China, as a great uneven plateau, of which the main ranges trend north and south; those in the north reaching an elevation of from twelve to seventeen thousand feet, while in the south they sink to seven or eight thousand feet. In the south, and especially in the south-west, there are many wide fertile plains and valleys, some with large lakes in them. These plains are very rich and thickly populated, the number of towns and villages and the comfortable appearance of the peasantry being very remarkable. Fruits of all kinds—pears, peaches, chestnuts, and even grapes—are found in abundance, while roses, rhododendrons, and camelias of several varieties grow untended on the hill-sides. Minerals are found in great quantities. The travellers constantly passed caravans laden with silver, lead, copper, and tin in ingots; and gold is beaten out into leaf in Tali, and sent in large quantities to Burma. Coal, iron, silver, tin, and copper mines were frequently passed. Mr. Colquhoun also found that the celebrated Puerh tea, the most fancied in China, is not really a Chinese tea at all, but is grown in the Shan district of I-bang, some five days south of Puerh, the nearest prefectural town. In the south the temperature is moderate, and the rains by no means excessive; but the farther north the travellers went, the more sparse became the population, and the more sterile the country, until in the extreme north the hills were enveloped in almost perpetual fogs, rain, and mists, and were practically uninhabitable. The people themselves are mostly the old aboriginal tribes—Lolo, Pai, and Maio—the Chinese being mostly of the official class, and found only in the towns. These aborigines have a much more distinct physiognomy than the bullet-headed Celestial, and are remarkable for their frank and genial hospitality. The women do not crush their feet, and they adopt a picturesque dress not unlike that worn of old by Tyrolese and Swiss maidens. They have a novel way of making marriage engagements. On New Year's Day the unmarried people range themselves, according to sex, on either side of a narrow gully. The ladies in turn toss a coloured ball to the other side, and whoever catches it is the happy man. It is said they are so skilful in throwing the ball that the favoured man is always sure to catch it; which is reassuring. As in Marco Polo's days, the *couvade* still prevails in some parts. When a child is born, the husband goes to bed for thirty days, and the wife looks after the work. At the conclusion of the paper, Lord Northbrook and Col. Yule paid a well deserved tribute to the late Capt. Gill, Prof. Palmer, and Lieut. Charrington. Capt. Gill, our readers may remember, had himself done some first-rate work on the South-East Chinese frontier, and described it in his "River of Golden Sand;" while Prof. Palmer's loss as an Arabic scholar is almost irretrievable.

SAMOYEDES report to Archangel that they have recently seen, South of Waigatz Island, the wreck of a large vessel crushed in the ice. If the statement be true, and if we remember their never-credited story of the unfortunate *Jeannette*, it is more than probable that the vessel is either the Danish exploring vessel the *Dijmphna*, with Lieut. Hovgaard's expedition, or the Norwegian steamer *Warna* with the Dutch meteorological expedition, bound for Port Dickson, both of which in September last froze in in the Kara Sea, from which place the ice may subsequently have carried the unfortunate vessel to where she now is stated to be. The last intelligence received from Lieut. Hovgaard was dated September 22, and addressed to Herr Aug. Gamil, of Copenhagen, the principal promoter of the expedition, from which it appears that all was then well with both vessels, but that the *Dijmphna* was, when caught in the ice, some considerable distance from shore, in fact in a spot where the whole force of the polar ice, when in drift, would strike her. Herr Aug. Gamil having telegraphed to the Russian Admiralty for any confirmation of the above report, has received a reply that no official information on the subject has been received at St. Petersburg; but that nevertheless instructions would be at once given to the officials on the north coast to scour the same, and gather further particulars. A search party is also being contemplated in Copenhagen, which will, if decided on, be led by M. Larsen, a Dane, who accompanied the American expedition in search of the crew of the *Jeannette*, as the special artist of the *Illustrated London News*.

THE German Government has raised the fund for the scientific exploration of Central Africa and other countries, which in 1882-83 was fixed at 75,000 marks (3750*l.*) to 100,000 marks (5000*l.*) for the financial year 1883-84.

THE AIMS AND METHOD OF GEOLOGICAL INQUIRY[1]

II.

IT will be observed that the results obtained by geologists could not have been arrived at had they confined themselves solely to the detection of resemblances and correspondences between the phenomena of the present and the past. The natural forces have always been the same in kind, if not in degree, and we can often watch the gradual development by their means of products which more or less closely resemble the rocks of our sections. But experimental evidence of this kind takes us only a short way, and we are sooner or later confronted by appearances, which are not reproduced by nature before our eyes. As another example of this I shall adduce one which, although it has far-reaching issues, has yet the merit of being readily comprehended without much preliminary geological knowledge. It is moreover instructive as showing how the imaginative faculty works in a mind trained to clear and steady observation of nature. The fact that a large proportion of the lakes of the world rest in rocky hollows or basins had been long known before it occurred to any one to ask how such rocky hollows had come into existence. The question was first asked and the answer given by Prof. (now Sir) A. C. Ramsay. He had pondered over the problem for years before its solution dawned upon him. None of the ordinary agents of geological change seemed capable of producing the phenomena. The most common of all denuding agents—water—certainly could not do so, for although it may dig long and deep trenches through rocks, water could not scoop out a basin like that occupied by Loch Lomond, or any of our Highland lakes. The tendency of water is, on the contrary, to silt up and to drain such hollows, by deepening the points of exit at their lower ends. Did the hollows in question occupy areas of depression—had they, in short, been formed by unequal subsidences of the ground? Some considerable inland seas, as for example the Dead Sea, and doubtless many larger and smaller sheets of water, owe their origin to local movements of this kind. But it is incredible that all the numerous lakes and lakelets of Northern Alpine regions could have originated in this way. In many cases these lakes are so abundant that it is hard to say of some countries, such as Finland, and large parts of Sweden, and even of our own islands, whether it is land or water that predominates. If all these numerous and closely aggregated rock-basins represent so many local subsidences, then the hard rocks in which most of them appear must have been at the time of their formation in a condition hardly less yielding than dough or putty. It was suggested that the lakes of the Alps and other hilly regions might have been caused, not by local sinkings confined to the valleys themselves, but by a general depression of the central high-grounds and water-sheds. The subsidence of the central mountains would of course entail depression in the upper reaches of the mountain-valleys, and in this way the inclination of those valleys would be reversed—each being converted into an elongated rock-basin. But a little consideration showed that before the lakes of such a region as the Alps could have been produced in this manner, those mountains must have been some 15,000 feet higher than at present. Or to put it the other way, in order to obliterate the Alpine lakes and restore the slopes of the valleys to what, if this hypothesis were true, must have been their original inclination, the Alps would need to be pushed up until they attained twice their present elevation. Now, we are hardly prepared to admit that the Swiss mountains were 30,000 feet high before the glacial period. If our Alpine and Northern lake-basins cannot be attributed to movements of depression, still less can they be accounted for by any system of fractures;—they lie neither in gaping cracks nor on the down-throw sides of dislocations. In a word, a study of the structure, inclination, and distribution of the rock-masses in which our lake-basins appear throws no light upon the origin of those hollows. We probably find in many cases that the position and form of a basin have been influenced in some way by the character of the rocks in which it lies—but we detect no evidence in the rock-masses themselves to account for its production. It is not necessary, however, that I should on this occasion mention each and every cause which has been suggested for the origin of rock-bound hollows. Some of these suggestions are unquestionably well founded. For example, there can be no doubt that certain lakes have been produced by the sudden damming-up of a valley in consequence of a fall of rock from adjoining slopes or cliffs; others, again, occupy holes caused by the falling in of the roofs of caves and subterranean tunnels; while yet others have been formed by a current of lava flowing across a valley and thus ponding back its stream, just as many a temporary sheet of water has been brought into existence in a similar way by the abnormal advance of a glacier. In these and other ways lakes have doubtless originated again and again, but the causes just referred to are all more or less exceptional, and manifestly incapable of producing the phenomena so conspicuous in the lake-regions of Britain, Scandinavia, and the Alps.

Ramsay, to whom the varied phenomena of glacier-regions had been long familiar, was struck by the remarkable fact that freshwater lakes predominate in Northern and Alpine countries, while they are comparatively rare in regions further south and outside of mountainous districts. The great development of lakes in Finland finds no counterpart in the low grounds of southern latitudes. It is in regions where glacial action formerly prevailed that rock-basins are most numerous, and this suggested to Ramsay that in some way or other the lakes of the Alps and the North were connected with glaciation. The final solution of the problem flashed upon him while he was studying the glacial features of Switzerland. His scientific imagination enabled him to reproduce in his own mind the aspect presented by the Alps during the glacial period, when the great mountain-valleys were choked with glacier-ice, which flowed out upon the low grounds of Germany, France, and Northern Italy, so as to cover all the sites of the present lakes. He saw that under such conditions enormous erosion must have been effected by the ice, by means of the rocky rubbish which it dragged on underneath, and that this erosion, other things being equal, would be most intense where the ice was thickest and the ground over which it advanced had the gentlest inclination. Such conditions, he inferred, would be met with somewhere in the lower course of a valley between the steeper descent of its upper reaches and the termination of the glacier. This inference was suggested by the consideration that pressure and erosion would be least when the glacier was flowing upon a steep slope, while at the base of such a slope where the valley flattened out, the ice would tend to heap up, as it were, and produce the maximum amount of pressure and erosion. Thereafter, as the ice continued to flow down its valley, it would become thinner and thinner until it reached its termination—and pressure and erosion would diminish with the gradual attenuation of the glacier. Such conditions, after some time, would necessarily result in the formation of elongated rock-basins, sloping in gradually from either end, and attaining their greatest depth at some point above a line drawn midway between the upper and lower ends of a hollow. There are many other details connected with this most ingenious theory which I cannot touch upon at present. It will be sufficient to say that the observed facts receive from it a simple and satisfactory explanation. Like all other well-based theories, it has been fruitful in accounting for many other phenomena, a study of which has developed it in various directions, and enabled us to understand certain appearances which the theory as at first propounded seemed hardly adequate to explain. As a proof of the soundness of Ramsay's conclusion that ice is capable of excavating large rock-basins, I may mention that his theory has led to the prediction of facts which were not previously known to geologists. He had pointed to the occurrence, in many of the sea-lochs of Western Scotland, of deep rock-bound hollows, which he concluded must have been formed by great valley-glaciers in the same way as the hollows occupied by fresh-water lakes in this and other similarly glaciated countries. Some years later, having discovered that the Outer Hebrides had been glaciated across from side to side by a *mer de glace* flowing outwards from the mainland, and having been satisfied as to the truth of the glacial-erosion theory, I was led by it to suppose that deep rock-basins ought to occur upon the floor of the sea along the inner margin of most of our Western Islands. This expectation was suggested by the simple consideration that those islands, presenting, as they for the most part do, a steep and abrupt face to the mainland, must have formed powerful obstructions to the out-flow of the *mer de glace* in the direction of the Atlantic. This being so, great erosion, I inferred, must have ensued in front of those islands. The lower part of the *mer de glace* which overflowed them would be forced down upon the bed of the sea by the ice continually advancing from behind, and compelled to flow as an under-current along the inner margin of the islands, until it circumvented the obstruction, and resumed the same direction as the upper portion of the *mer de glace*. A subsequent careful examination of the Admiralty's Charts of our western seas, which afford a graphic delineation of the configuration of the sea-bottom, proved that the deduction from Ramsay's theory was perfectly correct. Were that sea-bed to be elevated for a few hundred feet, so as to run off the water, and unite the islands to themselves and the mainland, we should find the surface of the new-born land plentifully diversified with lakes—all occupying the positions which a study of the glaciation of the mainland and islands would have led us to expect. Among the most considerable would be a chain of deep lakes extending along the inner margin of the Outer Hebrides, while many similar sheets of water would appear in front of those islands of the Inner Group that face the deep fiords of our western shores.

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[1] The Inaugural Lecture at the opening of the Class of Geology and Mineralogy in the University of Edinburgh, October 27, 1882, by James Geikie, LL.D., F.R.S. L. and E., Regius Professor of Geology and Mineralogy in the University. Continued from p. 46.

The few examples now given of geological methods of inquiry may suffice to show that the process of reading and interpreting the past in the light of the present necessitates not only accurate observation, but an extensive acquaintance with the mode in which the operations of Nature are carried on. They also serve to show that just as our knowledge of the past increases, so our insight into the present becomes more and more extended. For if it be true that the present is the key to the past. it is not less certain that without that unfolding of the past which a study of the rocks has enabled us to accomplish, we should not only miss the meaning of much that we see going on around us, but we should also remain in nearly complete ignorance of all that is taking place within the crust of our globe. Thus, although our science may be correctly defined as an inquiry into the development of the earth's crust and of the faunas and floras which have successively clothed and peopled its surface—yet that definition is somewhat incomplete. For, as we have seen, this inquiry into the past helps us to understand existing conditions better than we should otherwise do. In this respect it is with geology as with human history. The philosophical historian seeks in the past to discover the germ of the present. He tells us that we cannot hope to understand the complicated structure and relations of a society like ours without a full appreciation of all that has gone before. And so it is in the case of geological history. The present has grown out of the past, and bears myriad marks of its origin, which would either be unobserved or remain totally meaningless to us, were the past a sealed book. No student of physical geography, or of zoology and botany, therefore can afford to neglect the study of geology, if his desire be to acquire a philosophical comprehension of the bearings of those sciences. For it is geology which reveals to us the birth and evolution of our lands and seas—which enables us to follow the succession of life upon the globe, and to supply many of the missing links in that chain, which, as we believe, unites the beginning of life in the far distant past with its latest and highest expression in man. By its aid we track out the many wanderings of living genera and species which have resulted in the present distribution of plants and animals. But for geology, indeed, that distribution would be for the most part inexplicable. How, for example, could we account for the often widely separated colonies of arctic-alpine plants which occur upon the mountains of Middle and Southern Europe? How could these plants possibly have been transferred from their head-quarters in the far north to the hills of Britain, and Middle Germany, to the Alps and the Pyrenees? Not the most prolonged and laborious study of the botanist could ever have solved the problem. But we learn from the geologist that the apparent anomalous distribution of the flora in question is quite what his study of the rocks would have led him to expect. He now, indeed, appeals to the occurrence of those curious colonies of arctic-alpine plants as an additional proof in support of his view that during a comparatively recent period our continent experienced a climate of more than arctic severity. He tells us that at that time the reindeer, the glutton, the arctic fox, the musk ox, and other arctic animals migrated south into France, while a Scandinavian flora clothed the low grounds of Middle Europe. By and by, when the arctic rigour of the climate began to give way, the northern species of plants and animals slowly returned to the high latitudes from which they had been driven. Many plants, however, would meet with similar conditions by ascending the various mountains that lay in the path of retreat, and there they would continue to flourish long after every trace of an arctic-alpine flora had vanished from the low ground. This explanation fully meets the requirements of the case. It leaves none of the facts unaccounted for, but is in perfect harmony with all. But as if to make assurance doubly sure, Dr. Nathorst, a well-known Swedish geologist, recently made a search in the low grounds of Europe for the remains of the arctic-alpine flora, and succeeded in discovering these in many places. He detected leaves of the arctic willow and several other characteristic northern species in the glacial and post-glacial deposits of Southern Sweden, Denmark, England, Germany, and Switzerland, and thus supplied the one link which might have been sidered necessary to complete a chain of evidence already almost perfect.

From this and many similar instance that might be given we learn that the reconstruction of the past out of its own ruins is not mere guess-work and hypothesis. The geologist cannot only demonstrate that certain events have taken place, but he can assure us of the order in which they succeeded one after the other, during ages incalculably more remote than any with which historians have to deal. The written records out of which are constructed the early history of a people cannot always be depended upon—allowance must be made for the influences that may have swayed the chroniclers, and these are either unknown or can only be guessed at. It follows therefore that events are seldom presented to us in a consecutive history exactly as they occurred. They are always more or less coloured, and that colouring often depends fully as much upon the idiosyncrasies of the modern compiler as upon those of the contemporaneous recorder. The geologist has at least this advantage over the investigator of human history, that his records, however fragmentary they may be, tell nothing more and nothing less than the truth. Any errors that arise must be due either to insufficient observation or bad reasoning, or to both, while the progress of research and the penetrating criticism which every novel view undergoes must sooner or later discover where the truth lies. In this way the history of our globe is being gradually reconstructed—to an extent, indeed, that the earlier cultivators of the science could not have believed possible. But although many blanks in the records have been filled up, and our knowledge will doubtless be yet greatly increased, it must nevertheless be admitted that this knowledge must always bear but a very small proportion to our ignorance. In this, however, there is nothing to discourage us, as we may be quite sure that the work remaining to be done will far exceed all the energies of many generations to accomplish.

It is sometimes objected to Geology that its results are not always so exact as those which are obtained by an experimental science like chemistry. We are reproached with the fact that our theoretical conceptions undergo frequent modification, and are, even often abandoned, to be succeeded by others which, after flourishing for a time, are in like manner overturned and thrown aside. But the same reproach, if it be one, might be brought against other sciences. Each advancing science has its problems and speculations. And we cannot often feel assured that the solution now given of those problems will in all cases stand the test of time. Our theoretical conceptions of the ultimate constitution of matter, for example, have within comparatively few years undergone considerable change, and yet no one values chemistry the less. Let our theories be what they may, they do not and cannot overturn the results obtained by verified observation and often repeated and varied experiment. It remains for ever true that water is composed of oxygen and hydrogen, let our views of the atomic theory change as they may. And so it is not less certain that strata of conglomerate and sandstone containing marine or fresh-water fossils are of aqueous origin, however much our theoretical conceptions may vary as to the uniformity in degree between the past and present operations of Nature. It is true we did not see the conglomerate and sandstone in process of formation, but we know by observation that these rocks exactly resemble deposits of gravel and sand which are now being accumulated in water. Nature in this case makes the experiment for us, whereas the chemist has to do this for himself. The latter, having well ascertained by varied experiments the composition of certain samples of water, henceforth concludes that all water is made up of the same two gases in definite proportions. But this conclusion of his is just as much an assumption as the inference of the geologist that strata containing marine or freshwater fossils are aqueous accumulations. It is when we come to the larger generalisations of our science that we are more likely to go astray. The problems we have to solve demand not only an accurate knowledge of widely scattered phenomena, but a ready command of logical analysis. The facts may be sufficiently abundant, but if we reason badly we of course miss their meaning. Or, on the other hand, the evidence may be more or less imperfect. There are blanks which we fill up with conjecture—which can do no harm so long as we do not treat our conjectures as if they were facts. But when the gaps in the evidence are numerous, each theoriser will fill them up after his own fashion, and very various results will thus be obtained. Even in cases of this kind, however, a rigorous application of logical analysis will enable us to detect the fallacies which may underlie all the competing theories; and we are thus prepared to frame a new explanation for ourselves, and to set about searching for additional facts to prove or disprove our notions. In all such investigations it is obviously the duty of a careful observer and theoriser to see well to his premises—to be absolutely sure as to his facts, and to distinguish clearly between what is substantial knowledge, and what is mere conjecture. He will thus be in a position to judge whether his conclusions are based on a solid foundation or not. In a science of observation like geology, theory is necessarily often in advance of the facts. Some, indeed, have insisted that all conjectural explanations are quite a mistake; that it would be better to avoid theorising altogether, and to wait patiently until the chain of evidence had completed itself. I am afraid that, were it possible to follow this advice, we might often have to wait a very long time. After all, a heap of bricks is only a potential house: it will not grow up into walls without the aid of architect and builder. Discoveries in science have no doubt been made occasionally by isolated and haphazard observations; but that is exceptional, and we should not be where we are now had the examination of Nature been always conducted after such a fashion. If additional evidence be required, we must first have some notion where to look for it. In other words, it is essential to progress that we should have preconceived opinions or theories, which enable us to arrange the facts we already possess, and to point out the directions in which further evidence may be looked for. We cannot be too careful, however, that our preconceived notions do not lead us to colour the evidence or to blind us to facts that tell against our views. Every theory should be considered provisional until its truth has been fully demonstrated by an overwhelming array of testimony in its favour. Until this consummation is arrived at we must be constantly testing its truth, and be ready to abandon it at once whenever the evidence shows it to be erroneous. The failure of one theory after another need not disconcert or discourage us; for each failure, by reducing the number of possible explanations, must necessarily bring us nearer to our goal—the truth. I cannot but deem it a strong point in favour of geology as a branch of education that it not only cultivates the faculty of clear and continuous observation, but abounds in unsolved problems which are ever suggesting new ideas and thus stimulating that imagination which is one of the noblest gifts of our race. It is no reproach that the progress of our science is marked by the modification and abandonment of numerous hypotheses and theories. On the contrary these afford a measure of the rate at which geology advandes—just as this last yields the strongest testimony to the good results that accrue from having some provisional view by which to direct the course of our observations.

It is unavoidable that in the onward march of a science the facts become at last so numerous as to task all the energies of its votaries to keep abreast of their time. When a beginner first surveys the wide field embraced by geological inquiry, he may not unnaturally experience a feeling akin to despair. How is it possible, he may think, that I can master all these manifold details—how can I test the truth of all those numerous inferences and conclusions—and yet have sufficient leisure and energy left to undertake original observation? Well, no one can hope to advance the science in all its departments. When we reflect that in order to obtain a complete comprehension and mastery of the existing condition of things we should require to be adepts in physics, mechanics, chemistry, and every branch of natural science, it is obvious that such a perfect knowledge is beyond attainment. It is needless, therefore, that we should strive to become "admirable Crichtons" in this nineteenth century, and no beginner need be discouraged by the greatness of the science which he desires to cultivate. It is only by division of labour that so much has been accomplished; and the results are now so systematised that it is quite possible for any intelligent inquirer to gain a thorough comprehension of the principles of the science. But this it is absolutely necessary to acquire, and the student, therefore, should at first devote all his energies to learn as much as he can of those principles and their application. When he has progressed so far, he is then ready to set out as an explorer in the well-assured hope that if he be true to the logical methods which have hitherto succeeded so well, he will not fail to reap his reward in the discovery of new truths. But to secure success we must be content to be specialists. In other words, we must concentrate our energies upon some particular lines of inquiry, and do our utmost to work these out in all their details. At the same time we should make a great mistake if we did not always keep in mind the broader bearings of our science, and endeavour to maintain as wide a knowledge as we can of all its branches. Each of these, we may be sure, has something to tell which will aid us in our own special inquiries. We cannot, therefore, afford to neglect the side-lights which are thrown upon our path from the lamps of others who are working in adjacent fields. One cannot help thinking that many specialists would have given us more and better work if they had not allowed themselves to be cramped and narrowed by continuing too long in one rut or groove. They dig so deep that they get into a hole out of which it is sometimes difficult to climb, and thus not infrequently the work being done by fellow-labourers, escapes them, and they miss the suggestions which a knowledge of that work might otherwise have yielded them.

I have said nothing as to the practical applications of our science—that branch of our subject which is termed economic geology—not because I consider it the less important, but because its value is generally recognised and need not now be insisted upon. Many, I do not doubt, enter upon their geological studies with a distinct view of obtaining from the science such help as it can afford them in the practical pursuits of life. To such inquirers it will be my pleasure not less than my duty to give every assistance that is in my power. But I would point out to them that there is no short cut to the attainment of the knowledge they are in quest of. The study of economic geology cannot be separated from that of the recognised principles and methods of inquiry which must be followed by the scientific investigator. On the contrary, the more thoroughly we devote ourselves to the prosecution of geology for its own sake the better able shall we be to appreciate its economic bearings.

In beginning the duties of this Chair, if I enjoy certain advantages over my predecessor, I also at the same time labour under considerable disadvantages. The Class Museum formed by him, and the other appliances and aids to teaching which he laboriously gathered together have been generously handed over to the Chair—and this, I need not say, has greatly smoothed my path. But, on the other hand, he has left behind him a reputation which must bear hard upon me. He has not only sustained but increased the fame of what has been termed the Scottish School of Geology, and I feel that it will task all my energies to emulate the high standard he maintained as a teacher. It is not without diffidence, therefore, that I commence this course; but my hope is that the love of science, which has hitherto carried me over many years of a laborious occupation, may at least succeed in warming and sustaining the enthusiasm of those who come here to study with me what geology has to reveal concerning the past and present.

A METHOD FOR OBSERVING ARTIFICIAL TRANSITS[1]

As many astronomers who intend to observe the coming transit of Venus have neither the time nor means for making the necessary arrangements to practice on artificial transits, the simple method here proposed may be advantageously employed. Instead of observing an artificial sun and planet placed at a distance of several thousand feet from the observer, I would suggest that the real sun be observed, and the planet Venus to be represented by a circular disk, held, in the common focus of the objective and eye-piece, by means of a narrow metallic arm fastened to the eye-piece.

The relative motion of the sun and Venus can then be produced by so adjusting the rate of the driving-clock that the angular motion of the telescope on the hour-axis shall exceed the diurnal motion of the sun by seventeen seconds of time per hour. In this way, as the atmospheric disturbances of the sun's limb are real, a near approach to the phenomena observed during an actual transit will result. If a light-shade glass is employed, the opaque disk will be seen before it comes into apparent contact with the sun. The observer can, however, by an exercise of the will, confine his whole attention to the sun's limb.

By using a heavier shade-glass the disk will not be seen until it is projected against the image of the sun. The angular diameter of Venus at the time of transit being about 65″, the diameter of the opaque disk should be $65 \cdot l \cdot \sin 1'' = 0{\cdot}00031 \cdot l$, l being the focal length of the telescope used. The position angle of the point of contact can be changed at will by simply moving the telescope in declination.

ELECTRIC LIGHTING, THE TRANSMISSION OF FORCE BY ELECTRICITY[2]

Having received the honour of being elected Chairman of the Council of the Society of Arts for the ensuing year, the duty devolves upon me of opening the coming Session with some introductory remarks. Only a few months have elapsed since I was called upon to deliver a presidential address to the British Association at Southampton, and it may be reasonably supposed that I then exhausted my stock of accumulated thought and observation regarding the present development of science, both abstract and applied; that, in fact, I come before you, to use a popular phrase, pretty well pumped dry. And yet so large is the field of modern science and industry, that, notwithstanding the good opportunity given me at Southampton, I could there do only scanty justice to comparatively few of the branches of modern progress, and had to curtail, or entirely omit, reference to others, upon which I should otherwise have wished to dwell. There is this essential difference between the British Association and the Society of Arts, that the former can only take an annual survey of the progress of science, and must then confide to individuals, or to committees, specific inquiries, to be reported upon to the different sections at subsequent meetings; whereas the Society of Arts, with its 3,450 permanent members, its ninety-five associated societies, spread throughout the length and breadth of the country, its permanent building, its well-conducted *Journal*, its almost daily meetings and lectures, extending over six months of the year, possesses exceptionally favourable opportunities of following up questions of industrial progress to the point of their practical accomplishment. In glancing back upon its history during the 128 years of its existence, we discover that the Society of Arts was the first institution to introduce science into the industrial arts; it was through the Society of Arts and its illustrious Past President, the late Prince Consort, that the first Universal Exhibition was proposed, and brought to a successful issue in 1851; and it is due to the same Society, supported on all important occasions by its actual President, the Prince of Wales, that so many important changes in our educational and industrial institutions have been inaugurated, too numerous to be referred to specifically on the present occasion.

Amongst the practical questions that now chiefly occupy public attention are those of Electric Lighting, and of the transmission of force by electricity. These together form a subject which has occupied my attention and that of my brothers for a great number of years, and upon which I may consequently be expected to dwell on the present occasion, considering that at Southampton I could deal only with some purely scientific considerations involved in this important subject. I need hardly remind you that electric lighting, viewed as a physical experiment, has been known to us since the early part of the present century, and that many attempts have, from time to time, been made to promote its application. Two principal difficulties have stood in the way of its practical introduction, viz., the great cost of producing an electric current so long as chemical means had to be resorted to, and the mechanical difficulty of constructing electric lamps capable of sustaining, with steadiness, prolonged effects. The dynamo-machine, which enables us to convert mechanical into electrical force, purely and simply, has very effectually disposed of the former difficulty, inasmuch as a properly conceived and well constructed machine of this character converts more than ninety per cent. of the mechanical force imparted to it into electricity, ninety per cent. again of which may be re-converted into mechanical force at a moderate distance. The margin of loss, therefore, does not exceed twenty per cent., excluding purely mechanical losses, and this is quite capable of being further reduced to some extent by improved modes of construction; but it results from these figures that no great step in advance can be looked for in this direction. The dynamo-machine presents the great advantage of simplicity over steam or other power-transmitting engines; it has but one working part, namely, a shaft which, revolving in a pair of bearings, carries a coil or coils of wire admitting of perfect balancing. Frictional resistance is thus reduced to an absolute minimum, and no allowance has to be made for loss by condensation, or badly fitting pistons, stuffing boxes, or valves, or for the jerking action due to oscillating weights. The materials composing the machine, namely, soft iron and copper wire, undergo no deterioration or change by continuous working, and the depreciation of value is therefore a minimum, except where currents of exceptionally high potential are used, which appear to render the copper wire brittle.

The essential points to be attended to in the conception of the dynamo-machine, are the prevention of induced currrents in the iron, and the placing of the wire in such position as to make the whole of it effective for the production of outward current. These principles, which have been clearly established by the labours of comparative few workers in applied science, admit of being carried out in an almost infinite variety of constructive forms, for each of which may be claimed some real or imaginary merits regarding questions of convenience or cost of production.

For many years after the principles involved in the construction of dynamo-machines had been made known, little general interest was manifested in their favour, and few were the forms of construction offered for public use. The essential features involved in the dynamo-machine, the Siemens armature (1856), the Pacinotti ring (1861), and the self-exciting principle (1867), were published by their authors for the pure scientific interest attached to them, without being made subject matter of letters patent, which circumstance appears to have had the contrary effect of what might have been expected, in that it has retarded the introduction of this class of electrical machine, because no person or firm had a sufficient commercial interest to undertake the large expenditure which must necessarily be incurred in reducing a first conception into a practical shape. Great credit is due to Monsieur Gramme for taking the initiative in the practical introduction of dynamo-machines embodying those principles, but when five years ago I ventured to predict for the dynamo-electric current a great practical future, as a means of transmitting power to a distance, those views were still looked upon as more or less chimerical. A few striking examples of what could be practically effected by the dynamo-electric current such as the illumination of the Place de l'Opera, Paris, the occasional exhibition of powerful arc lights, and their adoption for military and lighthouse purposes, but especially the gradual accomplishment of the much desired lamp by incandescence in vacuum, gave rise to a somewhat sudden reversion of public feeling; and you may remember the scare at the Stock Exchange affecting the value of gas shares, which ensued in 1878, when the accomplishment of the sub-division of the electric light by incandescent wire was first announced, somewhat prematurely, through the Atlantic cable.

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[1] By Prof. J. M. Schaeberle, Ann Arbor, Michigan From the *American Journal of Science*.

[2] Address by Dr C. W. Siemens, F.R.S., Chairman of the Society of Arts, November 15.

From this time forward electric lighting has been attracting more and more public attention, until the brilliant displays at the exhibition of Paris, and at the Crystal Palace last year, served to excite public interest, to an extraordinary degree. New companies for the purpose of introducing electric light and power have been announced almost daily, whose claims to public attention as investments were based in some cases upon only very slight modifications of well-known forms of dynamo-machines, of arc regulators, or of incandescent carbon lights, the merits of which rested rather upon anticipations than upon any scientific or practical proof. These arrangements were supposed to be of such superlative merit that gas and other illuminants must soon be matters simply of history, and hence arose great speculative excitement. It should be borne in mind, however, that any great technical advance is necessarily the work of time and serious labour, and that when accomplished, it is generally found that so far from injuring existing industries, it calls additional ones into existence, to supply new demands, and thus gives rise to an increase in the sum total of our resources. It is, therefore, reasonable to expect that side by side with the introduction of the new illuminant, gas lighting will go on improving and extending, although the advantage of electric light for many applications, such as the lighting of public halls and warehouses, of our drawing rooms and dining-rooms, our passenger steamers, our docks and harbours, are so evident, that its advent may be looked upon as a matter of certainty.

Our Legislature has not been slow in recognising the importance of the new illuminant. In 1879, a Select Committee in the House of Commons instituted a careful inquiry into its nature and probable cost, with a view to legislation, and the conclusions at which they arrived were, I consider, the best that could have been laid down. They advised that applications should be encouraged tentatively by the granting of permissive Bills, and this policy has given rise to the Electric Lighting Bill, 1882, promoted by Mr. Chamberlain, the President of the Board of Trade, regarding which much controversy has arisen. It could, indeed, hardly be expected that any act of legislation upon this subject could give universal satisfaction, because while there are many believers in gas who would gladly oppose any measure likely to favour the progress of the rival illuminant, and others who wish to see it monopolised, either by local authorities, or by large financial corporations, there are others again who would throw the doors open so wide as to enable almost all comers to interfere with the public thoroughfares, for the establishment of conducting wires, without let or hindrance.

The law as now established takes, I consider, a medium course between these diverging opinions, and, if properly interpreted, will protect, I believe, all legitimate interests, without impeding the healthy growth of establishments for the distribution of electric energy for lighting and for the transmission of power. Any firm or lighting company may, by application to the local authorities, obtain leave to place electric conductors below public thoroughfares, subject to such conditions as may be mutually agreed upon, the terms of such license being limited to seven years; or an application may be made to the Board of Trade for a provisional order to the same effect, which, when sanctioned by Parliament, secures a right of occupation for twenty-one years. The license offers the advantage of cheapness, and may be regarded as a purely tentative measure, to enable the firm or company to prove the value of their plant. If this is fairly established, the license would in all probability be affirmed, either by an engagement for its prolongation from time to time, or by a provisional order which would, in that case, be obtained by joint application of the contractor and the local authority. At the time of expiration of the provisional order, a pre-emption of purchase is accorded to the local authority, against which it has been objected with much force by so competent an authority as Sir Frederick Bramwell, that the conditions of purchase laid down are not such as fairly to remunerate the contracting companies for their expenditure and risk, and that the power of purchase would inevitably induce the parochial bodies to become mere trading associations. But while admitting the undesirability of such a consummation, I cannot help thinking that it was necessary to put some term to contracts entered into with speculative bodies at a time when the true value of electric energy, and the best conditions under which it should be applied, are still very imperfectly understood. The supply of electric energy, particularly in its application to transmission of power, is a matter simply of commercial demand and supply, which need not partake of the character of a large monopoly similar to gas and water supply, and which may therefore be safely left in the hands of individuals, or of local associations, subject to a certain control for the protection of public interests. At the termination of the period of the provisional order, the contract may be renewed upon such terms and conditions as may at that time appear just and reasonable to Parliament, under whose authority the Board of Trade will be empowered to effect such renewal.

Complaints appear almost daily in the public papers to the effect that townships refuse their assent to applications by electric light companies for provisional orders; but it may be surmised that many of these applications are of a more or less speculative character, the object being to secure monopolies for eventual use or sale, under which circumstances the authorities are clearly justified in withholding their assent; and no licenses or provisional orders should, indeed, be granted, I consider, unless the applicants can give assurance of being able and willing to carry out the work within a reasonable time. But there are technical questions involved which are not yet sufficiently well understood to admit of immediate operations upon a large scale.

Attention has been very properly called to the great divergence in the opinions expressed by scientific men regarding the area that each lighting district should comprise, the capital required to light such an area, and the amount of electric tension that should be allowed in the conductors. In the case of gas supply, the works are necessarily situated in the outskirts of the town, on account of the nuisance this manufacture occasions to the immediate neighbourhood; and, therefore, gas supply must range over a large area. It would be possible, no doubt, to deal with electricity on a similar basis, to establish electrical mains in the shape of copper rods of great thickness, with branches diverging from it in all directions; but the question to be considered is, whether such an imitative course is desirable on account either of relative expense or of facility of working. My own opinion, based upon considerable practical experience and thought devoted to the subject, is decidedly adverse to such a plan. In my evidence before the Parliamentary Committee, I limited the desirable area of an electric district in densely populated towns to a quarter of a square mile, and estimated the cost of the necessary establishment of engines, dynamo-machines, and conductors, at 100,000*l.* while other witnesses held that areas from one to four square miles could be worked advantageously from one centre, and at a cost not exceeding materially the figure I had given. These discrepancies do not necessarily imply wide differences in the estimated cost of each machine or electric light, inasmuch as such estimates are necessarily based upon various assumptions regarding the number of houses and of public buildings comprised in such a district, and the amount of light to be apportioned to each, but I still maintain my preference for small districts.

By way of illustration, let us take the parish of St. James's, near at hand, a district not more densely populated than other equal areas within the metropolis, although comprising, perhaps, a greater number of public buildings. Its population, according to the preliminary report of the census taken on the 4th April, 1881, was 29,865, it contains 3,018 inhabited houses, and its area is 784,000 square yards, or slightly above a quarter of a square mile.

To light a comfortable house of moderate dimensions in all its parts, to the exclusion of gas, oil, or candles, would require about 100 incandescent lights of from 15 to 18-candle power each, that being, for instance, the number of Swan lights employed by Sir William Thomson in lighting his house at Glasgow University. Eleven-horse power would be required to excite this number of incandescent lights, and at this rate the parish of St. James's would require 3,018 × 11 = 33,200-horse power to work it. It may be fairly objected, however, that there are many houses in the parish much below the standard here referred to, but on the other hand there are 600 of them with shops on the ground floor, involving larger requirements. Nor does this estimate provide for the large consumption of electric energy that would take place in lighting the eleven churches, eighteen club-houses, nine concert halls, three theatres, besides numerous hotels, restaurants, and lecture halls. A theatre of moderate dimensions, such as the Savoy Theatre, has been proved by experience to require 1,200 incandescent lights, representing an expenditure of 133 horse power; and about one-half that power would have to be set aside for each of the other public buildings here mentioned, constituting an aggregate of 2,926-horse power; nor does this general estimate comprise street lighting, and to light the six and a half miles of principal streets of the parish with electric light, would require per mile, thirty-five arc lights of 350-candle power each, or a total of 227 lights. This, taken at the rate of 0·8-horse power per light, represents a further requirement of 182-horse power, making a total of 3,108-horse power, for purposes independent of house lighting, being equivalent to one-horse power per inhabited house, and bringing the total requirements up to 109 lights = 12-horse power per house.

I do not, however, agree with those who expect that gas lighting will be entirely superseded, but have, on the contrary, always maintained that the electric light, while possessing great and peculiar advantages for lighting our principal rooms, halls, warehouses, &c., owing to its brilliancy, and more particularly to its non-interference with the healthful condition of the atmosphere, will leave ample room for the development of the former, which is susceptible of great improvement, and is likely to hold its own for the ordinary lighting up of our streets and dwellings.

Assuming, therefore, that the bulk of domestic lighting remains to the gas companies, and that the electric light is introduced into private houses, only, at the rate of, say twelve incandescent lights per house, the parish of St. James's would have to be provided with electric energy sufficient to work (9 + 12) × 3,018 = 63,378 lights = 7,042-horse power effective; this is equal to about one-fourth the total lighting power required, taking into account that the total number of lights that have to be provided for a house are not all used at one and the same time. No allowance is made in this estimate for the transmission of power, which, in course of time, will form a very large application of electric energy; but considering that power will be required mostly in the day time, when light is not needed, a material increase in plant will not be necessary for that purpose.

In order to minimise the length and thickness of the electric conductor, it would be important to establish the source of power, as nearly as may be, in the centre of the parish, and the position that suggests itself to my mind is that of Golden-square. If the unoccupied area of this square, representing 2,500 square yards, was excavated to a depth of twenty-five feet, and then arched over so as to re-establish the present ground level, a suitable covered space would be provided for the boilers, engines, and dynamo-machines, without causing obstruction or public annoyance; the only erection above the surface would be the chimney, which, if made monumental in form, might be placed in the centre of the square, and be combined with shafts for ventilating the subterranean chamber, care being taken of course to avoid smoke by insuring perfect combustion of the fuel used. The cost of such a chamber, of engine power, and of dynamo-machines, capable of converting that power into electric energy, I estimate at 140,000*l*. To this expense would have to be added that of providing and laying the conductors, together with the switches, current regulators, and arrangements for testing the insulation of the wire.

The cost and dimensions of the conductors would depend upon their length, and the electromotive force to be allowed. The latter would no doubt be limited, by the authorities, to the point at which contact of the two conductors with the human frame would not produce injurious effects, or say to 200 volts, except for street lighting, for which purpose a higher tension is admissible. In considering the proper size of conductor to be used in any given installation, two principal factors have to be taken into account; first, the charge for interest and depreciation on the original cost of a unit length of the conductor; and, secondly, the cost of the electrical energy lost through the resistance of a unit of length. The sum of these two, which may be regarded as the cost of conveyance of electricity, is clearly least, as Sir William Thomson pointed out some time ago, when the two components are equal. This, then, is the principle on which the size of a conductor should be determined.

From the experience of large installations, I consider that electricity can, roughly speaking, be produced in London at a cost of about one shilling per 10,000 Ampère-Volts or Watts (746 Watts being equal to one horse-power) for an hour. Hence, assuming that each set of four incandescent lamps in series (such as Swan's, but for which may be substituted a smaller number of higher resistance and higher luminosity) requires 200 volts electromotive force, and 60 Watts for their efficient working, the total current required for 64,000 such lights is 19,200 ampères, and the cost of the electric energy lost by this current in passing through 1·100th of an ohm resistance, is 16*l*. per hour.

The resistance of a copper bar one quarter of a mile in length, and one square inch in section, is very nearly 1·100th of an ohm, and the weight is about 2⅓ tons. Assuming, then, the price of insulated copper conductor at 90*l*. per ton, and the rate of interest and depreciation at 7½ per cent., the charge per hour of the above conductor, when used eight hours per day, is 1½*d*. Hence, following the principle I have stated above, the proper size of conductor to use for an installation of the magnitude I have supposed, would be one of 48·29 inches section, or a round rod eight inches diameter.

If the mean distance of the lamps from the station be assumed as 350 yards, the weight of copper used in the complete system of conductors would be nearly 168 tons, and its cost 15,120*l*. To this must be added the cost of iron pipes, for carrying the conductors underground, and of testing boxes, and labour in placing them. Four pipes of 10 inch diameter each, would have to proceed in different directions from the central station, each containing sixteen separate conductors of one inch diameter, and separately insulated, each of them supplying a sub-district of 1,000 lights. The total cost of establishing these conductors may be taken at 37,000*l*., which brings up the total expenditure for central station and leads to 177,000*l*. I assume the conductors to be placed underground, as I consider it quite inadmissible, both as regards permanency and public safety and convenience, to place them above ground, within the precincts of towns. With this expenditure, the parish of St. James's would be supplied with the electric light to the extent of about 25 per cent, of the total illuminating power required. To provide a larger percentage of electric energy would increase the cost of establishment proportionately; and that of conductors, nearly in the square ratio of the increase of the district, unless the loss of energy by resistance is allowed to augment instead.

It may surprise uninitiated persons to be told that to supply a single parish with electric energy necessitates copper conductors of a collective area equal to a rod of eight inches in diameter; and how, it may be asked, will it be possible under such conditions to transmit the energy of waterfalls to distances of twenty or thirty miles, as has been suggested? It must indeed be admitted that the transmission of electric energy of such potential (200 volts) as is admissible in private dwellings would involve conductors of impracticable dimensions, and in order to transmit electrical energy to such distances, it is necessary to resort in the first place to an electric current of high tension. By increasing the tension from 200 to 1,200 volts the conductors may be reduced to one-sixth their area, and if we are content to lose a larger proportion of the energy obtained cheaply from a waterfall, we may effect a still greater reduction. A current of such high potential could not be introduced into houses for lighting purposes, but it could be passed through the coils of a secondary dynamo-machine, to give motion to another primary machine, producing currents of low potential to be distributed for general consumption. Or secondary batteries may be used to effect the conversion of currents of high into those of low potential, whichever means may be found the cheaper in first cost, in maintenance, and most economical of energy. It may be advisable to have several such relays of energy for great distances, the result of which would be a reduction of the size and cost of conductor at the expense of final effect, and the policy of the electrical engineer will, in such cases, have to be governed by the relative cost of the conductor, and of the power at its original source. If secondary batteries should become more permanent in their action than they are at the present time, they may be largely resorted to by consumers, to receive a charge of electrical energy during the day time, or the small hours of the night, when the central engine would otherwise be unemployed, and the advantage of resorting to these means will depend upon the relative first cost, and cost of working the secondary battery and the engine respectively. These questions are, however, outside the range of our present consideration.

The large aggregate of dwellings comprising the metropolis of London covers about seventy square miles, thirty of which may be taken to consist of parks, squares, and sparsely inhabited areas, which are not to be considered for our present purpose. The remaining forty square miles could be divided into say 140 districts, slightly exceeding a quarter of a square mile on the average, but containing each fully 3,000 houses, and a population similar to that of St. James's.

Assuming twenty of these districts to rank with the parish of St. James's (after deducting the 600 shops which I did not include in my estimate) as central districts, sixty to be residential districts, and sixty to be comparatively poor neighbourhoods, and estimating the illuminating power required for these three classes in the proportion of 1 to $\frac{2}{3}$ to $\frac{1}{3}$, we should find that the total capital expenditure for supplying the metropolis with electric energy to the extent of 25 per cent. of the total lighting requirements would be—

$$\begin{array}{rcl} 20 \times 177{,}000 &=& 3{,}540{,}000l. \\ 60 \times \frac{2}{3} \times 177{,}000 &=& 7{,}080{,}000l. \\ 60 \times \frac{1}{3} \times 177{,}000 &=& 3{,}540{,}000l. \\ \hline && 14{,}160{,}000l. \end{array}$$

or say 14,000,000*l.*, without including lamps and internal fittings, and making an average capital expenditure of 100,000*l.* per district.

To extend the same system over the towns of Great Britain, and Ireland would absorb a capital exceeding certainly 64,000,000*l.*, to which must be added 16,000,000*l.* for lamps and internal fittings, making a total capital expenditure of 80,000,000*l.* Some of us may live to see this capital realised, but to find such an amount of capital, and, what is more important, to find the manufacturing appliances to produce work representing this value of machinery and wire, must necessarily be the result of many years of technical development. If, therefore, we see that electric companies apply for provisional orders to supply electric energy, not only for every town throughout the country, but also for the colonies, and for foreign parts, we are forced to the conclusion that their ambition is somewhat in excess of their power of performance; and that no provisional order should be granted except conditionally on the work being executed within a reasonable time, as without such a provision the powers granted may have the effect of retarding instead of advancing electric lighting, and of providing an undue encouragement to purely speculative operations.

The extension of a district beyond the quarter of a square mile limit, would necessitate an establishment of unwieldy dimensions, and the total cost of electric conductors per unit area would be materially increased; but independently of the consideration of cost, great public inconvenience would arise in consequence of the number and dimensions of the electric conductors, which could no longer be accommodated in narrow channels placed below the kerb stones, but would necessitate the construction of costly subways—veritable *cava electrica.*

The amount of the working charges of an establishment comprising the parish of St. James's would depend on the number of working hours in the day, and on the price of fuel per ton. Assuming the 64,000 lights to incandesce for six hours a day, the price of coal to be 20*s.* a ton, and the consumption 2lbs. per effective horse power per hour, the annual charge under this head, taking eight hours' firing, would amount to about 18,300*l.*, to which would have to be added for wages, repairs, and sundries, about 6,000*l.*, for interest with depreciation at seven-and-a-half per cent., 13,300*l.*, and for general management say, 3,400*l.*, making a total annual charge of 41,000*l.*, or at the rate of 12*s.* 9½*d.* per incandescent lamp per annum. To this has to be added the cost of renewal of lamps, which may be taken at 5*s.* per lamp of sixteen candles, lasting 1,200 hours, or to 9*s.* per annum, making a total of 21*s.* 9½d. per lamp for a year.

In comparing these results with the cost of gas-lighting, we shall find that it takes 5 cubic feet of gas, in a good argand burner, to produce the same luminous effect as one incandescent light of 16-candle power. In lighting such a burner every day for six hours on the average, we obtain an annual gas consumption of 10,950 cubic feet, the value of which, taken at the rate of 2*s.* 8*d.* per thousand, represents an annual charge of 29*s.*, showing that electric light by incandescence, when carried out on a large scale, is decidedly cheaper than gas-lighting at present prices, and with the ordinary gas-burners.

On the other hand, the cost of establishing gas-works and mains of a capacity equal to 64,000 argand burners would involve an expenditure not exceeding 80,000*l.* as compared with 177,000*l.* in the case of electricity; and it is thus shown that although it is more costly to establish a given supply of illuminating power by electricity than gas, the former has the advantage as regards current cost of production.

It would not be safe, however, for the advocates of electric lighting to rely upon these figures as representing a permanent state of things. In calculating the cost of electric light, I have only allowed for depreciation and 5 per cent. interest upon capital expenditure, whereas gas companies are in the habit of dividing large dividends, and can afford to supply gas at a cheaper rate, by taking advantage of recent improvements in manufacturing operations, and of the ever-increasing value of their by-products, including tar, coke, and ammoniacal liquor. Burners have, moreover, been recently devised by which the luminous effect for a given expenditure of gas can be nearly doubled by purely mechanical arrangements, and the brilliancy of the light can be greatly improved.

On the other hand, electric lighting also may certainly be cheapened by resorting, to a greater extent than has been assumed, to arc lighting, which though less agreeable than the incandescent light for domestic purposes, can be produced at less than half the cost, and deserves on that account the preference for street lighting, and for large halls, in combination with incandescent lights. Lamps by incandescence may be produced hereafter at a lower cost, and of a more enduring character.

Considering the increasing public demand for improved illumination, it is not unreasonable to expect that the introduction of the electric light to the full extent here contemplated, would go hand in hand with an increasing consumption of gas for illuminating and for heating purposes, and the neck-to-neck competition between the representatives of the two systems of illumination, which is likely to ensue, cannot fail to improve the quality, and to cheapen the supply of both, a competition which the consuming public can afford to watch with complacent self-satisfaction. Electricity must win the day, as the light of luxury; but gas will, at the same time, find an ever-increasing application for the more humble purposes of diffusing light.

In my address to the British Association I dwelt upon the capabilities and prospects of gas, both as an illuminant and as a heating agent, and I do not think that I was over-sanguine in predicting for this combustible a future exceeding all present anticipations.

I also called attention to the advantages of gas as a heating agent, showing that if supplied specially for the purpose, it would become not only the most convenient, but by far the cheapest form of fuel that can be supplied to our towns. Such a general supply of heating separately from illuminating gas, by collecting the two gases into separate holders during the process of distillation, would have the beneficial effects—

1. Of giving to lighting gas a higher illuminating power.
2. Of relieving our towns of their most objectionable traffic—that in coal and ashes.
3. Of effecting the perfect cure of that bugbear of our winter existence—the smoke nuisance.
4. Of largely increasing the production of those valuable by-products, tar, coke, and ammonia, the annual value of which already exceeds by nearly 3,000,000*l.* that of the coal consumed in the gas-works.

The late exhibitions have been beneficial in arousing public interest in favour of smoke abatement, and it is satisfactory to find that many persons, without being compelled to do so, are now introducing perfectly smokeless arrangements for their domestic and kitchen fires.

The Society of Arts, which for more than 100 years has given its attention to important questions regarding public health, comfort, and instruction, would, in my opinion be the proper body to examine thoroughly into the question of the supply and economical application of gas and electricity for the purposes of lighting, of power production, and of heating. They would thus pave the way to such legislative reform as may be necessary to facilitate the introduction of a national system.

If I can be instrumental in engaging the interest of the Society in these important questions, especially that of smoke prevention, I shall vacate this chair next year with the pleasing consciousness that my term of office has not been devoid of a practical result.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—In the Higher Local Examination, in which the majority of the candidates are women, there was a notable falling off this year in the number of candidates in the Natural Science group of subjects. In 1880 there were 99, and 26 failed; in 1881 there were 89, and 17 failed; in 1882, only 39, and 9 failed. The total number of candidates increased from 882 in 1881 to 961 in 1882. The examiners' reports do not indicate any special falling off in the attainments shown by the candidates. In the elementary paper (including Physics, and Biology) the results were not particularly satisfactory. Confusion in the use of terms was common, and the inability to use chemical formulæ was very marked in some cases. In Physiology mistakes were made with regard to subjects of great practical interest, and many of them might have been avoided by reference to every-day experience. In Chemistry the theory was better understood than practical laboratory details.

A supplementary local examination was held in September, for the benefit of candidates seeking exemption from the Previous Examination, and of others desiring to become medical students, &c. Nineteen intending medical students entered, none of whom satisfied the requirements of the General Medical Council.

The Fellows elected at St. John's College last week included Prof. W. J. Sollas, 1st class in the Natural Science Tripos, 1873, Professor of Geology in University College, Bristol, and author of many valuable geological and palæontological memoirs; Mr. J. S. Yeo, Second Wrangler and Second Smith's Prizeman, 1882.

Dr. Hans Gadow will conduct an advanced class in the Morphology of the Vertebrata at the New Museums during the remainder of the present term.

The Members appointed by the Senate on the General Board of Studies, on which much important work will henceforth devolve, are Messrs. Bradshaw (University Librarian), J. Peile, Prof. Cayley, Aldis Wright, Dr. Parkinson, Coutts Trotter, Dr. Phear (Master of Emmanuel College), and Prof. Stuart.

The special Boards of Studies relating to Natural Sciences have selected the following representatives on the General Board of Studies:—Medicine, Prof. Paget; Mathematics, Dr. Ferrers; Physics and Chemistry, Prof. Liveing; Biology and Geology; Music, Mr. Sedley Taylor.

Prof. Stuart has issued his address as the liberal candidate for the University, in succession to the Right Hon. Sir H. Walpole, who proposes to resign.

SCIENTIFIC SERIALS

The American Journal of Science, October.—Notes on physiological optics, No. 5.—Vision by the light of the electric spark, by W. L. Stevens.—Crystals of monazite from Alexander county, North Carolina, by E. S. Dana.—Occurrence and composition of some American varieties of monazite, by S. L. Penfield.—Irregularities in the amplitude of oscillation of pendulums, by C. S. Peirce.—The Deerfield dyke and its minerals, by B. K. Emerson.—Occurrence of *Siphonotreta scotica* in the Utica formation near Ottawa, Ontario, by J. F. Whiteaves.—A recent species of *Heteropora*, from the Strait of Juan de Fuca, by the same.—Notes on interesting minerals occurring near Pike's Peak, Colorado, by W. Cross and W. F. Hillebrand.

Journal of the Asiatic Society of Bengal, vol. 4, part 2, No. 1 (August 31, 1882), contains: On a collection of Japanese Clausiliæ made by Surgeon R. Hungerford in 1881, by Dr. O. F. von Möllendorff (plate 1); out of 21 species, 10 are described as new. Also, by the same author, on *Clausilia nevilliana*, a new species from the Nicobars, and descriptions of three new Asiatic Clausiliæ.—Second list of Diurnal Lepidoptera from the Nicobars, by J. Wood-Mason and L. de Nicéville (plate 3).—On some new or little-known Mantodea, by J. Wood-Mason.

Bulletin de l'Academie Royale des Sciences de Belgique, No. 8.—On the new note of M. Dupont concerning his re-vindication of priority of M. Dewalque.—On the means proposed for calming the waves of the sea, by M. Van der Mensbrugghe.—On the dilatation of some isomorphous salts, by M. Spring.—Notes of comparative physiology, by M. Fredericq.—On some brominated derivatives of camphor, by M. de la Royère.—On the central bone of the carpus in mammalia, by M. Lebourcq.—Action of chlorine on sulphonic combinations, and on organic oxysulphides, by MM. Spring and Wissinger.

Verhandlungen der Naturforschenden Gesellschaft in Basel, Theil 7, Heft 1, 1882, contains: Studies on the history of the deer family, No. 1.—The skull structure, by L. Rütimeyer.—Studies on *Talpa europæa*, by Dr. J. Kober. The literature is given in detail, followed by notes on the mole's place in the order, its local names and habits, and on its anatomy and development (plates 1 and 2, chiefly relating to dentition and embryos).—First supplement to the Catalogue of the Collection of Reptiles in the Basle Museum, by F. Müller. Notes are appended to some of the rarer species, and a new genus and species (*Tropidocephalus azureus*) are indicated for a form allied to *Leiodera chilensis*, Gray, taken in Uruguay; it is figured on plate 3. The register of the collection to December, 1881 indicates 933 species.—On the hail-storm of June 29, 1879, by E. Haigenbach-Bischoff and others.—On the explosive powers of ice and on the Gletscherkorn, by E. H. Bischoff.—Meteorological Report for 1881, with reports by L. Rutimeyer on the comparative anatomy collections, and by F. Burckhardt and R. Holtz, on the map collection of the Society.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, November 9.—Mr. S. Roberts, F.R.S., president, in the chair.—After the reading of the Treasurer's and Secretaries' reports, the Chairman briefly touched upon the loss the Society had sustained during the recess, by the death of Prof. W. Stanley Jevons, F.R.S.—After the ballot for the Council of the ensuing session had been taken, Prof. Henrici, F.R.S., the newly elected president, took the chair, and called upon Mr. Roberts to read his address, which was entitled, "Remarks on Mathematical Terminology and the Philosophical Bearing of Recent Mathematical Speculations concerning the Realities of Space."—Mr. W. M. Hicks was admitted into the Society.—The following communications were made:—On in- and circumscribed polyhedra, Prof. Forsyth.—Note on quartic curves in space, Dr. Spottiswoode, P.R.S.—Note on the derivation of elliptic function formulæ from confocal conics, Mr. J. Griffiths.—On the explicit integration of certain differential resolvents, Sir J. Cockle, F.R.S.—On compound determinants, Mr. R. F. Scott.—On unicursal twisted quartics, Mr. R. A. Roberts.

Geological Society, November 1.—J. W. Hulke, F.R.S., president, in the chair.—Prof. Louis Lartet, of Toulouse, was elected a Foreign Correspondent of the Society.—The following communications were read:—The Hornblendic and other schists of the Lizard District, with some additional notes on the Serpentine, by Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S. The author described the metamorphic series, chiefly characterised by hornblendic schist, which occupies the southern portion of the Lizard and an extensive tract to the north of the serpentine region, besides some more limited areas. He found that this series was separable into a lower or micaceous group—schists with various green minerals (often a variety of hornblende), or with brownish mica; a middle or hornblendic group, characterised by black hornblende; and an upper or granulitic group, characterised by bands of quartz-felspar rock, often resembling in appearance a vein-granite. These are all highly metamorphosed; yet the second and third occasionally retain to a remarkable extent indications of the minuter bedding structures, such as alternating lamination and current bedding of various kinds. They form, in the author's opinion, one continuous series, of which the uppermost is the thinnest. The general strike of the series, though there are many variations, is either north-west or west-north-west. The junctions of the Palæozoic with the metamorphic series at Polurrian and at Porthalla were described. These are undoubtedly faulted: and the two rocks differ greatly, the former being a slate like any ordinary Palæozoic rock, the other a highly metamorphosed schist. Moreover, fragments of the hornblende schist and a kind of gneiss occur in a conglomerate in the former, south of Nare Point. The author considers the metamorphic series (the microscopic structure of which was fully described) undoubtedly Archæan, and probably rather early in that division. The rocks of the micaceous group have considerable resemblance in the greenish and lead-coloured schists of Holyhead Island and the adjoining mainland of Anglesey, and of the Menai Strait. Two outlying areas of serpentine, omitted in his former paper, were described—one at Polkerris, the other at Porthalla. The latter shows excellent junctions, and is clearly intrusive in the schist. The author stated that he had re-examined a large part of the district described in his former paper, and had obtained additional evidence of the intrusion of the serpentine into the sedimentary rock with which it is associated. This evidence is of so strong a nature that he could not conceive the possibility of any one who would carefully examine the district for himself, entertaining a doubt upon the matter.—Notes on some Upper Jurassic Astrorhizidæ and Lituolidæ, by Dr. Rudolf Häusler, F.G.S.

PARIS

Academy of Sciences, November 6.—M. Blanchard in the chair.—The following papers were read:—On the comparative observation of telluric and metallic lines as a means of estimating the absorbent powers of the atmosphere, by M. Cornu. He selects telluric lines (caused by aqueous vapour, and varying in intensity with the amount of it) near D, the scale being four times as large as Ångström's. Metallic lines, for comparison, are indicated; also a method of deducing the total quantity of vapour.—Results of experiments made at the exhibition of electricity, &c. (continued), by M. Allard and others. Three more systems are here discussed.—On M. Siemens' new theory of the sun, by M. Hirn. The recombination of the elements dissociated in space could occur only at a notable distance from the sun's photosphere, and on falling into this they must be anew entirely dissociated, an action which would cost the heat developed by combination. Again, the work done by solar radiation in dissociation must reduce the intensity of radiation; so that the brightness of the sun, stars, and planets should diminish much more rapidly than inversely as the square of the distances. M. Hirn also supports M. Faye's objections by numerical examples.—On the functions of seven letters, by M. Brioschi.—The earthquake of the Isthmus of Panama, by M. de Lesseps. The phenomena (of which he gives a scientific account) seem to have been much exaggerated. The character of comparative immunity of the isthmus (as compared with regions near) is not seriously affected; and in any case, the construction of a maritime canal without locks is justified. There is no ground for apprehension as to the banks of the canal.—M. Peligot presented a "Treatise of Analytical Chemistry applied to Agriculture," and indicated its scope.—MM. de la Tour du Breuil addressed a further note regarding their process for separation of sulphur; they have modified the process so that it is applicable either to resistant or to pulverulent ores.—On the comet observed in Chili in September, by M. de Bernardières.—On the great southern comet observed at the Imperial Observatory of Rio de Janeiro, by M. Cruls. *Inter alia*, he refers to the aspect of the tail as of a current of extremely bright light, in which were distinct bright lines. Behind the nucleus was a dark space, and one was reminded of a bridge-pile in a strong current. The tail extending a length of 12°, seemed suddenly interrupted, and the extension for 15° beyond was of much less width and brightness. Sodium and carbon lines were observed in the spectrum.—On the functions of the genus zero and of genus one, by M. Laguerre. —On a result of calculation obtained by M. Allégret, by M. MacMahon.—On the relation between the electromotive force of a dynamo-electric machine and its velocity of rotation, by M. Levy.—Spectrophotometric measurements of different points of the solar disc, by MM. Gouy and Thollon. They could measure separately the 200,000th part of the solar disc, and the thousandth part of the spectrum. The figures obtained show the decrease of radiation on approaching the limb (greater the more refrangible the rays). The method is also applied to spots.—On the comparison of mercury thermometers with the hydrogen thermometer, by M. Crafts. Fifteen Paris thermometers examined (the crystal containing 18 per cent. lead oxide) behaved like the thermometers of ordinary glass studied by Regnault, but very unlike those of Choisy-le-Roy crystal (with nearly twice as much oxide). A German thermometer of soda-glass gave a curve much nearer the mean than many others of Paris crystal.—On a hydrate of molybdic acid, $MoO_3 2HO$, by M. Parmentier.—On the transformation, in cold, of the blood of animals into solid and inodorous manure, by a new ferric sulphate, by M. Marguerite-Delacharlonny. This sulphate has the formula $Fe_2O_3 4SO_3$. With it the elimination of the water attains nearly one-half. It forms a hydrate which crystallises easily, and dissolves readily in heat. On adding a solution of the sulphate to fresh blood, the latter forms in a few seconds a firm elastic paste. It is then treated in a hydraulic press, and forms a sort of cake.—Researches on the passage of alcoholic liquor through porous bodies, by M. Gal. His experiments show the influence of the surrounding atmosphere on the alcoholic strength of liquids in bladders (an influence that has been too much overlooked).—On the reduction of sulphates by living beings, by MM. Etard and Olivier. The authors proved experimentally the reduction of sulphates, by Beggiatoa, and found at least three other algæ capable of the same action.—On mono-chlorised allylic alcohol and $CH_2=CCl—CH_2$ (OH) and its derivatives, by M. Henry.—Chemical studies on white beet of Silesia (continued), by M. Leplay.—On the reduction of nitrates in arable land (continued), by MM. Deheraine and Maquenne. *Bacillus amylobacter* is probably the reducing agent.—Direct fermentation of starch; mechanism of this metamorphosis, by M. Mercano. Diastase is a product of the vital activity of the microbe of maize, which produces it incessantly as it traverses the envelopes of the starch grains, thus favouring its action on the stratified granulose. The microbe is that which causes the fermentation of sugar-cane juice.—On the *rôle* of earthworms in propagation of charbon, and on the attenuation of the virus, by M. Feltz. His experiments confirm the views of M. Pasteur as against those of M. Koch.—On the disinfectant and antiseptic action of copper, by M. Burcq. He suggests treatment of infectious diseases with salts of copper, injection of the wood of huts with copper sulphate, also applications of copper to infected furniture, clothing, &c.—Analysis of the reflex of C. Loven, by M. Laffont.—On the venomous apparatus and the poison of the scorpion, M. Joyeux-Laffuie. The poison should be placed among poisons of the nervous system (Bert) and not among blood-poisons (Jousset de Bellesme).—Researches on the genital organ of oysters, by M. Hoek.

VIENNA

Imperial Academy of Sciences, October 5.—E. v. Bruecke, vice-president, in the chair.—The following papers were read:—L. Ditscheiner, on Guebhard's rings.—L. Pebal, note on the mechanical separation of minerals.—H. Schwarz, on new bodies obtained from coal-tar, isomerides of pyrocressol.—F. Schroeckenstein, geological leisure hours; a contribution to the petrography of crystalline rocks.

CONTENTS

PAGE
Recent Chemical Syntheses 49
The Butterflies of India. By H. J. Elwes 50
Our Book Shelf:—
  Buckley's "Winners in Life's Race" 51
Letters to the Editor:—
  "Weather Forecasts."—The Bishop of Carlisle 51
  The Comet.—J. P. McEwen, R.N., Assistant Harbour Master; T. W. Backhouse; Geo. M. Seabroke; Henry Cecil . . . 52
  Magnetic Arrangement of Clouds.—Rev. W. Clement Ley . . . 53
  "A Curious Halo."—Rev. W. Clement Ley; Rev. Gerard Hopkins 53
  Priestley and Lavoisier.—C. Tomlinson, F.R.S. 53
  Wire Guns.—W. H. C. B. 53
  Palæolithic River Gravels.—Wm. White; T. Karr Callard . . 53
  Aurora.—Clement L. Wragge 54
  A Dredging Implement.—W. A. Herdman 54
  Forged Irish Antiquities.—W. J. Knowles 54
The New Natural History Museum 54
The Comet 56
Recent Dynamo-Electric Machines (*With Illustrations*) . . . 58
The Projection Praxinoscope (*With Illustration*) 60
Notes 61
Geographical Notes 63
The Aims and Method of Geological Inquiry, II. By Prof. James Geikie, LL.D., F.R.S. L. and E. 64
A Method for Observing Artificial Transits 67
Electric Lighting, the Transmission of Force by Electricity. By Dr. C. W. Siemens, F.R.S. 67
University and Educational Intelligence 71
Scientific Serials 71
Societies and Academies 71

THURSDAY, NOVEMBER 23, 1882

THE CHALLENGER REPORTS

Reports on the Scientific Results of the Voyage of H.M.S. "Challenger" during the years 1873-1876, under the Command of Capt. Sir George Nares, R.N., F.R.S., and Capt. F. T. Thomson, R.N. Prepared under the Superintendence of Sir C. Wyville Thomson, F.R.S., and John Murray. Zoology—Vols. II., III., and IV. (Published by Order of Her Majesty's Government, 1881-1882.)

SINCE our last notice of these Reports, three more volumes of the zoological series have made their appearance. In vol. ii. published in 1881, and prepared under the superintendence of the late Sir C. Wyville Thomson, the first Report is by Prof. Moseley: On Certain Hydroid, Alcyonarian and Madreporian Corals procured during the Voyage. The great interest and importance of Mr. Moseley's investigations into the structure of the Hydrocorallinæ, and on the Helioporidæ and their allies, justified a previous publication, chiefly in the *Philosophical Transactions*, of the chief results of the author's work. The third part, describing the Deep Sea Madreporaria appears now for the first time. It ought to be noted that the memoirs forming the first two parts have been recast, and contain both additions and alterations. Mr. Moseley's history of *Millepora nodosa* will be acknowledged by all capable of judging, as a most solid contribution to our knowledge of the Hydrocorallinæ. So long ago as 1859, Agassiz announced that the structure of the polyps of Millepora showed that they belonged not to the corals, but to the Hydroids; but although this view was confirmed by others, especially by Pourtales, who once got an imperfect view of the expanded dactylozooids, still it remained for Prof. Moseley to settle this question of affinity beyond a doubt, which he has done by his painstaking dissections. He acknowledges his indebtedness to his colleague, Mr. Murray, who saw the zooids of *Millepora nodosa* in a living and expanded state upon the reefs of Tahiti. This species forms tubercular and irregular masses, often encrusting and overgrowing the dead fronds of *Lophoseris cactus*, which is a principal component of the Tahitian reefs. While fresh, the growing tips of the lobes have a bright gamboge yellow colour, fading off into a yellowish brown; the expanded zooids have the appearance of a close-set pearly white down upon the surface of the mass. Sometimes the encrusting film is very thin. When, as at Bermuda, *M. alcicornis* is found attached to glass bottles thrown into the harbour, this film will not be more than from ⅛th to ⅙th of a millimetre in thickness, and no doubt, now that attention is called to such specimens, they will be studied with the object of telling us more of the life history of these forms.

The Stylasteridæ, now definitely determined to be Hydroids, as was first strongly suggested by G. O. Sars, are described in great detail, and this portion of the report is accompanied by many splendid plates, and a list of all the species of Stylasteridæ at present known is given. Mosely places the group as a separate family, along side of the Milleporidæ, in the sub-order Hydrocorallinæ.

The second part of the report is on Helioporidæ and their allies, in which *Heliopora coerulea* is described from living specimens, and a detailed account of its structure and mode of growth is given. We have also an extremely valuable description of a species of Sarcophyton, almost certainly *S. lobatum*, from the Admiralty Islands, and the conclusion now so well known is come to that Heliopora is without doubt an Alcyonarian.

The third part comes as a quite fresh work, for the preliminary catalogue of the deep-sea Madrepores, was necessarily most imperfect. But here we have extended descriptions of the entire series of species dredged during the voyage, with sixteen plates and also numerous woodcuts intercalated throughout the text. No less than thirty-three species are described for the first time.

These deep-sea Madrepores would appear to be very widely distributed, some, as for example, *Bathyactis symmetrica*, having a world-wide range. At present the only genera which seem restricted in range are Stephanophyltia and Sphenotrochus, which have as yet only been obtained from the seas of the Malay Archipelago, and in comparatively shallower water, and the genus Leptopenus, which has been dredged throughout all the great oceans, but only south of the equator. The wide range of the species in depth has now become a well-known fact, though none the less interesting for that, the world distributed species above-mentioned ranging in depth from 70 to 2900 fathoms. The occurrence of the genera as fossils in Secondary and Tertiary deposits is also not without interest, but the deep-sea forms are not to be regarded as of greater geological antiquity than those found in shallow-water.

The report on the birds collected during the voyage is by Dr. P. L. Sclater. The collection embraced about 900 specimens in skins, besides which there was a considerable series of sea-birds in salt and spirits, and a collection of eggs. The collection was formed under the superintendence of Mr. John Murray, who placed at Dr. Sclater's disposal his ornithological note-book, which contained the history of every individual specimen. It will be remembered that the main object of the expedition was the exploration of the depths of the ocean, and that the collecting of land birds formed no part of the original plan, so that the comparative smallness of the collection is not surprising. The author of the report expresses his indebtedness to his friends, the late Marquis of Tweeddale, Dr. Otto Finsch, Prof. Salvadori, Mr. Howard Saunders, Mr. W. A. Forbes, and Mr. Osbert Salvin, for the assistance they gave him in preparing this report, which is accompanied by thirty coloured plates. Many of the notes appended to the description of the penguins are taken from Mr. Moseley's published accounts of the voyage, and are doubtless already well known to our readers.

Vol. iii., published towards the close of 1881, opens with a most elaborated and magnificently illustrated report by Prof. Alexander Agassiz, on the Echinoidea. The importance of this report has already been called attention to in a special notice (*vide* NATURE, vol. xxv. p. 41).

The second and concluding report in the volume is on the Pycnogonida, by Dr. P. P. C. Hoeck. The collection of these forms was very rich in species. Of the 120 specimens dredged during the voyage, there were no less than 36 species, and of these 33 are described as new to science. Five other species found during the cruise of the *Knight Errant* are also included in the report. These species are referred to 9 genera, of which three are described as new. Those genera which range over the widest area, are also those which range most in depth—while there are some species peculiar to deep-sea areas. No truly generic types seem to be thus characterised. Dr. Hoeck considers that the Pycnogonidæ form a distinct and very natural group or class of Arthropods. Their common progenitor must have been a form with three jointed mandibles—multi-jointed palpi and ovigerous legs, with numerous rows of denticulate spines on the last joints. In the most primitive condition the eye of the Pycnogonid consists of a rounded transparent part of the integument, the inner surface of which is furnished with some small ganglia and nerve-fibres issuing from the integumentary nerve-bundle. The highly-developed eye of the shallow-water species show ganglionic cells, distinct retinal rods, and a lens consisting of a thickened part of the chitinous skin of the animal. Those eyes which have lost their pigment and their retinal rods are rudimentary. Dr. Hoeck, treating of the affinities of this class writes: "about the relation in which the Pycnogonida stand to either the Crustacea or the Arachnida, we know as much or as little as we do about the relation in which these two classes of Arthropoda stand to each other."

Vol. iv. opens with an important contribution to anatomical science in the Report on the Anatomy of the Petrels (Tubinares) collected during the voyage. It is from the pen of Mr. W. A. Forbes, Fellow of St. John's, Cambridge.

The group of Petrels is one that up to the present date can scarcely be said to have been anatomically investigated. It is difficult at all times to procure specimens in the flesh—and some of the species are so large as to render their preservation a matter of considerable trouble. At the suggestion of the late A. H. Garrod, the naturalist staff of the *Challenger* made a fine collection of these oceanic birds in spirits, which contained 74 specimens belonging to 31 species and 22 genera. Prof. Garrod had scarcely commenced to work at this series before he was struck by the lingering illness which ended in his lamented and premature death, and his friend, Mr. W. A. Forbes, undertook to draw up the report which here appears. This report is of a very thorough character. Commencing with an account of the previous literature on the anatomy and classification of the group; we then have a complete sketch of the comparative anatomy of the group--the external characters, pterylosis and visceral anatomy are first described—these are succeeded by an account of the myology—to which follows a description of the tracheal structures and of certain other points in the anatomy of the soft parts, while an account of the osteology concludes the report. Some of the modifications, described by the author, "are of great physiological and morphological interest, whilst the numerous differences in points of detail displayed in the different sections and genera of the Petrels, lead one to expect that the future study of systematic ornithology will be not a little elucidated by the labours of the anatomist wherever he has material, as in the present case, at his command, sufficient for an adequate study of a natural group on the basis of structural differences more important than those that can be discerned from the superficial inspection of an ordinary skin." This report is illustrated by very numerous woodcuts and seven plates of anatomical details. In treating of the affinities of the group, Mr. Forbes declares it to be a difficult task to assign to it any satisfactory position in any arrangement of the class of birds.

The second report in the volume is on the Deep-sea Medusæ, by Prof. Ernst Haeckel. They form one of the smallest and least important groups of the rich and remarkable deep-sea fauna discovered during the voyage of the *Challenger*. The number of species described does not exceed eighteen, of which half are *Craspedotæ* and half *Acraspedæ*. These new species were briefly diagnosed in the "System der Medusen, 1879," but they are here described at great length and with a most splendid series of illustrations. The descriptive portion of the memoir is prefaced by a very elaborate sketch of the comparative morphology of the medusæ, which is illustrated by many woodcuts.

It would seem by no means certain that all the eighteen species of deep-sea medusæ here described are constant inhabitants of the deep sea. The method of capture by the tow-net by which such delicate and fragile organisms are brought from great depths is still imperfect, and it seems probable that the greater number of medusæ brought up apparently from the greater depths really swim in shallower water, and are only taken in during the "hauling-in" of the net. But Prof. Haeckel counts that those medusæ which have either adapted themselves by special modifications of organisation to a deep-sea habit of life, or which give evidence by their primitive structure of a remote phylogenetic origin, may with great probability be regarded as permanent and characteristic inhabitants of the depths of the sea; and as such he regards fourteen out of the eighteen described. With regard to the magnificent illustrations the author states: "It is of course impossible, from the imperfect state of preservation of the spirit specimens, to expect that they should be absolutely true to nature. I rather considered it my duty here, as in those figures in my 'System der Medusen,' which were drawn from spirit specimens, to take advantage of my knowledge of the forms of the living Medusæ to reconstruct the most probable approximate image of the living forms, I was greatly assisted in my efforts in this direction by the skilful hand of my lithographer, Adolf Giltsch." It seems hardly necessary to make any scientific criticisms on this straightforward statement.

The third and concluding memoir is by Hjalmar Théel, and contains the first part of his report on the Holothuroidea. It is altogether devoted to the holothuroids of the new order Elasipoda, which name has been with advantage substituted for that of Elasmopoda used in the Preliminary Report. Seven years have scarcely elapsed since the discovery in the Kara Sea of the form for which this family was established, and now over fifty species are known. These species of Elasipods are true deep water forms, and they may with all the more reason be said to characterise the abyssal fauna, as no single representative as far as is at present known has been found to exist at a depth less than 58 fathoms. Only one form, *Elpidia glacialis*, has been dredged at this inconsiderable depth, and even this was dredged in the Arctic Ocean, where true abyssal forms are to be met with at comparatively shallow depths. This species too can exist at immense depths, one from Station 160 having been dredged at a depth of 2600 fathoms, the greatest depth at which any Holothuroid has to this been dredged being 2900 fathoms. Among the more remarkable and distinguishing characteristics of this order Mr. Théel mentions the agreement in several important details—both in their internal anatomy and outer forms—of the adult and larval forms, an agreement more close than occurs in any previously known Holothuroid. He does not agree with Danielssen and Korren in placing the Elasipods low in the series of the Holothuroids; nay in some respects he regards them as having attained to a higher development than all the other Echinoderms, because, among other facts, their bodies are distinctly bilaterally symmetrical, with the dorsal and ventral surfaces distinct and often with a cephalic region well marked. Only the ventral ambulacræ are subservient to locomotion; these latter show a tendency to appear both definite as to place and number. The dorsal appendages are so modified as to perform functions different from the ventral ones This memoir contains forty-six plates, which give full details of the forms and structure of all the new species.

LIGHT

Light: A Course of Experimental Optics chiefly with the Lantern. By Lewis Wright. (London: Macmillan and Co., 1882.)

THIS is a book by a worker whose work in his own line is of a very high order, and whose experience will be of correspondingly high value to others who are working at the same subject. In all those departments of experimental optics in which the lantern is employed for the demonstration of actual experiments to an audience, Mr. Wright is a master hand: and his book, as might be expected, is consequently a valuable repertory of useful information and of suggestive hints. Of books on Light there are already enough and to spare. Of standard treatises and text-books in the department of Geometrical Optics the supply is more than could be desired. In Physical Optics there is still room for a good elementary mathematical text-book. In Physiological Optics also there is, save for the great treatise of Helmholtz, a void. But the work before us stands apart from all these, both in aim and in character. Indeed so well does it carry out the ideal of a work "on experimental optics chiefly with the lantern," that there was really no need to prefix to the title the word "Light." True it is that Mr. Wright does not confine himself to the mere working of lanterns and their accessories. He deals in a simple and practical way with the laws of reflexion and refraction, and with ordinary optical instruments: but he always adds something of practical interest to the teacher of optics. To illustrate the laws of reflexion and refraction he describes a simplified form of the apparatus so well known in Prof. Tyndall's lectures on Light; and the mechanical illustrations of wave-motion, &c., are also new in several respects. The chapter on Spectrum Analysis is brief and sketchy, but includes almost all the experiments which can be projected on to the screen with the lantern. Amongst these we notice very careful instructions for exhibiting the spectrum of Newton's rings and of other interference phenomena.

Nearly one-half of the book is devoted, and well devoted, to experimental work on Double-Refraction and Polarisation. In this section there are a number of beautiful experiments described which we do not remember having seen before in any treatise in the English language. Amongst these are some with compound mica plates built up of a series of films of definite thickness and united by Canada balsam. A series of twenty-four superposed mica films, each producing a retardation of one-eighth of a wave-length and each one-sixteenth of an inch shorter than the one beneath it, is in this way made to reproduce exactly the first three orders of colours of Newton's rings, but divided into the precise tints over narrow strips. A detailed account is also given of the combinations devised by Norremberg and Reusch for reproducing the phenomena of uniaxial crystals and of quartz by the superposition of thin films of mica crossed in various ways. Plates illustrative of these combinations contribute much to the value of the descriptions and explanations of the text. Mr. Wright also gives some account of his own researches upon the spiral figures produced by the introduction of quarter-undulation plates into the polariscope in which crystal sections are being examined by convergent light. There is a penultimate chapter on the polarisation of the sky and of minute particles, followed by a final chapter—wholly out of place in such a work—in which, so far as it is intelligible, there appears to be an attempt made to connect the undulatory theory of light with the trinitarian theory of theology. With the exception of this last, and with a few occasiona inelegancies of style, there is little fault to find with the book. The mathematical student of optics will without doubt grumble when he takes up the work, because the mathematical aspect of the subject is conspicuous by its absence. The author does not profess to be a mathematician: or he would hardly have pronounced in favour of Brewster's views on the theoretical polarising angle, as he does on p. 223. This, however, is a minor matter in a book whose great aim is to assist manipulation. The numerous illustrations, a large proportion of which are original, add greatly to its value. The coloured plates of polariscopic phenomena are, it should be added, of singular excellence.

S. P. T.

OUR BOOK SHELF

Practical Chemistry, Analytical Tables, &c. By J. Campbell Brown, D.Sc. (London: Churchill, 1882.)

NOTHING perhaps is more remarkable than the great increase during the past few years in the number of books on practical chemistry and analysis. This has no doubt to some extent been caused by the prominence given generally to the teaching of chemistry in the laboratory.

The books to which we refer consist with few exceptions of tabular statements of reactions of acids and bases and methods of detection of the same in simple salts or mixtures. They all appear to be on the same "type" and with the same intention of putting students through a course of drudgery in qualitative analysis according to a fixed "table." The book before us is no worse than others of its class, but attempts rather too much by giving condensed tables for alkaloids and gases, which are, however, in themselves very good ones. It is to be feared that these practical books tend to make students mere analytical machines in a small way, without giving them much real practical notion of chemistry. It is questionable whether a student who has worked through the modern tabular system of practical chemistry would be able, for instance, to state the reason for the employment of bricks in preference to chalk for the back of an ordinary fireplace or some equally simple practical question.

Elementary Chemical Arithmetic. By Sidney Lupton. (London: Macmillan and Co., 1882.)

THIS little book with its modest preface will be recognised by all teachers of chemistry, especially in large laboratory classes, and also by students as a really useful adjunct.

Unfortunately in large public laboratories a considerable proportion of the students have been very much neglected in the matter of their elementary mathematical education, or it has been of such a nature that they are not able to apply it to the solution of ordinary chemical problems, thus entailing, in many cases, a large amount of extra work and loss of time on the part of the teacher in giving instruction in elementary arithmetic. This book fits into its place exactly. It is divided into two main portions: an introduction, consisting of short but very understandable explanations of arithmetical processes in common demand in chemistry and physical chemistry of a practical and elementary nature, the second portion being problems divided under the headings of the different elements. Regarding these it may perhaps be said that they do not err on the side of being too chemical, and in one or two cases more attention has been given to the question as a question than to its absolute chemical correctness, but these are mere details that in no way detract from the utility of the book for its purpose.

What is required of the mass of chemical students is that they should be able to apply methods of reasoning founded on experimental facts in the science to the solution of concrete and abstract problems; and working through this book will certainly conduce to bring about an improvement in that direction.

The Watch and Clockmaker's Handbook. By F. J. Britten. (London: Kent and Co., 1881.)

THIS little book has been written, we are informed, chiefly for the instruction of country watchmakers. It cannot fail to be agreeable to them: it contains a great deal of useful practical information, and some is given of a higher quality, such as workmen are, to their credit, eager for now-a-days. To another and wider circle there is also much of a character to be interesting. The book is a proper supplement to the more popular horological treatises. There are good descriptions and pleasing diagrams of the various watch escapements; there is a chapter upon the art of springing; the mechanism of chronographs, repeating watches, and calendars is shown, but almost too briefly. Lastly, we find pictures and a short reference to the various tools which watchmakers employ, and some serviceable memoranda are added. Upon the whole the author has and deserves our praise.

H. DENT GARDNER

Heroes of Science. Botanists, Zoologists, and Geologists. By Prof. P. Martin Duncan, F.R.S., F.L.S. (London: The Society for Promoting Christian Knowledge, 1882.)

THIS little volume contains brief sketches of the lives of a few botanists, zoologists, and geologists, for the most part acknowledged compilations from well-known sources. No doubt the work will serve the purpose for which it is evidently intended—that of interesting young people in science.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Physics of the Earth's Crust

ON March 23 last Prof. Green sent to NATURE some remarks upon Mr. Hill's review of my "Physics of the Earth's Crust." More lately the third edition of his "Physical Geology" has appeared, in which he has repeated the substance of a part of what he then wrote. On account of the great weight which his authority will carry, I think I should offer some reply.

He truly says at p. 674, that I claim to have proved that the contraction of the earth through cooling cannot have caused the amount of squeezing and elevation which has taken place, and that the hypothesis is therefore insufficient to explain the facts which it professes to account for; but he then adds: "What Mr. Fisher has really done is this. His calculations go far to prove that, provided the earth cooled in the way assumed by Sir Wm. Thomson, contraction would not suffice to produce anything like the compression and elevation that has actually occurred. But this is quite another thing from disproving the contraction hypothesis. Mr. Fisher's investigations tend rather to establish a strong probability that the earth did not cool in the way supposed by Sir Wm. Thomson,"—that is, that it became solid throughout in a comparatively short space of time. But of course my calculations do not establish any probability against this way of cooling, unless we begin by assuming that contraction through cooling has been the cause of the elevations. And that seems to be begging the question. What they do prove is that the contraction hypothesis will not account for the elevations if the earth has cooled as a solid.

But there may have been another way of cooling which, on geological grounds, I believe to have beeen the true one. The earth may not have become solid throughout in a short space of time, and may not be solid even now. In that case the crust, whose corrugations we have to account for, must have floated on a denser liquid substratum. Under these circumstances every elevation above the mean level must have had a corresponding protuberance answering to it below. This is necessary, as was long ago pointed out by Sir G. B. Airy. I have, then, proved that, this being so, if the crust beneath the ocean is of the same density as beneath the continents, on what I conceive to be reasonable assumptions regarding the thickness and density of the crust and the density of the substratum, a shortening of the earth's radius by less than 700 miles would not have sufficed to produce the existing inequalities. I can imagine no theory of the constitution of the interior that would admit of so large an amount of contraction taking place, after the whole had become sufficiently cool for a crust to have begun to be formed, as to cause such an amount of shortening as this.

If, however, we suppose that the crust beneath the oceans is denser than that which forms the continents (and I have given several reasons for believing such to be the case), then a much smaller amount of radial shortening would suffice. I have estimated it at about forty two miles. Still, anything near this shortening is far beyond what any reasonable amount of contraction from cooling could produce. For if there be a liquid substratum this must be of nearly equable temperature throughout, and that cannot be much above the temperature of solidification; so that it does not appear how a much greater contraction can be got out of the gradual solidification, and incorporation of the upper parts of the liquid layer with the crust, than could be obtained on the former supposition of a cooling solid globe; and I have shown that, in that case, the radial shortening would be less than two miles.

Thus, then, I claim to have disproved the contraction-hypothesis under the two alternative hypotheses (1) of a solid globe, and (2) of a liquid substratum.

Capt. Dutton, of the United States Geological Survey, has said of this part of my work, "First and foremost he has rendered most effectual service in utterly destroying the hypothesis, which attributes the deformations of the strata and earth's crust to interior contraction by secular cooling. No person, it seems to me, can sufficiently master the cardinal points of his analysis, without being convinced that this hypothesis is nothing but a delusion and a snare, and that the quicker it is thrown aside and abandoned the better it will be for geological science" (*American Journal of Science*, vol. xxiii. p. 287).

I take this opportunity of pointing out a mistake in my book. At page 156 the number 1127 ought to be 1734; and consequently the number 0·996 ought to be 0·965. The argument will still hold.

O. FISHER

Harlton, Cambridge, November 9

P.S.—Since forwarding the above I have observed a note at p. 912 of Dr. Geikie's "Text Book of Geology," in which he says that I have "endeavoured" to show that the secular contraction of a solid globe through mere cooling will not account for the phenomena. The word "endeavoured," does not express the attitude of my mind upon the question. Forty-two years ago the contraction theory occurred to myself independently. I remember that in my youthful joy at what I thought thought a discovery, I forthwith vaulted over a gate! In 1868 I read my paper on "The Elevation of Mountains by lateral Pressure," fully believing that I was elucidating the cause which had produced them in the contraction through secular cooling. In 1873 I began my paper on "The Inequalities of the earth's Surface viewed in connection with the Secular Cooling," while still under the same impression. I first of all estimated the actual elevations, and, this done, I calculated the amount of those which would be formed upon Sir William Thomson's view of the mode of solidification. To my excessive surprise, the result showed the utter inadequacy of the contraction hypothesis. I thought I must have made some error in the calculations, but could find none. I still, however, adhered to the original idea of contraction, and suggested, towards the end of that paper, a fluid condition of the interior at some *former* period, thinking that sufficient contraction might be perhaps obtained by that means; for I had not yet dared to question Sir Wm. Thomson's dictum of the *present* complete solidity of the earth. It was not until about a year ago, when I wrote the chapter in my book about the "Amount of Compression," that I perceived that even the condition of a liquid substratum would not give the necessary degree of contraction to produce the compression. I have thus been driven from the contraction hypothesis step by step, and have by no means been endeavouring to support a preconceived opinion against it.—O. F.

Shadows after Sunset

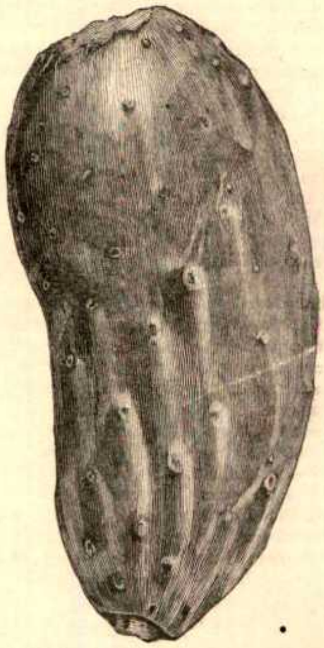
HAPPENING by chance to look into "Loomis's Meteorology," after reading M. Dechevren's account of the blue, white, and red bands visible before sunrise and after sunset at Zikawei, I noticed under the above heading the following account of shadow-bands, which not only appear to be very similar to those observed by Dechevrens, but are explained in identically the same way ("Loomis's Meteorology," p. 107): "A similar phenomenon [to the water-bands described in the preceding paragraph] is frequently noticed about fifteen minutes after sunset, when the shadows of clouds near the horizon are projected upon the western sky in the form of radiant beams diverging from the sun. These beams are parallel lines of indefinite length, but from the effect of perspective they seem to diverge from the sun, and if they could be traced entirely across the sky, they would for the same reason converge to a point directly opposite to the sun. Such cases are sometimes, though not very frequently noticed. Similar shadows are sometimes seen in the morning before sunrise, and form a conspicuous feature of the morning twilight. This effect is sometimes noticed in nearly every part of the world. It must have attracted the attention of the ancient Greeks, and is thought to explain that poetic expression "the rosy-fingered dawn."

M. Dechevrens appears to think the phenomenon does not occur in Europe or temperate latitudes generally, but from what Loomis says, one would infer that he may be mistaken in this, and that to a modified extent it may be visible in Europe and America. Perhaps some of your readers who are in the habit of observing the face of the sky will be able to verify this supposition. For my own part I have not remarked it in England, but have occasionally witnessed it in Bengal during the rains, very markedly. The explanation offered by M. Dechevrens seems the only reasonable one under the circumstances, but he hardly seems to lay sufficient stress upon the fact that when the sun is below the horizon his rays can only illuminate a shallow stratum of partially condensed vapour in the upper atmosphere. Any obstruction of his rays will consequently shut off the *whole* of the reflected light from this stratum, and cause the blue sky to appear through the shadow, all the more cerulean by contact with the whitish or rosy colour of the adjacent portions which still bask in the solar rays.

E. DOUGLAS ARCHIBALD

An Abnormal Fruit of Opuntia Ficus-Indica

THE accompanying figure represents a fruit of *Opuntia Ficus-Indica*, which is wholly inclosed in one of the well-known flat branches of this plant; normally the fruits appear as exserted obovate bodies on the margin, or on either side, of the branches. The figure is exactly half natural size; the fruit is therefore full grown. There is no interruption in the ascending curves of spinous tubercles, only they are somewhat smaller on the fruit, which has also a less wrinkled skin than the remainder of the branch. It is of rather uncommon occurrence, nobody having seen here anything alike in the extensive *tunales* or Indian fig-plantations of our neighbourhood; nor have I been able to find any mention of such a case in the books at my disposal. It is evidently an instance of non-development of peduncle, a special case of suppression of axile organs (Masters, "Teratology," p. 393). But I think it throws also some light on the nature of what generally is taken to be the pericarp of the Opuntia fruit, which, after all, seems to be a slightly modified branch, bearing the ovary of the flower in a cavity on its upper end. A similar view is held forth by Dr. Noll in a paper published in the *Annual Report* of the Senkenbergische Gesellschaft (Frankfurt, 1872, pp. 118-121, with two plates), where he describes and figures two abnormal fruits of *Opuntia coccinellifera* from the Canary Islands, with branches growing from the exterior part of the fruits. Their apparent pericarp is therefore an axile organ of a certain order, say of the order n, whilst the additional branch is of the next order, $n+1$. The case which forms the object of the present note is quite the reverse of those mentioned by Dr. Noll, as the branch of order n, or the exterior part of the normal fruit, is not developed independently, being represented by its parent-branch of order, $n-1$.



Abnormal Fruit of *Opuntia Ficus-Indica* from Carácas.

If this view be correct, there can no longer be any reason for speaking of an *exserted ovary* in Opuntia (Hooker and Bentham, "Genera plantarum," I., 851), as this organ is wholly sunk in the interior of a branch, just as it happens in other Cacteæ with an *ovarium immersum*.

A. ERNST

Carácas, October 4

The Comet

THANKS to the entire absence of twilight, and to a beautifully clear sky, I obtained a splendid view of the comet on November 14, 15h. 45m. The tail had a length of 30°, and was divided into two portions at the extreme end, the northern extremity curving very sharply upwards, and separated from the southern branch by a semi-circular space. The general form of the tail being very similar to the Greek character γ. The southern side still remained brighter than the northern. The nucleus was much more elongated than when I observed it on November 8. The two concentrations of light which were very noticeable on that date, were not now so conspicuous, being smaller and much closer together, so much so, that had the definition been otherwise than perfect the division between them could not have been seen. As showing the necessity of observing this interesting object in the absence of twilight I may mention that by 17h. 45m. G.M.T., the apparent length of the tail was reduced to 20°.

B. J. HOPKINS

79, Marlborough Road, Dalston, E., November 20

Soda Flames in Coal Fires

IF a coal-fire be looked into with some attention after a fresh supply of coals has nearly ceased to give out its gases, there will be seen here and there in the hottest parts, and coming out of them through crannies and round dark corners, a pale translucent yellow flame, which one soon gets to recognise easily. What does it consist of? If looked at through a prism, without any slit screen, this flame is at once seen to be monochromatic. Neither its shape nor brilliancy (in which it is deficient) are at all altered or impaired; and it is especially interesting on this account, as there is something uncanny in the appearance of this pale flame defying the power of the prism, as it flickers and plays about the brilliant spectrum representing the red-hot coals.

Coals vary much in their possession of the source of this flame. In some it seems scarcely present at all, while in others it is abundant, being recognisable even in the large surface-flames. The coal in which I have seen it best, is a close hard coal, with a slaty cleavage and rectangular fracture, known, I am told, as "Anchor Brights" (?) The yellow flame appears frequently even in the largest surface ones, when the gaseous products first disengaged have disappeared. Some of them seem, then, to consist entirely of this, giving little or no continuous spectrum. But it is in the body of the fire that it is most fascinating, imparting a reality to the otherwise confused forms, which is more than pretty. I am strongly reminded by this appearance, when, for instance, a black mass is seen to stand out with a clear outline against the pale yellow background of light, of the picture which was mentally present in the days before the solar eclipse of 1868—the first upon which the prism was brought to bear. I have fortunately found a copy of some "Instructions" issued on the occasion of distributing the "hand-spectroscopes" provided by the Royal Society for the study of that eclipse; in which this prognostication is indicated with quite as much precision as the known facts at that time warranted. That it was not fully understood was the only reason why the moon was *not* seen, as it might have been seen, on that memorable occasion, sharply outlined upon the coronal light, just as I now see the coal. This was long before the time when the same arrangement on a larger scale—a prism in front of the object-glass of a telescope—obtained such success in other hands. However that may be, the coal-fire experiment is a very pretty one, and might be made very instructive too as a drawing-room illustration—the ordinary prismatic pendants of a chandelier being quite equal to the occasion, if a direct-vision combination is not immediately available.

J. HERSCHEL

30, Sackville Street, W.

P.S.—As the monochromatic light—of sodium, of course—is plentiful in the large flames, it will be well seen as a *line*, straight or curved, if the light of the fire on a cylindrical or curved metallic or other reflecting surface be looked at, especially if dark coloured; such as an ebonite ruler, for instance. Of course a direct-vision pocket spectroscope is better than the pendant of a chandelier; but the lenses must be taken off, as well as the slit-screen.

Complementary Colours—A Mock Sunset

INSTANCES of two phenomena recently noticed in NATURE have chanced to come under my observation, and in each case impressed me much with their beauty and distinctness; the first, an effect of contrast of colour on the surface of clear water. Standing looking up stream on a bridge over the Ary, where it flows through meadows close to Inverary Castle, and admiring the transparent brown hue so often seen in the peat-stained waters of Scotch streams, my attention was attracted by a series of wavelets forming a ridge, somewhat spiral in appearance, across the stream, along the top of a low weir over which the water falls. Every single wave presented on its further surface (that seen foreshortened by the spectator) a nearly level space of pure full-toned amethyst colour, while its advancing front showed with crystalline transparency the deep "cairn-gorm" or burnt sienna tint proper to the water. The regular alternation of these patches of rich and brilliantly-contrasted colours, together with their permanency and apparent independence of anything peculiar in the state of the atmosphere, produced a striking and very beautiful effect.

The phenomenon of a mock sunset in the east I witnessed in great perfection on the Lake of Lucerne, when the whole eastern sky was traversed by broad rose-coloured bands converging from the north, south, and zenith towards a point opposite the sun.

T. H.

A Lunar Halo

LAST evening, about 7.15 p.m., a lunar halo of a peculiar character was seen here. It was at some distance from the moon, and instead of being, as usual, concentric with this body, was of an oval, or, more strictly speaking, a horse-shoe shape, the lower part of the halo not being complete. The moon, too, was not in the approximate centre of the horse-shoe. Supposing its distance from the vertex to be represented by the quantity 1, 2½ would represent its distance to the lower part of the halo. Some heavy mist-clouds lay under the moon, which thinned out and became more transparent upwards, and refraction from the dense parts of these may have been the cause of the curious distortion of the circle in this case.

J. RAND CAPRON

Guildown, November 21

A Correction

PERMIT me to correct an error which appears in your report of "The Additions to the Zoological Society's Gardens" (NATURE, vol. xxvi. p. 232). Your reporter states that one of the parrots presented by me is a "New Zealand parakeet (*Cyanorhamphus novæ-zealandiæ*"). The bird I sent is *Cyanosaisseti*, Verr., from hence (New Caledonia), and, according to Dr. Sclater's published catalogue, has never been in the Gardens. It differs—as I have already pointed out—from *C. novæ-zealandiæ* in size, extent of markings, but especially in the shape of the tail feathers (*Cf. Ibis*, vol. 1879, pp. 109 110). It is one of a small group of parrakeets that is found in New Zealand, Chatham Island, Norfolk Island, and here, closely resembling each other, but at once separable when seen together. Neither this, nor *Nymphicus uvæensis*, Layard, which is a new species just described by me, has ever been seen in Europe before, that I can learn.

E. L. LAYARD

British Consulate, Noumea, September 7

[The Secretary of the Zoological Society informs us that Mr. Layard is quite right in his remark, but that the bird has been long since correctly named, and will be shortly figured in the Zoological Society's *Proceedings* under its proper name.—ED.]

Thomson's Mouse-Mill Dynamo

ALLOW me to make a slight but important correction on your description, in last week's NATURE, of Sir William Thomson's *mouse-mill* dynamo. In your description it is said that "at one end of the hollow drum these copper bars [the mouse-mill bars] are united to each other in pairs, each to the one opposite it." This is not so. At one end of the hollow drum the ends of the copper bars are *all united together*, "metallically connected by soldering or otherwise." The effect is electrically the same as that of the arrangement described in your article; but, in the construction of the machine, the uniting of all the bars together at one end, instead of joining them in pairs, is so much more simple and easy that the correction seems of importance.

J. T. BOTTOMLEY

The University, Glasgow, November 18

"Weather Forecasts"

I HAVE recently designed and patented "An improved floating vessel for automatically compressing air by the action of the waves of the sea, and also for the generation of electricity by the agency of this compressed air." This vessel is capable of being moored in 1000 fathoms, and can be connected with the shore by means of an insulated electric cable. Such a vessel moored in the mid-Atlantic in the usual track of the cyclones which approach these islands from the west, would be of immense advantage to the Meteorological Office in determining the velocity of advance and direction taken by these cyclonic centres. I purpose exhibiting a model and drawings of the vessel at the Winter Electric Exhibition, to be held at the Westminster Aquarium next month. CHARLES W. HARDING

King's Lynn, November 14

Age of Dogs

I AM acquainted with a black retriever dog aged thirty-one years, and should like to know whether this age is often attained by dogs. R. CORDINER

Oxford, November 15

Waterspouts on Land

WHEN on a fishing expedition this year, in the mountainous district of Minnigaff, in this country, my attention was drawn to the effects of two waterspouts, which had taken place, one in July last, and the other some six months previously. The effects of both are to be seen in the faces of two mountains a mile apart. One is on a hill-farm called Blac Klaggan, about 100 yards above a mountain-stream, where an excavation, by the force of the spout, had been made to the depth of ten or twelve feet, and about twenty yards wide. Stones—boulder-stones from 10 cwt. to 3 tons, were spread out, in the course of the torrent, down to the "burn," which runs below—one boulder, lying in the bed, being quite 3 tons weight. The other waterspout had struck on *White Laggan*, on a steep mountain side, facing the upper part of Loch Dee. It was higher up on the hill, and had cut to the depth of about 15 feet, and was 10 yards wide, scattering the earth and boulders before it, to a distance of 150 yards below, and spreading out the smaller stones and gravel over a flat moor, in varied tracks, more than 100 yards further. I have not heard of anyone who saw either waterspout, and both are supposed to have taken place at night. All the other parts of both mountains are covered with heather and grass, above, on each side, and below, except in the direct course cut by the torrent from each waterspout. No one remembers any previous case of the sort in the district. Perhaps some of your readers can give other instances of this kind, and some information that may prove interesting and useful. JAMES HOSACK

Ellerslie, Kirkcudbright, N.B., November 13

METEOROLOGY OF THE MALAY ARCHIPELAGO[1]

THE two systems of meteorological observations carried on under the direction of the late Dr. Bergsma present us, in these two serial publications, with what must be classed among the most remarkable contributions made in recent years to observational science, and they are all the more valuable on account of the new and exact information they give as to the different climates of the Malay Archipelago, about which so little was previously known.

The first and longest continued series of observations made at the observatory at Batavia take rank among the very best yet made. They embrace hourly observations for the fifteen years ending with 1880, of atmospheric pressure, temperature, humidity, rain, wind, cloud, &c., which have been published *in extenso*. During the first thirteen years the records consisted wholly of eye-observations, but from the beginning of 1879 the observations were made by photographically and other self-recording instruments. In vol. v., in addition to the hourly observations for 1879 and 1880, there is given a discussion of the fifteen years' observations, which from the excellence of its design and execution, represents the meteorology of Batavia with a fulness and completeness at least equal to what has yet been done for any other place on the globe.

Among the more interesting results, those of the rainfall may be pointed to, particularly the tables showing the mean amounts for the different hours of the day. These reveal two daily maxima and two minima. The larger maximum occurs from 2 to 7 p.m., when 32 per cent. of the whole daily fall takes place, and the larger minimum from 6 to 11 a.m., when only 13 per cent. of the daily amount falls. The smaller maximum is from 10 p.m. to 2 a.m., when 17 per cent. falls, and the smaller minimum during the two hours from 8 to 10 p.m., when 7 per cent falls.

The most remarkable, if not the most important of the results arrived at are perhaps those referring to the influence of the moon on the pressure and temperature of the atmosphere and the rainfall, which establish the fact of a distinct lunar atmospheric tide. Assuming the lunar day to commence with the time of the upper transit of the moon, the following are the phases above or below the mean expressed in millimetres :—

	mm.			
1st max.	+0·057	at	lunar hour	1
,, min.	−0·053	at	,,	7
2nd max.	+0·064	at	,,	13
,, min.	−0·060	at	,,	19

The lunar tide has been determined for each of the four quarters, and also at perigee and apogee, and the results show differences of great interest. As regards the rainfall, while the mean amount in 24 hours during the 17 years ending with 1880 was 5·19 mm., at the time of new moon there was a mean excess of 0·94 mm., and at full moon also an excess of 0·19 mm., but on the other hand, at the third octant there was a deficiency of 0·61 mm., and at the fifth octant also a deficiency amounting to 0·55 mm.

The result is that the atmospheric pressure at Batavia has a lunar daily tide quite as distinctly marked as the ordinary diurnal barometer tide, except that its amplitude is much less, the lunar daily tide being as compared with the mean solar daily tide nearly in the proportion of a millimetre to an English inch. The lunar tide has also the important difference in that its phases follow the moon's apparent course much more closely than the diurnal barometric fluctuations follow that of sun. The two maxima occur about the 1st and 13th, and the two minima about the 7th and 19th lunar hours, whereas these four daily phases of the diurnal barometric fluctuation occur with respect to the sun's apparent course from one to six hours later. The influence of the moon's phases on the rainfall is quite decided ; for while the mean daily rainfall is 0·205 inches, it rises at full moon to 0·248 inch, from which time it gradually falls to 0·181 inch at the third octant, rises to 0·212 inch at the fourth octant, then falls to 0·184 inch at the fifth octant, and finally rises gradually to the maximum at the time of new moon. The important conclusion follows that the attractive influence of the moon, and consequently that of the sun, must be taken into account as factors concerned in bringing about oscillations of the barometer. In this connection it is interesting to note that in the higher latitudes in inland situations during winter, or at times and situations where the disturbing influences of temperature and humidity tend towards a minimum, the times of occurrence of the four phases of the daily oscillation of barometer approximate to those of the daily lunar atmospheric tide.

The second series of observations, giving the rainfall for the three years 1879, 1880, and 1881, form an extremely valuable contribution to our knowledge of the climates of the Malay Archipelago. This network of rainfall observation now includes 150 stations scattered over the islands at heights varying from near sea-level up to 6404 feet. The averages of the three years show that the mean annual rainfall over the archipelago varies from about 60 inches in Timor to upwards of 200 inches at some spots among the western slopes of Sumatra. But the determining character of the rainfall, as regards the climates is not the absolute amount that falls annually but rather the manner of its distribution through the months of the year. Over the larger proportion of the islands rain falls copiously every month of the year; but as regards some of the islands, the year is divided into dry and wet seasons as markedly as is seen in the climates of India.

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[1] Observations made at the Magnetical and Meteorological Observatory at Batavia, 1866 to 1880. Regenwaarnemingen in Nederlandsch-Indie, 1879-80-81. Door Dr. P. A. Bergsma, Directeur van het Observatorium te Batavia.

The reason of this difference is readily seen on examining the distribution of atmospheric pressure during the months from Australia to China with the prevailing winds resulting therefrom. During the winter months pressure is high in China and low in the interior of Australia, the mean difference being nearly three-quarters of an inch. Between the two regions the fall is practically uninterrupted, and the Malay Archipelago lying between them is swept by northerly winds. Since these winds have traversed no inconsiderable breadth of ocean, they deposit a copious rainfall particularly on the northern slopes of the higher islands, and consequently the rainfall of these months is large over all the islands without exception, the mean monthly amount in some places exceeding 30 inches. It is to these same winds that the north of Australia owes its rainfall; and it is their strength in any particular year which determines the distance to which the annual rains penetrate southwards into the interior of that continent.

On the other hand, during the summer of the northern hemisphere, atmospheric pressure is high in the interior of Australia, and low in China, the mean difference being aboot half an inch, and between the two regions the fall in the mean pressure is continuous and uninterrupted, and consequently southerly winds prevail over the intervening region. These winds are dry and absolutely rainless over the north of Australia, and over Timor and the other Malay islands, which are separated from Australia but by a comparatively narrow belt of sea. During the three years no rain whatever fell at Timor during July and August, and the fall was small during June, September, and October. As the winds pursue their course to northward, they eagerly lick up moisture from the sea, so that by the time they arrive at Amboyna they have become so saturated with moisture that the monthly rainfall of that place rises at this time of the year to nearly 30 inches. At some distance to the west of Timor rain falls at this season more or less regularly every year, the amount increasing in proportion to the extent of ocean traversed by the south-east winds, which blow towards the islands from the direction of Australia. These marked and vital differences of the climates of the Malay Archipelago, which, as they depend essentially on the geographical distribution of the land and sea of this part of the globe may be regarded as permanent, have played no inconspicuous part in the remarkable distribution of animal and vegetable life which characterises the archipelago.

THE COMET

THE receipt of observations from Australia, made between September 8 and 16, has allowed of the determination of the orbit of the present comet exclusively from positions obtained before the perihelion passage when it made so close an approach to the sun. From a mean of the Melbourne and Windsor N.S.W. observations on September 9, and the Melbourne meridian observations on September 14 and 16, Mr. Hind has deduced the following orbit:—

Perihelion passage, Greenwich M.T., Sept. 17·21897

	°	′	″
Longitude of perihelion	275	50	20
Ascending node	345	53	2
Inclination	38	0	17
Log. perihelion dist.	7·8501274		

Retrograde.

The longitudes are reckoned from the apparent equinox of September 17, and it should be mentioned that the small corrections have been neglected. On comparing the observed places with those calculated from the elements founded upon observations before perihelion, the following differences remain:—

			$\Delta\alpha . \cos\delta\ (c-o)$	$\Delta\delta$
			″	″
Tebbutt	Sept.	8	− 25	− 3
Tebbutt and Melbourne	,,	9	0	0
Melbne. merid.	,,	14	+ 21	+ 7
,,	,,	15	+ 5	+ 5
,,	,,	16	+ [illegible]	0
A. Common ...	,,	17	+ 12	− 4

When however we compare with the meridian observations at Dunecht and Coimbra on September 18, or the day after the comet's close approach to the sun, the computed place is found to differ by several minutes of arc from that observed, and at the time when Mr. Gill noted the comet's ingress upon the sun's disc, calculation places it 2′ 30″ within his limb. These differences appear to point to sensible perturbation about the perihelion passage, but a stricter discussion of observations before and after the time when the comet attained that position in its orbit, will be needed before any reliable judgment on this important question can be formed. It may be noted also that a very small change in the time of perihelion passage has a comparatively large effect upon the geocentric positions about that epoch.

Mr. W. F. Denning communicates the following estimates of the length of the tail of this comet made by him at Ashley-down, Bristol; the dates are astronomical:—

Oct. 1	... 10	Oct. 30	... 22	Nov. 8	... 23
,, 11	... 15	Nov. 5	... 23	,, 9	... 22
,, 25	... 19	,, 6	... 23		

To form an idea of the real extent of the tail, assume it first to be situate in the direction of the radius-vector, as is most frequently the case. At 6 a.m. on November 7, by the orbit last published in NATURE, the distance of the comet's nucleus from the earth (expressed in parts of the earth's mean distance from the sun) was 1·4844, and its distance from the sun was 1·4958, the earth's radius-vector being 0·9005. Hence we find the angle at the comet between lines supposed to be drawn to the earth and sun respectively was 38° 49′, from which it appears that an angular extent of 23° would give a real length, as a prolongation of the radius-vector of rather over 196,000,000 miles. But this must be an outside estimate of the linear distance of the extremity of the tail from the nucleus, as there was sensible curvature of the tail, the effect of which may be readily seen by a graphical process upon the above data.

We subjoin the Melbourne meridian-observations, to which reference has been made:—

	Melbourne M.T.				Apparent R.A.				Apparent N.P.D.		
	h.	m.	s.		h.	m.	s.		°	′	″
Sept. 14...	23	10	13·7	...	10	45	53·34	...	89	55	47·1
15...	23	22	36·6	...	11	2	14·89	...	89	29	39·2
16...	23	39	9·3	...	11	22	37·75	...	88	47	55·2

The observation of September 15 was made with great difficulty, the comet being obscured by cloud.

THE following communications speak for themselves:—

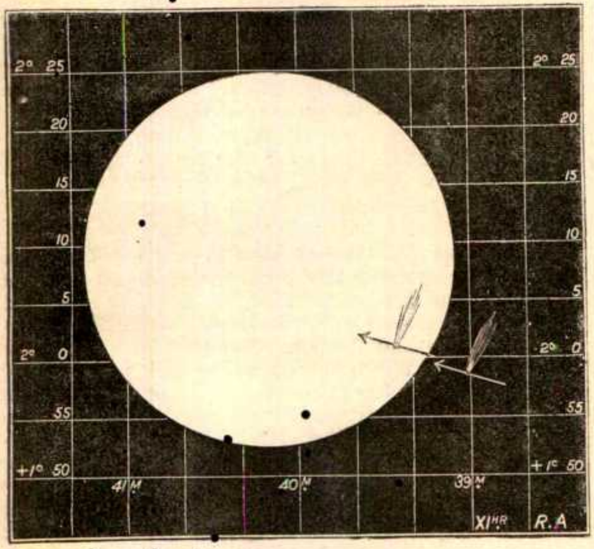
Columbia College, New York, November 4

DEAR SIR,—I have received the inclosed communication from Prof. Chandler, of Boston. The letter may interest your readers.

J. K. REES

Harvard College Observatory, Cambridge, October 28

DEAR SIR,—Your note of the 26th inst. was duly received. I respond cheerfully to your request, although as I have but a quarter of an hour at my disposal, I trust you will regard my answer as furnishing in a disconnected form the principal points in the results so far reached by me, and will bear in mind that I have not had an opportunity to arrange them in a more formal shape. Of course the most interesting point in connection with this comet, astronomically, is the opportunity afforded to decide the question of the disturbance which a comet will experience in passing through the coronal regions in the close vicinity of the sun. Of all the comets which have passed near enough to be disturbed by this cause, this is the only one which has been observed on both sides of perihelion. Not to mention others, the comets of 1680, 1843, and 1880, all of which present such close resemblance to the present comet, as to have raised in some quarters the question whether they are not, in fact, returns of the same body, were observed, either insufficiently to decide this question of disturbance in the sun's upper atmosphere, or were observed only on one side of perihelion.



Ingress of Gould's Comet upon Sun, September 17, 1882.

In the case of this comet, however, there will be available a very extensive series of accurate observations at the Cape of Good Hope from September 8; almost continuously up to within two hours of perihelion passage, ceasing only with the ingress of the comet upon the sun's disc, the instant of disappearance being accurately observed; an observation unparalleled in astronomical history, and of the greatest value. The comet was also observed at Rio Janeiro on September 11, and probably followed up to perihelion.

I have also received from Dr. Gould a private letter dated September 15, on other astronomical matters, at the end of which he states incidentally that a brilliant comet had been visible there "for more than a week, of which he had two observations, and was awaiting clear weather, in order to observe it in the meridian." Thus in all probability he was the first to descry the comet, as, by a curious coincidence, he was the first to see the one, which so closely resembled it in 1880.

After perihelion of course there exists, and will be accumulated hereafter, an abundant body of data to fix its orbit, after emergence from the coronal regions. Of all the observations before perihelion, we are in possession as yet only of a position on September 8 at the Cape of Good Hope, the time of ingress upon the sun's disc on September 17, and Mr. Common's observations on September 17. The last, Mr. Common's, I have not yet examined; but from the others I have been led to conclude that little if any disturbance could have been caused by resistance experienced in the sun's atmosphere, so to call it, for the sake of convenience.

The grounds of this conclusion are the following:—Taking all the observations available about a week ago, others have come to hand since, and verify the calculation, although they could not be used in it, which were made since perihelion passage, *i.e.* from September 18 to October 20, I first computed an orbit from normal places, assuming the orbit to be a parabola, with the following results:—

1882.

$$T = \text{Sept. } 17{\cdot}22013 \text{ Greenwich M.T.}$$

$$\left.\begin{array}{rrrr} & {}^\circ & ' & '' \\ \pi = & 55 & 22 & 26{\cdot}8 \\ \omega = & 69 & 28 & 46{\cdot}4 \\ \Omega = & 345 & 53 & 40{\cdot}4 \\ i = & 141 & 55 & 15{\cdot}0 \end{array}\right\} 1882{\cdot}0$$

$$\log. q = 7{\cdot}8915778$$

The deviation of the middle place $(c-o)$ was $+18''{\cdot}8$ in longitude and $+8''{\cdot}8$ in latitude. It was very plain that the observations could not be satisfied better than this by any parabolic hypothesis. I accordingly computed an elliptical orbit as follows:—

$$T = \text{Sept. } 17{\cdot}2304 \text{ Greenwich M.T.}$$

$$\left.\begin{array}{rrrr} & {}^\circ & ' & '' \\ \pi = & 55 & 12 & 41{\cdot}2 \\ \omega = & 69 & 22 & 7{\cdot}2 \\ \Omega = & 345 & 50 & 34{\cdot}0 \\ i = & 141 & 54 & 56{\cdot}2 \end{array}\right\} 1882{\cdot}0$$

$$\log. q = 7{\cdot}8835636$$

$$e = 0{\cdot}9999700$$

Notwithstanding the nearness to unity of the value of the eccentricity thus obtained, I believe that the ellipticity of the orbit is real, although the corresponding period is very long, something about 4000 years. Whether this is so or not is not of great importance as regards my present purpose. If now we take the observation of September 8, nine days before perihelion, and compare it with the places which are assigned by these orbits, we find that the difference is only 2½ seconds in right ascension and something over 1′ in declination. Thus the differences (Computation—Observation) are for the

	$c-o$	
	$\Delta\alpha$.	$\Delta\delta$.
Ellipse	$-2{\cdot}5$s. ...	$+75''$
Parabola	$+2{\cdot}5$s. ...	$+95''$

quantities which are certainly not larger than the uncertainty of the calculation, that is, not greater than we ought to expect even if the comet had been subjected to no chance of disturbance.

Again, if we compute the place which would be assigned by the two orbits for the instant of ingress of the comet upon the sun on September 17, as observed at the Cape of Good Hope, and also the place of the sun, we have their relative positions as shown in the inclosed diagram, where the calculated places of the comet are indicated by the sign ☌ for the ellipse and parabola in red and black respectively, and the arrows indicate the direction and amount of the comet's motion in a quarter of an hour, as calculated by the orbits. It is significant that it would be necessary to assume a correction of only five or six minutes in either time of perihelion passage to bring the comet exactly upon the sun's limb, where observations indicated it should be. As it cannot be considered that from present data we are certain as to the true time of perihelion passage within this amount, it seems that we have no reason to suppose that there has been any effect of retardation experienced. In fact the deviation shown by the ellipse is opposite to that which would have been the result of such retardation.

It should be remarked (as being of interest) that at the instant of entry upon the sun, the comet was about 1,600,000 miles from its surface (the true anomaly being about 90°).

The perihelion passage took place less than two hours after. The whole half circuit of the sun (from $v = -90°$ to $v = +90°$) occupied but $3\frac{1}{2}$ hours. It is certainly an interesting fact to consider, that an object of such limited dimensions and small gravity can pass at such an enormous velocity for hours through the sun's upper atmosphere, and emerge with so slight an effect on its motion as this body has apparently experienced.

An additional argument in support of my conclusion that little or no disturbance was suffered can be drawn from the fact that the comet, after passing this ordeal, is departing with nearly parabolic velocity, as the slight variation of the eccentricity from unity in the above elements proves.

Another interesting point which I would simply indicate, without discussing, is the bearing of the visibility of the comet clear up to the sun's edge. Prof. Pickering has suggested that the light which rendered it visible in this position must have been nearly all from the comet's own incandescence, scarcely any of it from reflection of the sun's light.

I think that the orbits which I have given may be considered as setting at rest completely the idea of identity of the present comet with those of 1668 and 1843. I say nothing of that of 1880, since there, although the hypothesis of its identity has been entertained in some quarters, it cannot for a moment be regarded as tenable. I have elsewhere shown that the deviations between the observations in 1880 and any hypothesis involving an ellipse of less than ten years' period for that comet, are too large to be considered for an instant as probable. The hypothesis of identity with comet 1880, I., may therefore be left to the sensation-mongers.

I inclose a copy of the *Science Observer Circular*, the regular issue of which will be out in a few days. The figures I have here given differ very slightly from those in the printed circular, but you may regard what I give in this letter as the latest. The elliptical orbit will dispose of the systematic deviations in the table (columns $o-c$) completely, and leave only the unavoidable observation errors.

You may make what use you please of this, except to treat it as a formally-prepared paper.

S. C. CHANDLER, Jun.

INFLUENCE OF "ENVIRONMENT" UPON PLANTS

IN the *Indian Forester* for July, 1882, Dr. Brandis, Director of the India Forest Department, has given the following interesting particulars as to the change in the season of flowering of the Australian acacias introduced in the Nilgiris:—

"*Acacia dealbata* was introduced on the Nilgiris before the year 1845. Col. Dun, the owner of many houses in Ootacamund, had planted several trees in his compounds, probably several years before 1845, but the tree was by no means common, and as late as 1855 was sold at the Government gardens, at two annas a plant. A curious fact regarding the flowering of this tree has been observed:—In 1845, and up to about 1850, the trees flowered in October, which corresponded with the Australian flowering time; but about 1860 they were observed to flower in September; in 1870 they flowered in August; in 1878 in July, and here, this year, 1882, they have begun to flower in June, this being the spring month here, corresponding with October in Australia. All the trees do not flower so early, because at various times seeds have been imported from Australia, and the produce of these would of course flower at the same time as the parent trees in Australia, until acclimatised here.

"Having watched the flowering of these trees for nearly forty years, there cannot be any doubt in the matter; and it is a curious fact that it should have taken the trees nearly forty years to regain their habit of flowering in the spring. Commencing in October, our autumn, it has gradually worked its way back to summer, and finally to spring; probably it will remain at this point."

I have tried to see whether any similar change of season could be traced at Kew.

Acacia dealbata can only be grown under glass with us. It forms a small tree in the Temperate House, and is a splendid object when in full flower. This usually takes place in early spring or towards the end of winter, say about February. Sir Joseph Hooker observed that *A. dealbata* and *A. decurrens*, var. *mollis* (which are closely allied species), flowered at the same time in Tasmania. In Aiton's Hortus Kewensis (1813, *A. decurrens*) is said to have been introduced in 1790 by Sir Joseph Banks, and to flower in May-July. The evidence, then, as far as it goes, would seem to indicate that the flowering time had also progressively worked back in England, though under more artificial conditions.

W. T. THISELTON DYER

THE MAGNETIC STORM AND AURORA

THE telegraphic system of this country has, since Friday morning last, been disturbed in a way that far exceeds anything of the kind that has ever happened before. Very powerful electric currents have been swaying backwards and forwards through the crust of the earth, taking all telegraphic circuits in their progress, and entirely stopping communication. Communication has been maintained only where it was possible to loop together two wires, so as to avoid the use of the earth altogether. The electric storm commenced on Thursday, but it reached its climax on Friday morning (November 17) between 10 and 11 a.m. The currents measured over 50 milliampères, which is five times greater than the ordinary working currents. They have repeated themselves at intervals ever since, but have scarcely attained such an intensity as on Friday morning.

Mr. Preece, whose experience in examining earth currents now extends over a period of thirty years, asserts that this storm was the most terrific he has ever observed. It was characterised on Friday by a rapid succession of alternate waves of great strength.

Both the storm and the aurora seem to have extended to America; the Philadelphia correspondent of the *Times* telegraphs under date November 19:—

"The electrical storm which began to derange the telegraph wires on Friday last still continues, though with less intensity. It spread through Canada and the greater part of the United States, as far west as Utah. The electricians say that the disturbance was unlike any heretofore known, acting upon the wires in strong waves, which produced constant changes in the polarity of the current. A magnificent aurora appeared on Friday night and was visible at all points, except where clouds obscured it. Cold weather, with snow, accompanied the storm in many places."

We have received many letters on the auroral phenomenon of Friday last; as introductory to these we give the following communication from Mr. W. H. M. Christie, the Astronomer Royal, under the title of "Magnetic Storm, Aurora and Sunspot".—

A remarkable magnetic storm, preceded by several days of considerable magnetic disturbance, was observed here on November 17. It commenced suddenly—November 16, 22h. 15m. G.M.T.—with a great decrease in all the magnetic elements, the declination being diminished by more than 1°, the horizontal force by more than 1-100th part, and the vertical force by nearly 1-100th part. From 4h. to 7h., and also from 11h. to 17h., the motions were large and violent, the range exceeding 2° for the declination, and 1-50th part for the horizontal and vertical force. Earth-current disturbances were also recorded, corresponding both in time and magnitude with the magnetic changes.

In the evening, as soon as it was dark, a brilliant aurora was seen, commencing with a bright glow of red light extending from the north and west beyond the zenith, interspersed with pale green phosphorescent light and streamers. At 6h. 4m. a very brilliant streak of greenish light about 20° long appeared in the east-north-east, and, rising slowly, passed nearly along a parallel of declination, a little above the moon, disappearing at 6h. 5m. 59s. in the west, about two minutes after it was first seen. The whole aurora had faded away by about 7h., but it burst out again at 11h. 45m., when an auroral arch, with brilliant streamers reaching nearly to the zenith, was seen from north-north-east to north-west. It faded away about 12h. 10m.

A remarkable sun-spot, visible to the naked eye, was seen on the sun on November 17 and following days, photographs being obtained on November 18, 19, and 20. Its dimensions on November 18, when it was near the central meridian, were: Length 194″, breadth 130″, area of umbra 735, of whole spot 2470 (expressed in millionths of the sun's visible surface), and its position: Heliographic latitude 19° N., longitude 121°. Its spectrum showed C, F, D_3, and the D lines reversed over the principal nucleus, C and F being extremely bright, and D_1, D_2, D_3 doubly reversed. It slightly diminished in size on the two following days. This is the largest spot that has yet been photographed at Greenwich.

Another very active magnetic disturbance commenced on November 19, soon after midnight, and at noon to-day (November 20) it is still in progress, all the elements being greatly disturbed.

W. H. M. Christie

Royal Observatory, Greenwich, November 20

An extensive aurora occurred last night, though I cannot pretend it was well seen here, both clouds and smoke preventing that. About sunset, and before any aurora had manifested itself, the smoke of the city was simply fearful on every side, rising in enormous volumes, through the calm air from a general bed or bank of it, blue gray below, brown above, that stood ten degrees high on every side in impervious thickness, as seen from the top of the Calton Hill. And no wonder that we neither imprison, nor even fine, those who wilfully thus besmirch the skies and poison the air of the people, when the chief offender was a chimney in the prison establishment itself; a chimney built like an ornamental watch-tower on a medieval Norman castle, but now sending up the most atrociously black column of pitchy coal smoke of all the chimneys around, and in vortex whirls that rose up to and fouled the very zenith sky; leaving in fact no portion whatever of the celestial hemisphere where a pure, unadulterated, and irreproachable optical observation of any astronomical phenomenon could be made, to compare with one through a natural, clear atmosphere of oxygen, nitrogen, and water gases.

About 8 or 9 o'clock aurora began to forcibly manifest itself, chiefly at heights of above 15° or 20°, smoke forbidding direct view lower down. Yet the aurora there must have been exceedingly bright, for the cirro-cumulus clouds above that elevation were often brilliantly illuminated from below, as by a morning dawn. The brightest displays occurred about midnight, and more in the north-east than the usual north-west direction. They seemed all to be of the usual monochrome, citron colour, and mostly took the form of needle-shape jets shooting upwards from a low, but broad circular arc, which they themselves assisted in forming; with this peculiarity too, that while no dark space was seen *below* the arc, as so often occurs, such a space, eminently and distinctly aurorally dark, was formed near the middle of the north-east arc itself, in the shape of a black break in that arch, of about five or six degrees wide, and sharply terminated on either side, while no other part of the sky, whether clear, cloudy, or smoky, could be called more than gray in its degree of darkness.

Auroras of one kind or another have been so frequent here for several weeks past, that, taken in connection with the many large sun-spots, I trust Prof. Simon Newcomb will be now quite satisfied touching the philosophic doubt he expressed a few years ago in his "Popular Astronomy," published during the dark, aurora-less nights of 1876–7. For he, at that time, hesitated to consider the past auroras of, and about, 1870 a consequence of, or anything more than a coincidence with, that maximum period of sun-spots; but showed his kindly feeling for the hypothesis by saying, that if the auroras became numerous again at the next maximum of sun-spots, the connection of the two phenomena would stand on a much surer basis.'

Now sun-spots have been of late so large and frequent that I have had not a few letters and communications about them. The last such party was a brace of newspaper reporters, who came together, open-mouthed; for having heard from country correspondents that spots had been discovered by them with the naked eye on the sun, they came to ask me whether it could be true!

Wherefore I could only tell them that it was exactly what should be at this time; and I pointed their attention to a framed and glazed copy of my map of the temperatures, and rise and fall of the sun-spot numbers from 1826 to 1878, its date of publication; but with the sun-spot curve carried forward in outline, and marked with a future maximum for 1882.

C. Piazzi Smyth,
Astronomer Royal for Scotland

15, Royal Terrace, Edinburgh, November 18

Other correspondents will doubtless communicate to you their observations upon the enormous sunspot now visible, and the magnificent aurora witnessed on Friday night, the 17th inst. My object in writing is to contribute a few notes respecting the grand magnetic storm registered by the Kew magnetographs. The disturbance commenced about 8.30 p.m. on the night of Saturday, the 11th inst. Throughout the whole of Sunday, Monday, and Tuesday the magnet continued slowly oscillating through arcs of about 20′ on either side of its normal position. On Wednesday and Thursday the vibrations were frequent, but very small, partaking rather of the nature of tremors. About 10.30 a.m. on Friday the storm became violent, and from that hour up to 5.30 a.m. of Saturday, the oscillations of the magnet and the changes of force were incessant and frequently enormous, the declination needle ranging at times through almost 2°. Correspondingly large variations were also exhibited by the bifilar and balance magnetometer. The largest deflections were between midnight and 5 a.m. of Saturday. Through that day the movements were somewhat more sluggish, and from 2 a.m. of yesterday up to 1 a.m. this morning (Monday) the disturbance was but trivial; it has now become again intense, and at the time of writing (noon) it is found that the needles are moving in arcs extending beyond their limits of registration. Observing the large sun-spot yesterday, it was seen that the image projected upon a screen exhibited traces of coloration, yellow and red, in parts of the penumbra; this was noticed both with the photoheliograph and a Dollond refractor by two observers; probably it will not have escaped the notice of other correspondents. The electrograph does not show any particular disturbance of atmospheric electricity during Friday night's aurora. The tension was much higher and more variable during the dense fog of the succeeding morning.

G. M. Whipple

Kew Observatory, November 20

An aurora was seen here last night. At about 5 o'clock p.m. I was told the "northern lights" were visible, and found that patches of rose-coloured clouds were forming in both the east and west, the larger and brighter portion being in the latter part of the sky. At times these were varied by a white glow, and occasionally there seemed a disposition on the part of the red patches to form into columns or beams. This, however, was never perfected, and no corona actually formed. At a little before 6 o'clock a strange and most unusual phenomenon was seen. I happened to turn to the south, where the moon (with a very pronounced lumière cendrée on its dark part) was nearly on the meridian, when I saw a spindle-shaped beam of glowing white light, quite unlike an auroral ray, had formed in the east. As I looked this slowly mounted from its position, rose to the zenith, and passed it, gradually crossing apparently above the moon, and then sank into the west, slowly lessening in size and brilliancy as it did so, and fading away as it reached the horizon. The peculiar long spindle shape, slow gliding motion and glowing silver light, and the marked isolation of this cloud from the other portions of the aurora made it a most remarkable object, and I do not recollect in any former aurora to have seen anything similar. About 6 o'clock the aurora gradually died away, to revive again at 9 in the shape of a white semicircle of light in a point north by west, which did not last long. Owing to moonlight, but little could be done with the spectroscope with a wide slit on the most glowing parts of the red patches only the usual green line, with a faint continuous spectrum towards the violet could be made out. At times I thought I caught traces of other lines, but with no certainty at all. The spindle-shaped beam was also examined with the spectroscope, but only gave the green line. Even in the brightest parts of the red glow; the red line could not be made out. The peculiarity of the moving beam of light was its absolute southern position. Its apparent passage across the sky was only a few degrees above the moon, then at a comparatively low altitude. J. RAND CAPRON

Guildown, Guildford, November 18

P.S.—In connection with the aurora of last week, it is interesting to notice the great disturbance of the telegraphic needles which has taken place, as I understand, all over the country. At the local post-office here all the longer lines were much affected during Friday and Saturday, sometimes to an extent interfering with ordinary messages. On Sunday morning my own time signal needle, though connected only with a short (mile and a half) wire, showed continuous disturbance; and this morning I have been watching a needle at the post-office which was working independently of any message or induced current from other wires. The effect upon the needle was not violent, but it gradually drew them over to one side or the other, where they remained a short time, and then steadily returned; and by rotating the disc containing the stop-studs, it was easy to follow the considerable deflection which took place. I saw a message sent during one of these deflections. Of course the needle was violently disturbed for the time, but returned to its deflected position afterwards. From inquiries I made, the deflections, whether to right or left, varied considerably, both as to occasions and length of time during which the needle was drawn aside, and there was no special tendency as to direction of the current. From these observations it would seem we have just [illegible] auroræ in active play around us, though from daylight and other circumstances, not always visible as on Friday night. Saturday and last night I saw no actual auroræ, but my assistant thought there was a red glow in the clouded sky of Saturday, and last night there seemed to be a white glow in the east not accounted for by the moonlight.

Since writing the above I learn that the currents have been *very* strong to-day between 1.30 and 2, and working with London intercepted. The needle generally vibrated to and fro, showing a twisted direction of current.—J. R. C.

Guildown, Guildford, November 20

A MAGNIFICENT aurora was visible here on Friday night, 17th inst., which was remarkable not only for its brilliancy but for the successive changes in its character as the night advanced. At about 4.45 my attention was arrested by a splendid rosy light as from a cloud over-head, though the sun had withdrawn its light from the hill tops at 4 o'clock. It looked like a broad irregular band of cloud stretching across from west to east, but crossing south of the zenith. A little later bundles of rays of light formed in it, slowly waxing and waning; they appeared in the mass much as crystals forming in a concentrated solution, and without, so far as I could see, any parallelism or harmony of direction. Some of them were visible also in the N.E. away from the general mass. At 5.30 the lights were not so bright and by 7 o'clock nothing could be seen. At 8.30 a splendid display occurred. There was still some red light coming up from the west and stretching towards the zenith, but a low corona was displayed in the northern skies. The crown of the arch was magnetic north. Its lower border was a jagged edge upon the dark space below it, formed by broad and narrow bundles of rays of slighty yellow light all extending radially from the corona very high up over-head. These varied in intensity at different times and seemed to be travelling now east, now west; but the greatest display was at the corona. A band of light similar in character and movement appeared below the corona in the N.N.E., but was not continuous beyond or up to the middle of the arch. Up to this time there had been no rapid flashing of light from the horizon zenithwards. But at 12 o'clock the display was totally changed; waves of light travelling with tremendous velocity upwards from the horizon all round, along certain straight paths momentarily, but repeatedly illumined by them all centreing in a point about 10 degrees south of the zenith formed a magnificient spectacle. It was in plan like an umbrella over one's head, but at the point where the "ribs" should meet the "stick" there was an irregular vortex which looked as if it might be made of clouds, but was not, for it was illumined by the flashes in the same way. At 12.30 it was fading away and when the comet was rising at 3.15, I could see nothing more of it in the east and south, the only directions in which I could see. R. H. TIDDEMAN

H.M. Geological Survey, Kirkby Stephen,
Westmoreland, November 19

AURORAS of varying brilliancy were seen at York on the 12th, 13th, 14th, 15th, 17th, and 18th (Morning of 19th) November, the 16th and evening of 18th being too cloudy for observation; the 17th giving an exhibition of exceptional brilliancy. On the 13th, 14th, and 17th a low arch was visible (5° to 15° altitude), above which a green light was very evenly diffused for 10° to 20°, then shading off in a more or less patchy manner. Streamers were rare and transient, always of the green light. At 10.15 on the 14th two appeared just west of north, broad, short, but very intense, starting from about ½° *below* the arch. At 12 on the 17th similar streamers reached 40° to 50° up, the bases being fog-hidden. Each night the display was observed soon after dusk, and was seen to last, on three occasions, till after midnight. On the 7th, at York, it seems to have begun, suddenly, at 5 precisely; the same hour is also given me from Street, Somerset, by Joseph Clark. Seen by me at Leeds from 5.15 to 6 o'clock, masses of an exquisite rose-crimson spanned the heavens, rising from near Arcturus, having Vega near the centre, and reaching down south, at times a few degrees beyond Atair, and northwards to and even beyond Polaris. Hence the illumination passed on to the east and south-east, changing imperceptibly to green near the horizon, the same colour, as on previous evenings filling the northern sky, the arch centre almost due north-north-west (magnetic north). The light was evenly spread, fading gradually into the green (which was faint) on the north, over Headingley, into a very clear sky, brightly lit by the moon on the south, and over Leeds. There were at this time no streamers, no scintillations. The bright areas expanded and contracted rapidly, but yet imperceptibly. At 5.25 a green arch suddenly shot across south of the crimson areas, very defined 1½° to 2° broad, from west-south-west to east-south-east, passing just *over* the moon. It lasted hardly a minute; the crimson cloud was then bright. Just such a "bar" "shot out" from the south-east at Street, soon after 6, "of yellowish light; it quickly increased in size and brilliancy, and went right across the heavens to the south-west," passing across in less than four minutes. It passed *south* of the moon (*i.e.* apparent altitude really the *same* as that at 5.25, Leeds being nearly 3°, 6 diameters of the moon, north of Street). My cousin continued:—"There seemed to be a dark something before the bright bar, which showed the path it would take, also a dark streak where it passed. The postmaster tells me that the telegraph-needle worked very badly this *afternoon*, turning to the right hand constantly." (The wire runs about north and south for two miles of Street at the south end). The following suggestions arise in connection with this series of auroral displays. Except the brilliant crimson cloud of the 17th, the phenomena on the various nights were very similar; *i.e.* the green glare very uniform, streamers rare, and unusually thick; the low arch over a dark, hazy, apparently cloudy space. It is said that clouds always lie near the north horizon during auroras in Great Britain. Is it certain that these in some cases may not be part of the special phenomenon? Certainly, I have always found it *look* cloudy. If such were the case 100 miles or more south of Leeds on the 17th, such "clouds" must have been where, from Leeds, the south to south-west horizon looked specially clear. Again, is the apparent shadow before an advancing ray or bar only an illusion? It certainly is a not unusual impression. That the bars of bright light seen at Leeds and Street occupied the same relative elevation is striking. If such phenomena are produced at heights of about 50 miles, and supposing the moon's altitude were 25°, the bar seen at Leeds about 5.25 should have passed a little south of the zenith at Birmingham, a few degrees below the Pole Star near Gloucester, and 30° from the north horizon at Street. Again, do we view an actual *object* in auroral displays, and not, as the rainbow, a subjective impression only? If we do, and the display were 50 miles high, the altitude of Atair being, then, about 40°, this southern limit of the red cloud would be about 40° *north* of the zenith at Birmingham, 30° at Gloucester, under 20° at Street. If it was more extended, then either the display must exist at a much greater altitude, or it must be in some way subjective in nature. If it were 100 miles above us, or far higher than is now usually supposed, still the limit of the display would have been 10° south of the zenith at Birmingham, 10° and 35° north of it at Gloucester and Street respectively. For at Leeds, from 5.15 to 6 o'clock, the southern limit reached rarely and only a few degrees below Atair. Finally, since auroras are likely to be frequent at present, could not a regular corps of observers be organised over the United Kingdom, as has been done in the case of meteors? A few data accurately recorded for time and position at two or three localities, would settle definitely the above question, and if auroras are actual objects, the height of the display. The *lower*, well-defined edge of arches, angular height, and point of the compass of streamers, and limits of the coloured clouds might all be determined with comparative ease by star reference.

Bootham, York, November 18 J. EDMUND CLARK

P.S.—November 19. A sixth aurora last night, seen at 5.45 a.m.; the comet as well defined as a month ago, except the nucleus.

LAST evening there was a very fine display of the aurora borealis visible in York. I noticed it first at 5h. 15m. in the west: a large patch of brilliant rose-coloured light sprang from the western horizon, and extended some 30° or 40° towards the zenith, tipped by a fringe of pale yellowish-green light; so bright was the colour, as to be suggestive of an extensive conflagration in the neighbourhood. This bank of coloured light gradually extended northward in the form of an ill-defined arch, when suddenly, about 5.45 p.m., another brilliant bank of rose-coloured light sprang up due east, and was joined by the arch extending from the westerly bank of light. Above this arch were extensive streamers of greenish-yellow light extending past the constellations Taurus, Ursa Major, Cygnus, Lyra, Aquila. A second arch of greenish light subtended the eastern and south-western sky, and stretched from Taurus beyond to the south of Aquila to the horizon. The effect was very splendid, for inside this arch of light the moon was shining brilliantly. I have rarely seen so grand a display in these latitudes, and never where the colour was so brilliant. It gradually faded away, and was very feeble when I last saw it, at 7.15 p.m. I watched the ever-changing scene for about an hour. During the month there have been several large spots on the sun, which I have observed each day that it was possible to make an observation, with a 4¼-inch refractor by Cooke. H. CLIFFORD GILL

Bootham, York, November 18

P.S.—I see in this morning's paper that the telegraphs have been seriously affected by the magnetic storm, not only in England, but on the Continent.

A FINE aurora was visible from here last evening. When my attention was first called to it a few minutes after 5, the whole northern half of the heavens was suffused with a ruddy glow, as though there was a fire in the neighbourhood. Without paying further attention to its general appearance to the eye, I at once proceeded to examine it with a spectroscope, and found a distinct and sometimes quite bright green band. By the aid of a micrometer scale attached to the spectroscope I took about half a dozen readings of the position of the green band, and successively compared its position with that of one of the bands in the spectrum of the flame at the base of a Bunsen burner. My readings were necessarily taken hastily, but they uniformly agreed in being nearly coincident with, but slightly more refrangible than, the band of wave-length 5581, in the flame of the Bunsen burner. The green band was certainly nearer the hydrocarbon band of wave-length 5581, than to the next one in the same group, on the more refrangible side of wave-length 5542, and so agrees well with Ångstrom's measurement 5567. The ruddy colour varied in intensity and position for about an hour, and soon after six disappeared. I found the green band was easily seen by directing the spectroscope to parts of the sky, on the northern side, even when without it, one would not have noticed any unusual appearance. I also thought I saw indications of blue or indigo bands, but I could not identify any with certainty. Later on in the evening, from about half-past seven till a quarter to nine, when the sky was much clearer and the stars and moon were bright, now and then the aurora was very brilliant; but the light was green except just once towards the last, when at about 60° or 70° from the horizon, the ruddy glow appeared for a few moments. About half-past eight the sky from the horizon to about 30° was suddenly so brightly green, that had I not known of the aurora, I should have imagined the appearance was due to green fire. About this time fine green streamers frequently shot upwards to a great height. Unfortunately during the latter part of the display I had no spectroscope with me to make further observations.

HENRY ROBINSON

University Chemical Laboratory, Cambridge,
November 18

MAY I ask space for the record of an observation made during the fine auroral display of Friday evening, which if compared with similar observations made at other stations may serve to determine with considerable accuracy the height above the earth at which the display took place? For the sake of better observation of the aurora I had gone up to the Durdham Downs by which Clifton is bordered to the north, and from which one has an almost uninterrupted horizon in all directions. The sky was every where very clear, even close to the horizon, and the auroral arch was very conspicuous in the north; its summit lying between the stars Delta and Epsilon in the Great Bear. At 3 minutes past six o'clock a brilliant elongated patch of greenish white light appeared suddenly in the east, below Saturn and to the right of it, the centre of the patch being about 8 degrees from Saturn on a line drawn through the planet at an angle of 45° with the horizon. When first seen the patch was about 6 degrees in length and half a degree in width and the ends had a rough splintered appearance. It rapidly increased in length and less rapidly in thickness, till it closely resembled in general appearance the great Nebula in Andromeda as seen with a good telescope, and the length of the conspicuously luminous portion was apparently about as great as the distance between the stars Alpha Pegasi and Delta Andromedae, i.e., about 27 degrees. The breadth at the centre seemed about equal to twice the moon's diameter. I expected it to lengthen out into an arch across the sky like other fainter ones, which were visible at the time between it and the arch to which I have already referred, but instead of doing so the patch began to shift rapidly across the sky end foremost, as if ascending the eastern slope of the arch which I had expected it to form, then after reaching the summit where its length was horizontal, it rapidly descended the western slope and disappeared near the horizon, passing close under the moon at a distance which I estimated immediately afterwards as rather less than three times the moon's diameter, (measuring from the centre of the luminosity to the moon's lower cusp). The duration of the phenomenon was hardly a minute and its brilliance far exceeded that of any other portion of the display. My colleague Mr. Jupp, who observed a portion of the phenomenon from another place estimated the distance from the moon's cusp as four moon's diameters. The width at the centre we agree in estimating at two moon's diameters. It is not, I believe, often that any portion of an auroral display is so easily distinguished from the rest and localized as was this.

A. M. WORTHINGTON

Clifton College, Bristol, November 19

AN auroral display of unusual magnificence, and lasting upwards of four hours, was observed here last evening. At about 5h. the northern quarter of the sky from the horizon to the zenith, was covered with a delicate crimson glow of surpassing beauty, which included evanescent streamers of a deeper tint. These were succeeded by others of a creamy-white colour, which were more persistent, but did not attain so great an altitude. At 6h. 5m., when the display was at its maximum, a remarkable phenomenon was seen—a bright greenish-white band of a lenticular form, about 20° in length and 5° broad (its axis being parallel to the horizon in the south), passed from the south-east to the south-west horizon, attaining an altitude, when due south, of about 20°. It occupied about six seconds in passing from horizon to horizon, and its brightness seemed to be but slightly affected by the light of the moon, which was shining in the south, and below which it moved. The light of the rosy streamers, when first examined at 5h. 15m. with a small direct-vision spectroscope, gave two very distinct bright lines, one in the red (presumably near C), and the other in the green. There was a faint continuous spectrum towards the more refrangible end, but no traces of other lines. Afterwards, when the display was at its best, only the bright line in the green was observed, but it was much more brilliant than before, and could be traced in every part of the sky except in the south. It was weak in the zenith, but towards the north horizon it stood out with extraordinary distinctness, and was especially strong in the lenticular band seen at 6h. 5m. This line could be easily seen in the northern sky when all signs of the aurora had apparently passed away. At 7h. 45m. the glow assumed the form of a well-defined arch, extending from the north-east to the north-west horizon, and reaching an altitude of about 30°. It remained more or less distinct till 8h. 30m., after which time the light gradually diminished, till at 9h. the sky assumed its usual appearance. During the greater portion of the evening the sky was perfectly cloudless. This display was certainly finer than that seen on October 25, 1870, and though fewer bright lines were observed in its spectrum than on that occasion, the two which were seen were far better defined, and much more brilliant.

Kempston, Bedford, November 18 THOS. GWYN ELGER

ON last Friday afternoon at 5.15 I observed in the north a magnificent auroral display. The moonlight mixed with the fading twilight was of course unfavourable to the brilliancy of such a phenomenon: notwithstanding which the auroral glare—suggestive of rose-coloured clouds, alternately intensifying and fading—was a very remarkable spectacle. A sharp frost supervened. C. ROSE INGLEBY

Valentines, Ilford, November 20

A BRILLIANT auroral display was observed here last night. I first noticed the pale auroral arc at 5h. 30m., the top of the arc at that time being just below Merak and Phecta in Ursa Major. At 5h. 40m. red streamers were seen in the north-west and shortly afterwards in the north-east, and then at intervals pale streamers were observed all along the arc. For about five minutes a double arc was visible, a band of dark sky intervening between the two, which combined to form one broad arc, and remained so to the end of the display. At 6h. there was a very apparent waning of the streamers, and at 6h. 30m. they had entirely disappeared The auroral-arc remained until about 9h. 30m. With a Browning's miniature spectroscope I saw the green line very distinctly, while the red streamers appeared to show a very faint red band. Perhaps it is worthy of notice that the sky, which to the naked eye was dark, showed on examination the characteristic spectrum. C. H. ROMANES

Worthing, November 18

ANOTHER splendid display of aurora was seen here last evening, commencing at 5.10 with a column of rose-coloured light in the north-west, which, rapidly becoming diffused, spread upwards to the zenith, a similar glow being visible in the east. In the northern horizon a double arch of white light extended from beyond Capella to the north-west, from time to time shifting its position and increasing in altitude till the two arches had melted into one, from which rosy streamers went upwards. But lovelier and more wonderful even than this display was a shaft of intense white light, which, just as the chimes of the old church clock were dying away at 6, passed rapidly like a flying arch across the heavens at an altitude of about 30 degrees, and vanished below the southern horizon. After 6.45 the rosy tints had gradually subsided, and at 8 a pale light in the north was all that remained, but I have been told that at 12 and 3 a.m. coloured streamers were again visible. E. BROWN

Further Barton, Cirencester, November 18

THE fine display of the aurora borealis was seen here Friday evening from a little before 6 o'clock. The sky was clear, and the moon, seven days old, was well up. The chief features of the aurora were the two patches of deep pink light, one in the west, in the constellation Hercules, and the other in the east, between Capella and the Pleiades; connecting these two patches was an arc of lighter tint passing between the two Bears. At 6.10 a beam extended from this arc to the left of Cassiopea, towards the zenith; at 6.20 this had disappeared, and another very distinct lay through the body of Ursa Minor, right to the zenith, more over the concentration, as it were, of pink light near Perseus in the east had disappeared, and the light ended at Capella. At 6.40 Capella and β Aurigæ were clear of it. The patch in the west did not disappear, but grew fainter. At 6.50, while watching the display, a magnificent meteor fell slowly from the body of the Little to the tail of the Big Bear, leaving a short red tail there. At 7 the pink tint of the auroral arc had almost disappeared, giving place to one of phosphorescent light, extending from near where Jupiter was rising in the east, through the body of Ursa Major, to below Hercules in the west. This grew fainter, till at 7.30 it was scarcely noticeable. But at a little before 11 p.m. there extended a narrower and brighter line of phosphorescent light, slightly arched from 10° to 15° above the horizon. From this, at 11.20, the streamers began to radiate towards the zenith, alternately forming and disappearing, some stretching to the zenith, some only half way. At 11.45 repeated flashes of light swept up along the streamers, happily likened by one of your correspondents to the flapping of a flag in a breeze. At times a long streamer would appear broken off from the arc of light, and fade away. At 12 the streamers had vanished, leaving only the phosphorescent light near the horizon, though now and then a streamer would form. At 12.30 a pink tint appeared in the north-east, and more streamers formed till 12.45, when the light began gradually to fade away, till at 1 a.m. nothing of the display was to be seen. The day had been overcast, wind north, but towards evening it had cleared; during the night it was freezing; the barometer, at 29.6, was rising; the moon had set at 11 p.m., and the sky, free from clouds, was all that could be desired in which to witness this splendid display of northern lights. FRANK STAPLETON

Oxford, November 19

I BEG to hand you an account of the extraordinary apparition of Friday evening last, November 17, as seen at Clevedon, during a brilliant rose-coloured aurora. The time was about 6.15 p.m. There rose suddenly, through the haze in the east, a beam of light, at an angle of some 60° with the horizon. It crossed the cloudless sky rather below the moon, and sank in the west, occupying about eighty seconds in the transit. The trajectory was much flatter than that of the stars, &c., but was at right angles to the meridian, which was crossed at an approximate altitude of 22°. I estimated the length of the beam at 35°, and the breadth at the middle to be 3°; from whence it tapered gadually to a point at each end. The colour was uniform throughout—a very pale yellowish white, without coruscation or change; and there was no indication of a trail, or of any sort of atmospheric disturbance. The impression conveyed to me was that the beam was stationary in space, and comparatively near, and that we were being carried past it by the rotation of the earth. The major axis lay on the apparent path, but in the earlier and latter parts of the course it was much foreshortened; and as the western horizon was approached, a formation of a similar character, perhaps 7° northward, and running on a parallel track, was visible for several seconds before both were lost in the trees. This second object was also noticed by others whose view westward was less interrupted. I watched the whole evening without seeing any tendency to a repetition of the phenomenon. The sky remained cloudless, with the temperature at the freezing point. There was no wind; and the aurora, which continued off and on until past eleven o'clock, at no time threw out any considerable rays or streamers. The strange visitor caused great commotion among the many who were out of doors looking at the aurora, some of them fearing that the supposed runaway comet was coming into collision with the moon, then half an hour past the meridian, and relieved when it passed below it. I had, however, a much better corroboration of the altitude above given, a careful observer who was with me placing a rod in the direction of the supposed meridian passage. The angle closely agreed with my estimate. We now require to know at what place south of this the beam was seen to cross the moon's disc, for computing the actual distance and position. Many of your readers will not have failed to note that a splendid aurora again coincides with rapid and striking changes in the configuration of a gigantic spot in the sun. With a $3\frac{3}{4}$-inch achromatic, I was able on the same afternoon to observe those changes from hour to hour, on a scale I never before witnessed.

STEPHEN H. SAXBY

East Clevedon Vicarage, Somerset, November 20

WHILE watching the grand display of aurora on Friday night from our roof, at about 6h. 7m., my wife and I saw a strange gleam of light rising above a bank of cloud on the eastern horizon, nearly vertically below the Pleiades, like the gleam of another moon rising in a haze. It grew out slowly, as we watched it, into a strong beam of white light slanting towards the south, and we stood in wonderment as it lengthened out making straight towards the moon. Presently its tail was disengaged from the cloud, and it stole through the sky like a long luminous nebulous "cigar ship" exactly across the moon, and away down into the west, sinking as slowly as it had risen. In the middle of its course it was, as well as I could estimate, about 40° in length and about 5° in width. The ends were, I think, slightly tapering and hazy; the sides pretty well defined. I did not notice if the moon's crescent was at all blurred during the passage; my wife is under the impression that it was. The time occupied from first appearance to final disappearance was about one minute. You will probably receive many accounts of this strange apparition. It will be interesting to know the position relative to the moon in which it was seen by different observers. Was it clear of the earth's atmosphere or not?

Woodbridge, November 19 HUBERT AIRY

YOU will no doubt have abundant accounts of Friday's aurora. I have received the following from a correspondent in North Devon, dated Friday 6.5 p.m. . . . . "As we watched, a brilliant comet (apparently) appeared near Saturn" [which must have been low down, a little N. of E.] "and in a direct line between Saturn and the moon" [at that hour nearly in the meridian and 28° in altitude]. "It was about twice as big as the comet." [Here follows a sketch, which the above 'asides' render it unnecessary to copy.] "It travelled stern foremost towards the moon, and was in sight a full minute. As it disappeared it seemed to leave a black cloud of its own shape which also disappeared in a few seconds (an optical delusion perhaps)." It does not appear to have occurred to the writer that this appearance was itself auroral. J. HERSCHEL

30, Sackville Street, November 18

I AM unable to explain the following occurrence which I observed this evening at 6h. 5m. p.m. It appeared to be a well-defined spindle-shaped body of a cloudy consistency, having a brilliant white colour. It subtended a visual angle of about 20 degrees. I first observed it due east, and immediately noticed that it was moving with very great rapidity, as in less than one minute it had disappeared below the horizon in the south-south-west. There was a rosy aurora visible at the time in the north, which, however, was in no way connected with it. The atmosphere was perfectly clear in that part of the heavens traversed by the phenomenon, though in other parts of the sky there were a few stationary clouds visible. A friend who was with me at the time will corroborate all my statements. As I am utterly at a loss to explain this phenomenon, I would be much obliged for any suggestions or explanations from your readers. A. S. P.

Cambridge, November 17

I THOUGHT that many of the readers of NATURE would be interested in a curious phenomenon which appeared during the beautiful coloured aurora on the evening of the 17th. I was watching it from a position commanding a large view of the sky, when, as I was looking south-east, a long patch of white light appeared about 10° above the horizon. This was commonplace at first, but then it quickly developed into a long, gleaming, and well-defined streak. It looked very like two brilliant comets joined end to end by the tip of the tail. This took about a minute to form, and when complete, it started off in the direction of its length in a curved path which gradually rose above the horizon until it culminated at an elevation of 30° on the magnetic meridian; after which the west end inclined downwards, and it continued its journey in inverse order to the south-west, keeping its symmetry and shape like a rigid body all the way, until it reached a position in the south-west, corresponding to its place when forming, and here it halted and dissolved away. The band of light was about 30° long, and beautifully curved along its path. It took about three-quarters of a minute in its transit, which occurred at 6 p.m. It was an extraordinary sight, and I hope some one else has observed it. During the phenomenon, the aurora in the north-east and north-west (magnetic) was very fine, showing rich red and apple-green streamers; these were very steady all the time. I have made a sketch of the band of light, as nearly as I can remember it. It was very bright, even when under the moon. I think this sketch gives a good idea of it, and I inclose it in case it be wanted. The southern sky was quite *clear* at the time. H. D. TAYLOR

Haworth, York, November 19

ON Friday, November 17, we had a great auroral display at 4.30 before sunset, and continuing till 5.30, the heavens were aglow with auroral light of a rosy tint, changing occasionally into silver grey. A haze overspread the sky until 10 o'clock, from which hour till 2 a.m. Saturday the sky was brilliant with aurora. The streams of light culminated near the zenith, and at midnight the magnetic storm appeared to reach its maximum. The magnetic disturbance must have been great for several hours, as nearly all telegraphic operations had to be suspended.

Newcastle-on-Tyne, November 19 T. P. BARKAS

ABOUT 5.20 p.m. on Friday last I witnessed the most remarkable auroral display I have ever seen, and as it only lasted a few minutes, may have escaped the attention of many. My attention was first attracted by a broad crimson band stretching quite across the sky, and almost coinciding with the Milky Way. Some of the bright stars could be seen through it, but gradually it became opaque at the zenith and appeared to concentrate around an opening, forming a complete corona, out of which the rays seemed to boil over and dart out in every direction, but chiefly northwards. It was a most weird-looking sight, and reminded me of "The Glory," as shown in pictures of Saints. Overhead it rapidly faded away, but bright streamers were visible up till 9 p.m., when a thick fog came on.

W. MAKEIG JONES

Wath on-Dearne, Rotherham, November 20

IN connection with the recent appearance of the aurora borealis, a remarkably large sun-spot was visible to-day, occupying a position in about the middle of the disc. The spot might be called an aggregation of spots, from its area. Several minor spots were also visible, which were discrete.

Rugby, November 19 GEORGE RAYLEIGH VICARS

THERE was visible here on Friday, the 17th, between 5.30 and 6 p.m., a display of aurora. My attention was called to it by the ruddiness of the sky towards the north, and I continued watching it till near 6 o'clock. The sky was clouded with cumulo stratus, and the stars only visible here and there through the intervals of these clouds. The centre of the ruddiness or glow appeared to be over Auriga, the most brilliant star of which group was just visible. It extended to the east so as to cover Gemini, and about an equal distance west. It shifted and varied very rapidly, maintaining its ruddy colour, and this very rapidity of shift assured me that it was really an aurora. After 6 o'clock p.m. the clouds nearly completely covered the sky, and neither at 7 o'clock nor at 8 o'clock did I see any further sign of the appearance. I could not distinguish any beams whatever. J. P. O'REILLY

Royal College of Science for Ireland, Stephen's Green,
Dublin, November 18

P.S.—I was informed that on the evening of Thursday a similar display had been noticed.

AT about 6.5 p.m. on Friday a bright, white, cloud-like object, in shape like a fish-torpedo or a weaver's shuttle, was observed to cross the heavens from east to west. Its length was roughly about 30°, and its breadth about 4°. I noted it first shoot up, like a strong electric ray in a fog, a little south of Aldebaran, and slowly, as it were, slide along at the same N.P.D. across the face of the moon (which was shining brightly at the time), and disappear in the west under Atair. Its surface had a mottled appearance; its colour *white;* its motion was slow, being visible, from horizon to horizon, upwards of 50 seconds; its brightness was strong, and did not seem to fade, even when crossing the moon, and it seemed preceded and followed by a strong black margin; though this I suppose was the effect of contrast and subjective only. The aurora was noted here from 4.30 on Friday till about 5 a.m. on Saturday.

JOHN L. DOBSON

Beaumont College, Old Windsor, November 21

THE CHLOROPHYLL CORPUSCLES OF HYDRA

IN the last number of the *Zeitschrift fur wiss. Zoologie* is an article by Mr. Hamann, assistant in the Zoological Institute of Jena, on the "Origin and Development of the Green Cells in Hydra." I cannot allow this article to pass in silence, and think that the pages of NATURE, in which already there has appeared a good deal relative to the supposed infection of animal tissues by green unicellular Algæ, offer the most fitting place in which to lodge a protest against the reception of Mr. Hamann's conclusions as reasonable.

In the first place, Mr. Hamann has not made himself acquainted with previous writings on this subject. He briefly states that "R. Lankester disputes" the algal nature of the green corpuscles suggested by Brandt, and the existence of a cell-nucleus in them, and refers the reader to a paper by me on "Symbiosis of Animals with Plants," which has no existence. Mr. Hamann has not read the article to which he refers, which appeared in the *Quart. Journ. Microsc. Sci.* April, 1882, and was entitled "On the Chlorophyll Corpuscles and Amyloid Deposits of Spongilla and Hydra." Mr. Hamann has accordingly failed altogether to take up the points of importance in the discussion. These seem to me to stand somewhat as follows: It had already been urged (1) that the green corpuscles of Hydra multiply by fission; (2) that they possess each one or more cell-nuclei; (3) that they possess a cell-wall comparable to the cellulose wall of a unicellular Alga; (4) that starch is developed within them even after their removal from the living Hydra. It had been inferred (by Semper, and later by Brandt) that consequently these corpuscles must be considered as unicellular Algæ.

To these considerations I had replied in the article above named, by describing carefully the nature of the "fragmentation," or division of the chlorophyll corpuscles of both Hydra and Spongilla. I cited the notorious fact with regard to the chlorophyll corpuscles of plants, namely, that they multiply by fission. I showed further, by description and figures, that *there is not any structure present in the chlorophyll corpuscles of either Hydra or Spongilla which is comparable to a cell-nucleus or to a cell-wall*, and that the ascribing of such parts to the chlorophyll corpuscles of Hydra is *totally erroneous*.

I further insisted that we are not acquainted with any unicellular Algæ at all resembling the chlorophyll corpuscles of Hydra, whilst the chlorophyll corpuscles of plants closely resemble them,—and finally I pointed out that there is as much reason to regard the chlorophyll corpuscles in the leaf of a buttercup as unicellular Algæ as there is so to regard those of *Hydra viridis*.

Mr. Hamann does not in any way deal with these observations, but naïvely remarks, after describing his observation of the already-known multiplication by division of the chlorophyll corpuscles of Hydra, "after these observations the nature of our green corpuscles as Algæ seems to me to be firmly established." This seeming can only arise from the fact that Mr. Hamann is not acquainted with the characteristics either of Algæ or of the chlorophyll corpuscles of plants.

A simple assertion that a nucleus and a cell-wall are present in the chlorophyll corpuscles of Hydra is all that Mr. Hamann gives us on this head; although his paper is illustrated by a plate, no nucleus and no cell-wall are figured by him. Were he able to adduce good evidence of the existence of either of these structures, the view which he has advocated would be materially advanced. But this he is unable to do, because such structures do not exist.

Mr. Hamann offers some observations on the occurrence of chlorophyll-corpuscles in the egg-cell of Hydra which lead him to assume that these corpuscles enter the egg-cell by "wandering" from the endoderm-cells. The figures and statements which he makes do not, in my opinion, tend necessarily to that conclusion.

Lastly, I would point out that the exceedingly variable form of the chlorophyll-corpuscles of Hydra and Spongilla which I have illustrated by figures in my memoir above cited, is not noticed by Mr. Hamann. This variability is quite inconsistent with the view that they are parasitic Algæ. So also is the fact that these corpuscles are represented by colourless corpuscles in the colourless varieties of Spongilla and Hydra which turn green when treated with sulphuric acid.

It should be distinctly borne in mind that it is by no means necessary, supposing that the green corpuscles of Hydra are parasitic Algæ, that a nucleus should be present in them, nor indeed a well defined cell wall. But when the presence of such structures is asserted as evidence that these corpuscles are different in nature from the otherwise closely similar corpuscles formed in the protoplasm of green plants, the question of the actual presence or absence of the nucleus and cell-wall becomes important, and must be definitely decided upon thorough histological evidence.

So far it appears to me, as I have previously maintained, that there is no more and no less evidence for considering the green corpuscles of *Hydra viridis* as parasitic Algæ, than there is for taking a similar view with regard to the green corpuscles in the leaf of an ordinary green plant.

E. RAY LANKESTER

NOTES

WE regret to notice that in Tuesday's papers the death of Prof. Henry Draper of New York is telegraphed. We hope to be able to refer to his work in an early issue.

THE Council of the British Association have nominated Mr. A. G. Vernon Harcourt, M.A., F.R.S., to the office of General Secretary of the Association, in the room of the late Prof. F. M. Balfour.

MARINO PALMIERI, whose death we announced a fortnight ago, must not be confounded with his father, Luigi, the eminent director of the Vesuvius Observatory, who we are glad to be able to say is alive and well.

THE death is announced, on November 11, of Dr. Franz Ritter von Kobell, Professor of Mineralogy and keeper of the mineralogical State collections at Munich, well known through his numerous mineralogical publications. He died at the age of seventy-nine years.

M. JANSSEN has been sent to Oran to observe the transit of Venus from a physical point of view.

WE have received a circular in reference to the visit of the British Association in Montreal, containing the results of the recent meeting in that city, to which we have already referred. It is evident that the Canadians are determined to do all in their power to make the visit of the Association a success. "The city of Montreal, which has a population of about 150,000 souls has," the circular states, "twice entertained the American Association for the Advancement of Science; for the second time in August, 1882, when an attendance of more than 900 members and Associates was registered, and the Association, with its nine sections, found ample accommodation in the buildings of McGill University. The ordinary summer-passage is made in eight or nine days from Liverpool to Quebec, which city is connected with Montreal by two lines of rail, making the journey in six hours, and by river-steamers. From Montreal to Ottawa, the capital of the Dominion, is four hours by rail; from Montreal to Toronto, thirteen hours; and to Niagara Falls, sixteen hours by rail. Montreal is in direct connection with Boston by two lines of rail, by which the journey is made in ten hours. There are also two lines connecting Montreal with New York city in thirteen hours, and one with New Haven in sixteen hours. It is expected that the American Association for the Advancement of Science will hold its meeting in 1884 in New Haven, or some other eastern city of the United States, at such a time as may permit the attendance of members of the British Association, either before or after their gathering at Montreal. We have assurances that the Government of the Dominion of Canada will make a liberal grant of money to defray the expenses of members of the British Association in crossing the ocean, and that the various railroad and steamboat lines in Canada and in the United States will offer most liberal arrangements to our guests. The Grand Trunk Railway will arrange for an excursion of members of the Association to the Great Lakes and Chicago, while the Canadian Pacific Railroad will give an excursion to the provinces of the North-West, as far as the Rocky Mountains. It is believed that the British Association may count upon a large attendance of local members and associates both from the provinces of the Dominion and the United States. In any case, the Finance Committee are prepared to guarantee that the revenue from this source shall not fall below that ordinarily received by the Association. Members of the British Association in coming to Canada may be assured of a most cordial welcome and generous hospitality, not only from the citizens of Montreal, where every facility will be furnished for their meeting, but from the people throughout the country. It is hoped by the Invitation Committee that these assurances, and the above statement of the advantages and facilities offered them, may secure a large attendance of the members of the British Association at Montreal in 1884."

IN the sitting of the Academy of Sciences of November 20, M. Dumas read an *arrêté* from the Minister of Public Instruction, regulating the competition for the Volta Prize, which will be delivered in 1887. It is expressly decreed that the competition be open to every nation. A report will be made by the Commission *ad hoc*, and published *in extenso* in the *Journal Officiel*.

AT the meeting of the Paris Academy on Monday M. Dumas stated that at the very beginning of its work, the Academical Commission for the destruction of the Phylloxera proposed to arrange for the immediate destruction by fire of each plant proved to be infested. Objections were made to this scheme grounded on the state of French legislation on rural property, and the Academical Commission desisted. M. Dumas states that he has in hand an official report from Switzerland establishing the soundness of the views taken by the Academy on this important question. The Cantons of Geneva, Vaud, and Lucerne having resorted to the destroying process, all the vines, of which the value exceeds 40,000,000*l.*, had been saved at the expense of a few thousand pounds. A special tax had been imposed on the proprietors of vines for compensation to the owners of the destroyed plants.

A FIRST application has been made of the resolutions of the last Congress of Electricians proposing that regular observations should be made on earth currents during magnetical perturbation. The perturbations of November 17 have been accompanied in France by strong earth currents principally in the south-north direction. We may state, moreover, that others were observed on November 20, exhibiting a very great force.

LARGE electrical disturbances have been observed in Sweden and Norway during last week. On Friday last all telegraphic communication became for a time suspended, and at Stockholm and Jönköping central telegraph stations several instruments were destroyed. In Norway the electric storm was accompanied by thunder, a phenomenon almost unknown at this time of the year.

A LARGE and enthusiastic meeting was held on Saturday evening last, in Trinity College, Prof. Moseley, F.R.S., in the chair, when the following resolution was carried unanimously:—"That it is desirable that a society should be formed for the purpose of bringing together the Undergraduates and Bachelors of Arts of the University, who are engaged in the study of Natural Science, for the friendly discussion of scientific and other topics." The officers and members of the new club were elected, Mr. Bond, B.A., being elected president, and the first meeting will be held in the course of next week.

THE Rev. T. W. Webb writes to the *Times* as follows on the comet:—"As it must be universally admitted that the magnificent comet now receding from our sight is the most interesting, in a popular as well as scientific point of view, of any that have appeared for many years, will you allow me to add the record of a very remarkable phenomenon to the somewhat scanty details respecting its aspect which have as yet been laid before the public? In an extremely valuable letter received by me this morning from a very able and careful observer, Mr. J. T. Stevenson, of Auckland, it is stated that on October 6 and 10 an 'anomalous' tail was feebly but distinctly visible, pointing towards the sun. Your astronomical readers will remember that a similar 'glowing wake' attended the returning course of Newton's great comet in 1680, distinguished, like the present, by its close approach to the surface of the sun, and a few more cases might be cited. It is, however, of such infrequent occurrence, that another instance forms a valuable addition to our stock of information as to these mysterious bodies. I ought to mention that Mr. Stevenson's letter was despatched immediately after his last observation, so that we may hope that, with a climate and position of the comet giving him great advantage over northern astronomers, he may have been able to trace this singular appendage on subsequent occasions."

THE German Society for the Prevention of the Pollution of Rivers, the Soil, and the Atmosphere, held their fifth annual meeting at Brunswick on October 19 and 20 last, under the presidency of Prof. Reclam (Leipzig). The number of papers read was considerable and the attendance very large. Among the speakers were Burgomaster Rittmeyer (Brunswick), Prof. Müller (Berlin), Dr. Blasius (Brunswick), Dr. Engler (Stuttgart), Herr Knauff (Berlin), Dr. Gerson (Hamburg), Dr. Petri (Berlin), and Dr. Beckurts (Brunswick).

WE have received from Mr. J. P. Walker, C.E., Stirling, a communication on the Forth Bridge, but we can hardly venture to insert in our columns the descriptions of Mr. Walker's and other plans for such a bridge. The peculiarities of the plan drawn by Messrs. Fowler (chief engineer) and Baker, and the circumstance that it had been accepted by Commissions of Parliament and of the Board of Trade, gave it great claims on our attention, which can scarcely be recognised as applying to any other proposal.

THE Glasgow *Evening Times* has the credit of being the first daily paper, so far as we know, to introduce into its pages star-maps showing the aspect of the heavens at stated times. On November 11 it started with four such maps, and the series will be continued. There are full instructions as to the meaning and use of the maps, and we have no doubt they will be the means of leading many people to form a practical acquaintance with astronomy.

THE first number of the *American Journal of Forestry* bearing date October last has just reached us. It is edited by Franklin B. Hough, Ph.D., Chief of Forestry Division U.S. Department of Agriculture, and has as contributors an array of well known names connected with forestry matters in America. The journal in size and shape corresponds with the *Journal of Forestry* published in this country, and edited by Mr. F. G. Heath. It is not, however, so tastefully got up, though the printing and the character of the articles are very similar. The contents of the number before us, for instance, are, after the Editorial "Announcement," a paper on "Forestry in Michigan," one on "Larch Wood," one on the "Forestry of the Future," by the Editor, on "Forest Fires," on the "American Forest Congress," and the usual "Miscellany." The forests in America are so extensive and there is so much connected with them that belongs legitimately to the subject of forestry that we have no doubt the journal will meet with a wide circulation.

THE Annual Report of the Public Gardens and Plantations in Jamaica for the year ending September 30, 1881, has just reached us, and from it we gain some idea of the work that is being carried on in the island under Mr. Morris's care in the dissemination of useful plants. It is satisfactory to find that of late years a considerable amount of attention has been directed to the extended cultivation of economic plants in all our Colonies, a branch of culture that must in the end prove of more lasting value to mankind generally than the growth of any mere horticultural novelty or scientific rarity. Mr. Morris's Report from beginning to end is a record of what can be done by a single establishment in the introduction of new plants and their distribution amongst planters in the several colonies. As an illustration of this Mr. Morris says "there is much activity displayed even by the poorest peasants in obtaining and cultivating new and important plants, and I cannot but hope, that before many years have elapsed this activity will result in the greater prosperity and wealth of the island, and in placing it in the first rank as exporter of fruit and raw materials to the markets of England and America." Regarding Jamaica in particular, Mr. Morris says: "It is evident that Jamaica must depend for its prosperity and success almost entirely on the resources and products of an agricultural character. We have no large stores of timber, we have no minerals, we have no manufacturing industries, and we cannot hope to struggle successfully with other countries in the more advanced arts and sciences. We nevertheless possess a rich and productive soil, a salubrious climate, abundant springs, and a vast extent of uncleared mountain land; and it is mainly on the due utilisation of these valuable natural resources that our prosperity must ultimately depend. Under these circumstances the chief aim of the Department has been directed towards bringing into notice the nature and character of such resources, and to fostering and promoting any well directed efforts for their utilisation. The position and prospects of several new industries, such as Liberian coffee, cacao, tobacco, oranges, mangoes, pine-apples, spices, india-rubbers, fibre-yielding plants, &c., are carefully noticed with this view, and the success which has already attended these comparatively recent efforts would indicate that capital and energy are alone wanting to place the island in an important position as to the source of most tropical productions." Naturally a good deal of attention has been paid to cinchona cultivation, and a large number of plants of the best varieties have been raised, seeds and plants having been distributed to private plantations, and sold in considerable quantities during the year. The cultivation of the jalap plant promises also to become one of considerable importance in Jamaica.

To give an idea of the dairy-industry in France, M. Hervé Mangon recently stated (at an agricultural gathering) that the milk produced in the country would, if collected, form a stream about 1 metre in width and 33 centimetres in depth (say 3 feet 4 inches and 1 foot 1 inch), flowing night and day all the year, with a mean velocity of a metre per second. Young animals drink a part of this enormous volume of milk, man takes a good part of it, and the rest is transformed into cheese and butter. No branch of agricultural industry has so progressed during the last fifty years as the making of butter. In 1833 France bought abroad 1,200,000 kgr. of butter, and sold to foreigners only 1,100,000 kgr. She now exports 34 to 35 million kgr. of butter annually, and receives in return from abroad (especially from England) a sum of more than 100 million francs. La Manche alone furnishes more than one-third of the total exportation.

A VALUABLE investigation of the origin of metalliferous lodes, by Prof. Sandberger, of Würzburg, has recently been published at Wiesbaden. The various theories are discussed, more especially those of ascension, and of lateral secretion or levigation. Till 1873 the author was a partisan of the former, but he was led to make a chemical study of the gangues and lodes in the Black Forest, and by 1877 he had got so far as to obtain proof, for the greater part of the mining districts in Germany, that the lodes had been formed by levigation of the encasing rock. The second part of the work is elevated to a special study of the environs of Wild Schapbach in the Black Forest, as illustrating the theory of levigation. (An outline of these researches appears in *Archives des Sciences*, October 15.)

AN improved feed-water heater and purifier has been recently described to the Franklin Institute by Mr. George Strong. It is said to effect a saving in coal of 22 per cent., and an increase of evaporation of 1·09 pounds of water per pound of coal. Considering that all substances likely to give trouble by deposition would be precipitated at about 250° F., he obtains this in the heater by action of exhaust steam, aided by a coil of live steam from the boiler. He also uses a filter formed of wood-charcoal, and-bone black firmly held between two perforated plates. (Further details will be found in the *Journal* of the Institute for November.)

IT appears from the *Shen-pao*, a Chinese newspaper published at Shangbai, that the Chinese are taking practical steps in the matter of foreign education. A school for the education of Chinese boys in foreign matters has been established in the Pun-yen district of Canton, and it has already fifty scholars. So far the school has been a success, and to meet the requirements of the scholars it is proposed at the next Chinese New Year to solicit subscriptions to enlarge the school premises. The teachers are Chinese well versed in English, and the school bids fair to be followed by many others of a similar kind. A satisfactory circumstance about it is that the institution has been founded by the people themselves with official countenance or assistance, and that Chinese gentlemen competent to teach these schools are now to be found. European teachers and professors are of course absolutely necessary for a time; but their want of knowledge or imperfect knowledge of the language of the country must cause them to be make-shifts at the best.

UNDER the title of "Les Grandes Ascensions Maritimes." M. W. de Fonvielle has published (Paris, Ghio) a *brochure* giving an account of several balloon ascents over the ocean, including some, such as the late Mr. Powell's, which have come to grief by being driven into the sea.

THE Tenth Annual Report of the Lambeth Field Club speaks hopefully of its condition; it seems to be doing good work.

THE public dinner in celebration of the 100th anniversary of the first experiment at Annonay by Joseph Mongolfier was given on Saturday at Paris by the Académie d'Aérostation Météorologique. Three members of the Mongolfier family were present. Several speeches were given, and a general committee was appointed to organise a national celebration on June 5, 1883, the anniversary of the first public experiment at Annonay before the États Généraux du Forez.

THE *Journal Officiel* states that the director of the Compagnie du Cap has given to the Paris Museum of Natural History a diamond weighing 4½ carats, enveloped in its native rock. It is supposed that this generous donation will determine the public authorities to send to the museum a part of the jewels of the French Crown, which are now kept in the Bank. The question of their sale has not yet been settled, in spite of several parliamentary and extra-parliamentary reports.

NEWS from Perugia now states that the earthquake began on October 28 at 6 p.m., and with short interruptions lasted until

October 29 at midnight. A real panic is reigning among the population of Cascia. The extent of the zone of the phenomena cannot yet be ascertained, but it seems that the eruption of Mount Etna is closely connected with it. Several old houses fell at the first shock.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. A. S. Gissing; two Common Herons (*Ardea cinerea*), British, presented by Mr. R. H. Rabbetts; a Common Barn Owl (*Strix flammea*), British, presented by Mrs. A. Wright; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, deposited; two Red-billed Tree Ducks (*Dendrocygna autumnalis*) from America, a Zenaida Dove (*Zenaida amabilis*) from the West Indies, purchased; a Hairy-rumped Agouti (*Dasyprocta prymnolopha*) from Guiana, received on approval.

BIOLOGICAL NOTES

APPARENT BIRD-TRACKS BY THE SEA-SHORE.—At a recent meeting (October 3) of the Academy of Natural Sciences of Philadelphia, Mr. Thomas Meehan called attention to what appeared to be the tracks of a three-toed bird in the sand near low water-mark, at Atlantic City. These tracks were of a nature that would be readily recognised by observers as bird-tracks; but while thinking of what bird could have caused them, and reflecting on the phenomenon of their being only found on the sand near low water-mark, Mr. Meehan noted on the face of the smooth, receding waves, spots where the water sparkled in the light, and he found this was caused by little riplets as the wavelets passed down over the half-exposed bodies of a small crustacean (*Hippa talpoidea*), and that the water, in passing over the bodies made the trifid marks which had been taken for impressions of bird's feet. These little crustacea take shelter in the sand near low water-mark, and enter head foremost in a perpendicular direction downwards, resting just beneath the surface. The returning wave took some of the surface sand with it, and then the looser portions of the bodies uppermost in the sand were exposed. Often the little creatures would be quite washed out; when recovering themselves, they would rapidly advance in a direction contrary to the retreat of the wave, and would enter the wet sand again as before, their sides being parallel with the shore. Their bodies terminate in a caruncular point which, with the position of the two hind-legs, offer a tridentate obstruction to the sand brought down by the retreating wave, and the water passing round the points made the three toe-like grooves, which resembled a bird's foot from one and a half to two inches long. The crustacea, in their scrambles for protection beneath the sand, managed to keep at fairly regular distances from each other, and hence there was considerable regularity in the tracks, as if they had really been produced by birds. Although the author of these notes presented them as a trifle, yet it will be at once apparent that they are of great interest. Trifid impressions like these, filled with mud and the deposit then to become solid rock, would puzzle, if not altogether mislead, future observers.

AUSTRALIAN FRESHWATER SPONGES.—Up to this date, but one species of freshwater sponge has been described from Australia, *Spongilla capewelli* of Bowerbank; but Mr. W. A. Haswell, at a meeting of the Linnean Society of New South Wales (May 31, 1882) describes two new species from a pond near Brisbane, and one from the River Bell at Wellington. *Spongilla sceptroides* is a green, smooth, encrusting species, with the skeletal spicules very slightly curved, acute at both ends, ornamented with very minute projecting points. The statoblasts are spherical, defended by long, slender, straight, cylindrical spicules, which are armed with numerous acute spinules, chiefly collected around the extremities, forming heads; it is found growing on submerged twigs. *Sp. botryoides* is a yellowish flat-encrusting species, with curved skeletal spicules, fusiform, acute, with scattered, extremely minute, projecting points. Statoblasts protected by a crust of short, strongly-curved spicules, with heads at each end of numerous short, blunt, or sub-acute spines, somewhat botryoidal-like, the shaft smooth. This species was found with the first: another species found by Mr. E. P. Ramsay in the Bell River, growing on masses attached to submerged timber seems nearly related to *S. Meyeni*, from Bombay. In colour it varies from a grass green to a yellow. It is massive, lobulated, with oscula between the projections. The skeleton spicules are perfectly smooth, and the amphidiscs are provided with from one to ten acute and prominent spines. Another species from somewhat deep water is indicated by Mr. Haswell.

EARTH-WORMS IN NEW ZEALAND.—The following interesting observations form part of a communication from Mr. A. T. Urquhart, to the editor of the *New Zealand Journal of Science*, and appear in the September number of that periodical. In October, 1875, I dug a trench on some newly-cleared land—a raised beach at Manukau Harbour. The section then showed about 4½ inches of black mould and a horizontal layer, 1 inch thick, of burnt clay, wood-ashes, small stones, and pumice lying on a brownish-green arenaceous clay. The vegetation cleared was the growth of some thirty years. A portion of the land was left undisturbed. Measurements again taken a few days ago gave an average depth of 1¼ inches of turf, 5¾ inches of black mould, and there was no perceptible difference in the layer of ash. An angular block of Trachyte—about twenty-five pounds in weight—placed in May, 1875, had sunk 1 inch, allowing for the turf. As the results of some accurate calculations, as to the number of worms per acre, Mr. Urquhart gives results so considerably higher than Henson's, that he would have hesitated to publish them, were he not in a position to prove them. Henson, it will be remembered by the readers of Darwin on "Vegetable Mould," calculates that there are 53,767 worms per acre in garden mould, and above half that number in corn-fields. Mr. Urquhart's estimates, founded on digging about a quarter of an acre, as well as by a large number of tests on various parts of the fields, some that were under pasture for over sixteen years, gave from four to twenty-six earth-worms per each square foot. The alluvial flats, slopes, and richer portions of the upper lands would average eight to the square foot or say 348,480 per acre. In the uncultivated fern lands worms are scarce. In New Zealand worms not only leave their burrows, but climb up trees in search of food, this chiefly in the night time, though often until a late hour on damp warm mornings.

THE GENESIS OF THE HYPOPHYSIS IN PETROMYZON PLANERI.—Prof. Anton Dohrn, of Naples, writes:—"In his contributions to the history of development of the Pertromyzons (*Morphol. Jahrbuch*, vol. vii. p. 158), Mr. W. B. Scott says: 'The organ of smell is one of the most peculiar parts of the whole organisation of the Cyclostoms. . . . . The position of the organ is a symmetrical from the very beginning. It first begins to manifest itself as a shallow depression above the mouth, which we may regard as a common depression for the nasal cavity and the hypophysis. The ectoderm covering the head becomes suddenly thickened at one spot, in order to form the special smell sense epithelia which lie close to the front extremity of the brain. The cells at the bottom of the depression decrease in depth, while the cells that cover the opposite wall of the depression (*i.e.* the continuation of the upper lip) are very low.' Balfour ("Comp. Embryology," vol. ii. p. 358) makes the following criticism upon this statement:—"I have not myself completely followed the development of the pituitary body in Petromyzon, but I have observed a slight diverticulum of the stomodaeum, which I believe gives origin to it. Fuller details are in any case required before we can admit so great a divergence from the normal development as is indicated by Scott's statements." According to researches which I made this summer, the question is solved, but in a way different from either Balfour's or Scott's suggestions. The hypophysis arises rather as an independent depression of the ectoderm between the depressions for the nose and the mouth. Its connection with the nasal depression is only secondary, and is caused by the strong and early development of the upper lip. It has no connection with the mouth depression, because the upper lip develops between the mouth depression and the hypophysis. The particulars of these relations will appear in the next number of the "Studies in the Early Development of Vertebrates" in the *Proceedings* of the Zoological Station at Naples (*Zool. Anzeiger*, November 6, 1882).

FORMIC AND ACETIC ACID IN PLANTS.—Dr. Bergmann sums up the results of his investigations as to the occurrence and import of formic and acetic acids in plants as follows:—1. Formic and acetic acids are met with as constituents of protoplasm throughout the whole of the vegetable kingdom in the most various portions of the plant-organism, and, both in chlorophyllaceous and non-chlorophyllaceous forms.

2. Formic and acetic acids are to be regarded as constant products of metastasis in vegetable protoplasm. 3. It is probable that other members also of the unstable group of fatty acids, as for instance, proprionic acid, butyric acid, caproic acid, or even the whole group, are universally distributed in the vegetable kingdom. 4. An increase of the amount of unstable acids takes place in a plant-organism when its assimilation is interfered with by deprivation of light, *i.e.* when put into a state of starvation (inanition). 5. Formic and acetic acids accordingly belong to the constituents of regressive tissue-metamorphosis. It has been premised that the homologous, unstable fatty acids have a similar import in vegetable tissue-metamorphosis. 6. No increase in the amount of unstable acids takes place in a plant-organism, which is withdrawn for a period from the light, under the minimum-temperature required for growth. 7. Accordingly the formation of formic and acetic acids in a plant seems to take place to a certain degree independently of respiration. 8. Acetic and formic acids are mainly to be regarded as decomposition products of the constituents of vegetable protoplasm.

GEOGRAPHICAL NOTES

DR. WISSMANN, of the German African Society has, it is stated, just arrived at Zanzibar, having left Loando in April, 1881, in company with Dr. Pogge. Striking inwards across the numerous streams that take their origin in the great watershed which separates the Congo and Zambesi basins, the travellers were at Mukenge, about 6° S. and 22° E., in November last year. Shortly after they set out for Nyangwe on the Lualaba, whence Wissmann proceeded eastwards to Zanzibar, while Pogge turned back to Mukenge, there to plant a station. The details of this journey will doubtless be full of novelty and interest.

THE German African Society has recently issued a report upon its latest undertakings. There are now four German expeditions in Africa, two proceeding from the east, and two from the west. In the east there is Dr. Stecker, who as the companion of Dr. Rohlfs, paid a visit to King John of Abyssinia, and then continued his journey through the Soudan. His last letter is dated February 15. Dr. Bohrn and Dr. Kayser, who belong to Capt. von Scholer's expedition, report upon a three months' journey to Lake Tanganyika, from which they returned to the station at the end of 1881. From the Gondo Station itself Herr Paul Reichard, who remained there, sends a report; Capt. v. Scholer, after founding a station at Kakama, proceeded to Zanzibar. News has also been received regarding the exploration of the Wala River, to the west of Gondo, as far as its mouth, by Dr. Böhrn and Herr Reichard. On the other hand, Robert Flegel is busily at work. He has taken a minute cartographical survey of the hitherto unknown part of the Niger, between Inuri and Shay. In the spring of 1881 he prepared for a journey to Southern Adanana. He reached Keffi at the beginning of December; thence he intended to proceed by way of Schiber, on the Binne River, through the "heathen lands" to Kantscha and Yola, south of the Binne, then winter there, and thence proceed by water from Meo Kebbi to the Tubori Marsh to Kuka.

AT the beginning of November, Dr. Arthur Krause returned to Germany from his journey to the Chukchi Peninsula and Alaska, which he undertook, partly in company with his brother, Dr. Aurel Krause, and partly alone, at the instance of the Bremen Geographical Society. Dr. Aurel Krause returned to Germany last summer by way of Panama, while his brother remained in Alaska until the autumn. The two brothers have made copious collections of natural history and ethnographical specimens.

THE November number of *Petermann's Mittheilungen* contains an account by Dr. Gerhard Rholfs of the results of his recent journey in Abyssinia. Dr. Ferd. Löwl, of Prag, has a long and able paper on the origin of transverse-valleys; Lieut. Kreitner describes the route from Ansifan through the Gobi desert to Hami; while there are interesting letters from Emin-Bey, Lupton-Bey, and Dr. Junker, mainly referring to the work of the Russian explorer in the Welle region. He has been doing much to clear up the hydrography of the region, and has come to the conclusion that the Welle is really the upper course of the Shari; while the Aruwimi, the great tributary of the Congo, rises further to the east.

A SPECIAL supplement to the *Chamber of Commerce Journal* contains an account by Mr. Colquhoun of his recent journey through Yunnan to Burmah, accompanied by an excellent map. Under the title of "Across Chrysê," Messrs. Sampson Low and Co. will shortly publish a detailed narrative, with many illustrations, of Mr. Colquhoun's journey.

THE ordnance survey of Scotland, a work which has been going on for thirty-seven years, has been completed, and the surveying staff will be withdrawn from Scotland next week. During the last few years nearly a hundred men have been employed in the work.

THE Emperor of Russia has ordered 2200*l.* to be allotted from the Imperial Treasury to the Russian traveller in New Guinea and the Malay Archipelago, M. Miklucho Maklay, in order to enable him to work up the results of his explorations. His Majesty has also ordered M. Maklay to be informed that the cost of the publication of his book of travels will be defrayed by the privy purse.

THE PELAGIC FAUNA OF FRESHWATER LAKES

SEVERAL naturalists have within recent years made the pelagic fauna of freshwater lakes in various regions a subject of study. In the *Archives des Sciences* for September, Prof. Forel gives a list of those researches, with a *résumé* of the results they have yielded.

This fauna has but few species; but the number of individuals of each species is, on the other hand, enormous. The following is an enumeration of the species observed:—

OSTRACODA: *Cypris ovum*. CLADOCERA: *Sida crystallina*, *Daphnella brachyura*, *D. pulex*, *D. magna*, *D. longispina*, *D. hyalina*, *D. cristata*, *D. galeata*, *D. quadrangula*, *D. mucronata*, *Bosmina longirostris*, *B. longispina*, *B. longicornis*, *Bythotrephes longmanus*, *Leptodora hyalina*. COPEPODA: *Cyclops coronatus*, *C. quadricornis*, *C. serrulatus*, *C. tenuicornis*, *C. brevicornis*, *C. minutus*, *Heterocope robusta*, *Diaptomus castor*, *D. gracilis*.

The author excludes from consideration those animals that enter into the pelagic fauna in an accidental and accessory way, such as fishes (especially *Coregonus*), preying on the entomostraca, and other fishes which prey on those, also infusoria living on pelagic algæ, and animals coming occasionally from the border or the bottom of a lake.

The pelagic fauna is, in its general *traits*, very much the same in all European lakes where it has been examined, from the plains to the Alps, from Scandinavia to Italy. But it is rarely represented in one lake by all animals of the fauna. Pavesi has made a very complete study, in this respect, of the Italian lakes, giving, for each, a complete list of the species found. But an observation by Weissmann has to be remembered here. He found that the different species of Cladocera presented an annual periodicity; they disappear at certain seasons (different for different species), when they are represented only by eggs. Thus the list of pelagic animals of a lake, to be complete, must be based on numerous takes in different seasons.

The common characters of the fauna accord with the mode of life, which involves constant swimming; thus the animals have no organ of fixation, but a well-developed organ of natation. Their density, nearly equal to that of water, enables them to float between two waters without exerting themselves much. Their movements are slow, and they escape enemies rather by their transparence than by agility. This transparence is, indeed, their essential character; they do not generally show a visible point, except that of their eye, which is strongly pigmented with black, brown, or red. The quality of transparence may be interpreted as a case of mimicry.

The food of the fauna is vegetable or animal. Some feed on pelagic algæ, few in species, *Anabœna circinalis*, *Pleurococcus angulosus*, *Pl. palustris*, *Tetraspora virescens*, *Palmella Ralfsii*, but very abundant in individuals; others pursue and eat the smallest animal species living in the same waters.

The pelagic animals present daily migrations; swimming near the surface at night, and remaining in the depths by day. Fric thought he found, in the Bohemian lakes, each species select a determinate depth; neither Pavesi nor the author have observed such constancy. The different species form groups, or troops, where the net makes rich captures, but these banks of animals of the same species, have not, at least in the large Swiss lakes, a determinate fixed position.

As to the maximum depth at which they are found, Prof. Forel has taken them in Lake Leman as deep as 100 and even 150 metres; at the greatest depths only *Diaptomus*.

The optic nerve of these animals probably suffers from too bright light, and so they descend whenever the light of sun or moon becomes too strong; still, they require some light to seek their prey. In their migrations they traverse a considerable thickness of water. What is the limit of light in freshwater lakes? The author showed in 1877, that the transparence varied with the season; it is much greater in winter than in summer. Under the most favourable conditions, a bright object sinking in the water of Lake Leman disappears at about 16 or 17m. depth. Paper sensitised with chloride of silver gave as light-limit in Lake Leman 45 m. in summer, and 100 m. in winter. Asper, using more sensitive plates (prepared with bromide of silver emulsion), found the *actinic* rays still active in the Lake of Zurich at 90 m. and more. All this, however, does not determine the limit of absolute obscurity for the retina, and especially for the optic nerve of lower animals

With regard to the origin of this pelagic fauna, Prof. Forel confidently rejects the idea of local differentiation of litttoral species in each lake, producing the pelagic fauna of the lake. The very remarkable character of generality, the almost absolute identity of the pelagic entomostraca in all European lakes point to dissemination and mixture.

How has this dissemination occurred? Active migration from one lake to another is inadmissible, considering obstacles and power of locomotion. On the other hand, a passive migration in the state of winter eggs, attached to the feathers of birds of passage, ducks, grebes, gulls, &c., explains the transport sufficiently. Pavesi has argued against this common origin and mode of dissemination, on account of irregularity in the pelagic population of different Italian lakes, certain species being absent in certain lakes, while they are represented in neighbouring lakes. But this irregularity seems to the author to correspond perfectly with the accidental and fortuitous character of the mode of dissemination referred to. "If this mode of transport be admitted, the differentiation of pelagic species is no longer necessarily localised in the lake in which we find the animals, any more than in the present geological epoch. This fact is very important for explanation of the pelagic fauna of certain lakes the origin of which is comparatively modern; for our Swiss lakes, the glacial epoch forms an absolute limit which prevents our supposing a local differentiation of ancient tertiary species, and their transformation into our present species; the origin of the pelagic faunas of certain Italian lakes of volcanic nature, is still more modern. But since we are no longer limited to a local differentiation of autochthonous species, we find more time and more space for this process of differentiation."

Prof. Forel believes the cause of differentiation of pelagic fauna will be found in a combination of two facts, viz., the daily migrations of entomostraca, and the regular local breezes on large lakes. There are two such breezes in calm weather, one blowing from the land at night, the other from the water by day. Crepuscular animals of the shore region, which come to swim on the surface at night, are carried out into the lake by the surface-current of the land breeze. By day the light sends them down, and thus they escape the surface current of the breeze that would bring them back to the shore. Carried each night further out, they become finally relegated to the pelagic region. Differentiation by natural selection then operates, and after a few generations, there remain only the admirably transparent animals and excellent swimmers we know. This differentiation once effected, the pelagic species is transported by the migratory birds from one country to another, from one lake to another, where it is multiplied, if the conditions are favourable. Thus we may find, even in lakes too small to possess an alternation of breezes, true pelagic Entomostraca that have been differentiated in other larger lakes by the play of such breezes. The differentiation of most pelagic species may thus be easily accounted for.

There are two species, however, the author points out, whose origin is not so explained; these are the most beautiful and interesting of pelagic Entomostraca: *Leptodora hyalina* and *Bythotrephes longimanus*. These two Cladocera have no known parentage in the freshwater species forming either the shore fauna of lakes or the marsh or river fauna. We must, with Pavesi, seek a marine origin for them. *Bythotrephes* probably descended from a common ancestor with Podon, its nearest parent, and the *Leptodora* from a primitive Daphnis.

How did the passage from salt to fresh water take place? Pavesi supposes closure of a fjord and gradual transformation of the lake water in consequence. Prof. Forel further suggests as possible, passive migration and successive transport to lagoons less and less salt; and there may have been other ways. We have not the elements for settling the question. "But the adaptation to fresh water once accomplished, the dissemination of these forms of marine origin has certainly taken place like that of other pelagic fresh-water forms, and those two species have so been transported into lakes which have never had direct communication with the sea."

There are evident analogies, Prof. Forel remarks in closing, between the lacustrian and the marine pelagic fauna; the differences appear chiefly in relative size and proportions. In the sea all is on a large scale; in lakes, on a small; the number of species and of individuals, the size, the extent of the migrations, the area of extension. But, with this exception, the general laws are the same in the two analogous faunas.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

FOUR chairs in the University College, Dundee, have been filled up as follows:—Mr. Steggall, Fielden Lecturer in Mathematics, in Owens College, Manchester, was appointed Professor of Mathematics; Mr. Carnelly, Professor of Chemistry in Firth College, Sheffield, was appointed Professor of Chemistry; Mr. Ewing, Professor of Engineering in the University of Tokio, Japan, was appointed to the Chair of Engineering; and Mr. Thomas Gilray, M.A., Head Master in English at Glasgow Academy, to the Chair of English Literature and Modern History. The salary guaranteed to each professor is 500*l*.

THE University of Zürich will, at the end of the current winter term, celebrate the fiftieth anniversary of its foundation.

SCIENTIFIC SERIALS

The Journal of Anatomy and Physiology, vol. xvii. Part 1, October, 1882, contains—On the lymphatics of the walls of the larger blood-vessels, and lymphatics, by Drs. George and Elizabeth Hoggan.—On micrococcus poisoning, by Dr. A. Ogston.—On omphalo-mesenteric remains in mammals, by Dr. W. Allen.—On the action of saline cathartics, by Dr. M. Hay.—On a hitherto undescribed fracture of the Astragalus, by Dr. F. J. Shepherd.—On a secondary astragalus in the human foot, by Prof. W. Turner.—Note on the rectus abdominalis et sternalis muscle, by Dr. G. E. Dobson.—On a case of ectopia vesicæ, &c., in a newly-born infant, by Dr. F. Ogston.—On nickel and cobalt; their physiological action on the animal organism. Part I., Toxicology, by Dr. T. P. A. Stuart.—A kerato-thyro-hyoid-muscle as a variation in human anatomy, by S. G. Shattock.—On Cesalpino and Harvey, by Prof. Humphry.

The Proceedings of the Linnean Society of New South Wales, vol. vii. part 1 (Sydney, 1882), contains: Wm. A. Haswell, on the structure of the paired fins of Ceratodus (plate 1).—Notes on the anatomy of *Ædirhinus insolitus* and *Turacæna crassirostris*.—Wm. Macleay, on Port Jackson Pleuronectidæ, with descriptions of new species; on the fishes of Palmer River; on an Alpine species of Galaxias.—E. P. Ramsay, the zoology of the Solomons, Part IV.; on a new species of Mus from Ugi Island; contributions to Australasian oology (plates 3-5); on the zoology of Lord Howes Island; on *Apogon guntheri* of Castelnau; on some Fijian bird's eggs.—Alex. Morton, notes of a cruise to the Solomons.—Prof. F. W. Hutton, note on *Fossarina petterdi*; list of New Zealand freshwater shells.—Rev. Dr. Wools, the plants of New South Wales, No. 8.—Rev. J. E. T. Woods, botanical notes on Queensland; on a new species of Stomopneustes, and a new variety of *Hipponoe variegata*; on fossil plants of Queensland.—J. Brazier, fluviatile shells of New South Wales; a list of Cypræidæ of the Victorian coast.—Wm. Mitten, on some Polynesian mosses.—Rev. C. Kalchbrenner, new Australian fungi.—Dr. J. C. Cox, on the edible oysters of Australia.

Journal and Proceedings of the Royal Society of New South Wales, vol. 15, 1882, contains: On the climate of Mackay, by H. L. Roth.—Notes of a journey on the Darling, by W. E. Abbott.—The astronomy of the Australian aborigines, by Rev. P. MacPherson.—On the spectrum of the recent comet; on new double-stars; on the transit of Mercury, November 8, 1881, by H. C. Russell.—On the inorganic constituents from epiphytic ferns, by W. A. Dixon.—A census of the genera of plants native to Australia, by Baron Ferd. von Mueller.—On water storage and canalisation for the colony, by F. B. Gipps.

Rivista Scientifico-Industriale e Journale del Naturalista, September 15.—Luni-solar influence on earthquakes, by F. L. Bombicci.—On the transformation of electricity into voltaic currents, and the application of these currents, by G. Govò.—Doderlein's ichthyological manual of the Mediterranean, by E. Riggio.

Archives des Sciences Physiques et Naturelles, September 15.—On the rotatory polarisation of quartz (third part), by MM. Soret and Sarasin.—The pelagic fauna of freshwater lakes, by F. A. Forel.—Researches on the quantity of carbonic acid contained in the atmospheric air, by E. Risler.—The air thermometer arranged with a view to a determination of high tempera tures in practice, by H. Schneebeli.—Remarks on M. Louis Lossier's work, entitled "Electrolytic Calculations," by C. E. Guillaume.—Geometric proof of the theorem of Wheatstone's bridge. by the same.—Emile Plantamour.

Bulletin de l'Academie Imperiale des Sciences de St. Petersbourg, Part xxviii., No. 2.—New researches on artificial double stars, by O. Struve.—Topographical observations of Jupiter, by J. Kalazzi.—On the oxidation of isodibutylene by hypermanganate of potash, by A. Boutlerov.—Observations of the planets Jupiter, Saturn, and Neptune in their oppositions in 1881, by A. Sowitsch.—Determination of the mass of Jupiter by means of observations of the reciprocal distances and the directions of his satellites, by O. Backland.—Action of zinc-methyl on chloral, by B. Rizza.—De Marci Antonini Commentariis, by A. Nauck.—Hydrological researches (continued), by C. Schmidt.

Zeitschrift für wissenschaftliche Zoologie, Bd. 37, Heft. 2, September 27, 1882, contains: Contributions to the anatomy of *Ankylostoma auodenale* (Dubini) = *Dochmius duodenalis* (Leuckart), by Wm. Schulthess (plates 11 and 12).—On the ontogeny of *Reniera filigrana* (O. Schmidt), by Wm. Marshall (plates 13 and 14).—Contribution to a knowledge of the structure and functions of the heart in osseous fishes, by Kasem-Beck and J. Dogiel, of Kasan (plates 15 and 16).—Contribution to a knowledge of the cestoid worms, by Dr. Z. von Roboz (plates 17 and 18).—Comparative embryological studies of Elias Metschnikoff, No. 3, on the gastrula of some Metazoa (*Echinus miliotuberculatus*, *Lineus lacteus*, *Phoronis hippocrepina*, *Polygordius flavocapitatus*, *Ascidia mentula*, and *Discoporella radiata* (plates 19 and 20).

Morphologisches Jahrbuch eine Zeitschrift für Anatomie und Entwickelungsgeschichte, bd. viii. heft 2, 1882, contains:—Contribution to the Angiology of the Amphibia, by Dr. J. E. V. Boas (with plates 6 to 8).—On the nasal cavities and the lachrymo-nasal canals in the amniotic vertebrata, by Dr. G. Born (with plates 9 and 10).—New foundations for a knowledge of cells, by Dr. A. Rauber (with plates 11-14).—Observations on the development of the crown of tentacles in Hydra, by H. Jung.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, November 2.—Sir J. Lubbock, Bart., in the chair.—Prof. J. C. Ewart, G. Fry, and Lord Walsingham were elected Fellows of the Society.—Mr. A. P. W. Thomas drew attention to a series of specimens under the microscope, and diagrams illustrative of the life history of the Liver Fluke (*Fasciola hepatica*). His experiments show that the embryos of the Fluke, as free Cercariæ, burrow into and develop within the body of *Linnæus truncatulus*, and thereafter pass with the herbage into the stomach, and ultimately liver of the sheep. Salt added to the sheep's diet is found to act as a prophylactic.—Mr. W. T. Thiselton Dyer exhibited specimens and made remarks on the plant producing *Cassia lignea*, and on the native implements used in the collection and preparation of the Cassia bark in Southern China.—Mr. C. T. Druery showed two proliferous forms of *Athyrium filix fœmina*, a family hitherto remarkable for its unproliferous nature. Both examples appeared simultaneously; not the least significant feature being their extreme precocity, since bulbil-bearing ferns are proliferous usually only on their mature fronds.—Mr. F. Crisp exhibited preparations in illustration of the views of Drs. Loew and Bokorny on the difference between dead and living protoplasm, viz. the power of the living organism to reduce silver salts in a very dilute alkaline solution.—Prof. E. Ray Lankester exhibited and made remarks on a series of marine organisms dredged by him, last summer, in the fjords of Norway. Of these may be mentioned a branch of *Paragorgia arborea*, three feet across, specimens of the same in spirit, as also of *Lophelia prolifera*, *Amphiheria ramea*, *Stylaster norvegicus*, *Primnoa lepadifera*, and *Paramuricia ramosa*, both dried, and also with the polyps preserved in spirit. The collection also included some very large new forms of Foraminifera specimens of *Rhizocrinus Lofotensis*, of the aberrant mollusca *Neomenia* and *Chætoderma*, and of *Rhabdopleura Normani*, besides a large series of sponges and Asteroidea.—Mr. T. Christy exhibited a living specimen of the Japanese peppermint plant, which yields the Menthol of commerce, this being the first plant grown in this country. Mr. Holmes mentioned that although this mint did not differ in botanical characters from *Mentha arvensis*, it had a strong peppermint odour and flavour, which were not found in the specimens growing either in Europe or India. He therefore proposed that the plant should be named *M. arvensis*, var. *piperaneus* by way of distinction.—Mr. J. G. Baker showed a specimen of *Lycopodium complanatum* collected in Skye by Prof. Lawson.—Sir J. Lubbock then read his tenth communication on the habits of ants, bees, and wasps, a notice of which appeared in our last issue, p. 46.—A paper was read on medicinal plants of North-West Queensland, by W. E. Armit. Among these is a species of *Aristolochia* and a Croton; also *Grewia polygama*, a specific for dysentery; *Careya arborescens*, used for poultices; *Erythreæ australis*, and *Andropogon citriodora*, tonics in febrile complaints; and *Euphorbia pilulifera* and *Datura australis*, valuable in cases of asthma.—A remarkable malformation of the leaves of *Beyeria opaca*, var. *linearis*, from Yorkes Peninsula, South Australia, was described by Mr. Otto Tepper.—Dr. F. Day exhibited specimens in illustration of a paper read by him, on variation in form and hybridism in *Salmo fontanalis*.—Mr. H. N. Ridley afterwards read some teratological notes on a Carex, a Grass, and an Equisetum

Zoological Society, November 14—Prof. W. H. Flower, F.R.S., president, in the chair.—A letter was read from Mr. E. L. Layard respecting a specimen of *Schœnicola platyura* received by the British Museum from the late Mr. Cuming.—Prof. F. Jeffrey Bell exhibited some examples of *Lymnæus truncatulus*, lately discovered to be the chief host of the larvæ of the sheep-fluke.—Prof. Flower exhibited and made remarks upon the skull of a young chimpanzee from Lado, in the Soudan, sent to him by Dr. Emin Bey, which exhibited the deformity called "Acrocephaly," associated with the premature closure of the fronto-parietal suture.—Mr. H. E. Dresser exhibited and made remarks on specimens of *Melittophagus boehmi*, Reichenow, and *Merops dresseri*, Shelley, which he showed to be identical.—A communication was read from Mr. W. A. Forbes containing some supplementary notes on the anatomy of the Chinese Water Deer (*Hydropites inermis*).—A communication was read from the Rev. L. Baron, containing notes on the habits of the Aye-Aye of Madagascar in its native state.—Mr. G. E. Dobson read a paper on the natural position of the family Dipodidæ, which he maintained to be with Hystricine, and not, as generally supposed, with the Murine Rodents, and to be most nearly allied to the Chinchillidæ.—Prof. F. Jeffrey Bell read a paper on the genus *Psolus*, relating its literary history, and giving an enumeration of the described species. Attention was directed to the extensive distribution of *P. fabricii*, and to the variations during growth. After the description of other known forms, two new species (*P. peronii* and *P. ambulata*) were described; for the latter a new sub-genus was suggested, and the genus itself was divided into three sub-generic groups.—A second paper from Prof Bell contained an account of a Crinoid from the Straits of Magellan, obtained by Dr. Coppinger during the voyage of H.M.S. *Alert*, which was referred to a new variety of *Antedon eschrichti* of the Arctic seas.—Mr. W. H. Neale read some notes on the natural history of Franz-Josef Land, as observed in 1881–82, during the stay of the *Eira* expedition in that land.—Dr. Gwyn Jeffreys read the fifth part of his list of the Mollusca procured during the expeditions of H.M.S. *Lightning* and

Porcupine. This part, which embraced the species from the Solenoconchia to the Calyptræidæ, comprised sixty-nine species, of which twenty two were now for the first time described or figured. The geographical, hydrographical, and geological range of all these species was given, as in his former papers; and the author especially noticed the points of agreement between the deep-water Mollusca from the American and European expeditions.

Physical Society, November 11.—Prof. Clifton, president, in the chair.—Prof. Rowland, of Baltimore, exhibited a number of his new concave gratings for giving a diffraction spectrum. He explained the theory of their action. Gratings can be ruled on any surface if the lines are at a proper distance apart and of the proper form. The best surface, however, is a cylindrical or spherical one. The gratings are solid slabs of polished speculum metal ruled with lines equidistant by a special machine of Prof. Rowland's invention. An account of this machine will be published shortly. The number of lines per inch varied in the specimens shown from 5000 to 42,000, but higher numbers can be engraved by the cutting diamond. One great advantage of their use is that the relative wave-lengths can be measured by the micrometer with great accuracy. The author has designed an ingenious mechanical arrangement for keeping the photographic plates in focus. In this way photographs of great distinctness can be obtained. Prof. Rowland exhibited some 10 inches long, which showed the E-line doubled, and the large B groups very clearly. Lines are divided by this method which have never been divided before; and the work of photographing takes a mere fraction of the time formerly required. A photographic plate sensitive throughout its length is got by means of a mixture of eocene, iodised collodion, and bromised collodes. Prof. Rowland and Capt. Abney, R.E., are at present engaged in preparing a new map of the whole spectrum with a focus of 18 feet. In reply to Mr. Hilgar, F.R.A.S., he stated that if the metal is the true speculum metal used by Lord Rosse, it would stand the effects of climate he thought; but if too much copper were put in it might not. In reply to Mr. Warren de la Rue he said that 42,000 was the largest number of lines he had yet required to engrave on the metal.—Prof. Guthrie read a letter from Capt. Abney, pointing out Prof. Rowland's plates gave clearer spectra than any others; they were free from "ghosts" caused by periodicity in the ruling; and the speculum metal had no particular absorption.—Prof. Dewar, F.R.S., observed that Professor Liveing and he had been engaged for three years past in preparing a map of the ultra-violet spectrum, which would soon be published. He considered the concave gratings to make a new departure in the subject, and they would have greatly facilitated the preparation of his map.—Mr. W. R. Browne then read a paper on the conservation of energy and central forces. He showed that the doctrine of the conservation of energy necessarily involved central forces and could not be proved unless on the assumption of a system of central forces. This involved the hypothesis of Boscovich that matter consists of a collection of centres of force, and the author criticised the objections of Clerk Maxwell, Tait, and others to Boscovich's theory. The paper will appear in the *Transactions* of the Society.—Prof. S. P. Thompson read some historical notes on physics, in which he showed that the voltaic arc between carbon points was produced by a Mr. Etienne Gaspar Robertson (whose name indicates a Scotch origin) at Paris in 1802. This reference is found in the *Journal de Paris* for that year. Laboratory note-books at the Royal Institution, however, are said to show that Davy experimented with the arc quite as early. The experiment usually attributed to Franklin of exhausting air from a vessel of water "off the boil" and causing it to boil afresh, is found in Boyle's "New Experiments touching the Spring of the air. Prof. Thompson also exhibited an early Reis's telephone, made by Philip Reis in 1861 at Frankfort, and designed to transmit speech. It was modelled on the human ear, one form of transmitter being a rudely-carved wooden ear, with a tympan, having a platinum wire behind, hard pressed against a platinum-tipped adjustible spring. Prof. Thompson showed by various proofs that words were actually sent by that and similar apparatus.

Meteorological Society, November 15.—Mr. J. K. Laughton, F.R.A.S., president, in the chair.—Eleven new Fellows were elected, viz. Rev. J. Brunskill, F. B. Buckland, C. F. Casella, W. H. M. Christie, F.R.S., A. Cresswell, R. S. Culley, C. Morris, O. L. O'Connor, H. Parker, F.Z.S., A. Rowntree, and D. R. Sharpe.—The papers read were: On certain types of British weather, by the Hon. Ralph Abercromby, F.M.S. The author shows that there is a tendency of the weather all over the Temperate Zone to occur in spells associated with certain types of pressure distribution. In Great Britain there are at least four persistent types—the southerly, the westerly, the northerly, and the easterly. In spite of much fluctuation, one or other of these types will often continue for weeks together, and tend to recur at the same date every year. The value of the recognition of type groups is shown in the following ways:—(1) They explain many phenomena of weather and many popular prognostics; (2) in some cases they enable forecasts to be issued with greater certainty and for a longer time ahead; (3) we can by their means correct statistical results by giving the real test of identity of recurrent weather which no single item such as heat, cold, rain, &c., can do; (4) they enable us to treat such geological questions as the influence of changing distribution of land and sea on climate in a more satisfactory manner than any other method.—On the use of kites for meteorological observation, by Prof. E. Douglas Archibald, M.A., F.R.S. In this paper the author advocates the use of kites for meteorological observation, and describes the mode in which they may be best flown so as not to be mere toys, but scientific instruments, capable of ascending to great heights, remaining steady in currents of varying velocity, and of being manipulated with ease and rapidity by the observer.—The meteorology of Mozufferpore, Tirhoot, 1881, by Charles N. Pearson, F.M.S.

Institution of Civil Engineers, November 14.—The president, Sir W. G. Armstrong, C.B., F.R.S., in the chair.—The paper read was on "Recent Hydraulic Experiments," by Major Allan Cunningham, R.E., Honorary Fellow of King's College, London.

BERLIN

Physical Society, October 3.—Prof. Rœber in the chair.—Dr. Kœnig had already reported in a previous session on the Leukoscope, designed and constructed by Prof. Helmholtz, and communicated now the results of his further experiments with this instrument. It consists essentially of a calc-spar-rhomboid, a plate of quartz, and a Nicol's prism. A luminous pencil entering the calc-spar is split up into two rays polarised at right angles which traverse the quartz-plate and the Nicol. When spectroscopically analysed, these rays show two spectra of absorption-bands, in the spectrum of the one pencil at points where in the spectrum of the other pencil the intensity is undiminished, and *vice versâ*, so that the two spectra superposed would give a continuous spectrum. The number of bands increases with the thickness of the quartz, and they are shifted by rotating the Nicol. The *modus operandi* then is to put in a quartz plate of such a thickness, and to rotate the Nicol so much that in each of the spectra the colours that are not blotted produce together white light. When different sources of light are examined with the leukoscope, the different amounts of rotation of the Nicol are required for effecting a conformity of the two images, the relative quantity of certain rays being different in every different light, the prevailing tint belonging therefore to the one, and not to the other spectrum. Further experiments having proved that the plate of quartz could remain unaltered, the rotation angles of the Nicol were a gauge of the quality of colours of the light examined. Dr. Kœnig has tried in this way a series of sources of light, and found the angles wanted for homogeneity of the white images to be as follows:—With stearin candles = 71°·20; with gaslight = 71°·5; with electric arc light = 79°; with magnesium light = 86°; with solar light = 90°·5; with burning phosphorus and Drummond lime-light the angles were between gas and electric-arc light. The succession of the sources of light thus stated coincides strikingly with the results of spectro-photometric measurements of Prof. Pickering. The fact that the magnesium light is more like the solar light than the electric arc light quite corresponds with the known fact that of the aniline dyes, scarcely distinguishable by gas-light, the greatest part can be perceived by electric light, but not all, viz. the so-called bronze hues; whereas by the magnesium light they are all as well distinguished as by solar light. Furthermore Dr. Kœnig has made many measurements with the leukoscope on different electric incandescent lights; with Swan's lamp and Edison's lamp he gave the results of his experiments in tables in which the strength of the current, the intensity of light, and the angles of the leukoscope were indicated. From these numbers it follows that luminosity augments at first at a much greater rate with increasing strength of current than the latter; by doubling the strength of the current the illuminating power was increased about sixty-fold. The angles of the leukoscope became likewise greater with the rising intensity of the light in such a manner that a curve traced with the light intensities as abscissæ and the angles as ordinates is concave to the abscissæ and approaches assymptotically a maximum near 78°, an angle approximately equal to the angle (79°) of the glowing carbons of the electric arc light. The measurements with a Siemens' incandescent lamp gave also numbers which could be represented by a similar curve.

Paris

Academy of Sciences, November 13.—M. Jamin in the chair.—The following papers were read:—Results of experiments made with electric candles at the Exhibition of Electricity, by M. Allard and others. The systems examined, those of Jablochkoff, Jamin, and Debrun, now produce nearly the same economical results.—On the reproduction of osmides of iridium, by M. Debray. Osmium and iridium may crystallise together in all proportions, without the form of their combination being altered. They are then isomorphous. And natural osmides may be true isomorphous mixtures, belonging to the cubic system, notwithstanding the hexagonal appearance of certain varieties (but this view is given with reserve, natural osmides being of complex composition).—Report to the Bureau des Longitudes on the approaching eclipse of May 6, 1883, by M. Janssen.—Note on the telluric lines and the spectrum of aqueous vapour, by M. Janssen. He recalls his own method, based on study of the spectrum of water vapour in a tube; he is now working at it. The *telluric lines* (so called by him) are historically quite different from the *bands of Brewster*; (an expression of M. Cornu's seemed to affirm their equivalence.)—On the currents produced by nitrates in igneous fusion, in contact with red hot carbon, by M. Brard. Owing to the tendency of fused nitrates to spread in heated bodies, a current may be had if a short carbon rod with one end put in the nitrate has only the other end incandescent; also if fused nitrate in a metal capsule is placed on burning carbon (the nitrate soaks through, so that the outer surface of the capsule becomes quite moist); indeed, such a capsule merely hung over a centre of combustion gives a current (from the nitrate bath to the outer surface of the capsule). The effect is improved by putting plumbago, or lampblack, on the outside, and covering all with metallic foil. Nitrates kept in fusion have great fixity.—Chemical studies on the white beet of Silesia (continued), by M. Leplay.—Observations made during the total eclipse of the sun of May 17, 1882, by M. Tacchini. He gives a *résumé* of his memoir, which will shortly appear.—On Abelian differential equations in the case of reduction of the number of periods, by M. Picard.—On a theorem of M. Tisserand, by M. Stieltjes.—Extension of the problem of Riemann to hypergeometric functions of two variables, by M. Goursat.—On the development of functions in series of other functions, by M. Hugoniot.—On the exactness of measurements made with the mercury thermometer, by M. Crafts. The pressure (of the air) has little influence. Permanent elevation of the fixed points, produced at a high temperature, preserves the thermometer against the influence of heat, in this respect, at lower temperatures.—Conclusions of hydrodynamic experiments in imitation of phenomena of electricity and magnetism; reply to a note of M. Ledieu, by M. Decharme. The theory of waves seems to him to be "the secret of nature," and he places the results of his experiments against the unverified ideas of M. Ledieu.—Electric deformations of quartz, by MM. Jacques and Curie. With delicate apparatus, they observed and measured such deformation when a charge was given to two pieces of tin at the opposite ends of the electric axis. The dilatations measured agreed satisfactorily with those calculated from the electricity liberated.—On the electrification of air, by M. Mascart. In the Amphitheatre of the College of France he electrified the air by discharging a Leyden jar with a flame, during ten seconds; another flame, 8 m. off, communicated with an electrometer in an adjoining hall. The maximum deflection in the latter was reached in about a quarter of an hour; then there was slow diminution, but after two hours 1-20th of the maximum still remained. The electrified gas probably rises and is diffused like smoke. To study the lower atmospheric layers, the potential should be determined in a large inclosure formed of metallic netting, connected with the ground.—On atmospheric nitrification, by MM. Muntz and Aubin. A constant absence of nitrates in meteoric waters was observed at the top of the Pic du Midi (nearly 3000 m.). A comparison of thunderstorms shows the summit to be generally above them. Atmospheric nitrification is probably produced in the lower regions of the air.—On the decomposition of phosphates at a high temperature by sulphate of potash, by M. Grandeau.—Point of solidification of various mixtures of naphthaline and stearic acid, by M. Courtonne.—On œnocyanine, by M. Maumené.—On the cause of liberation of oxygen from oxygenated water by fibrine; influence of hydrocyanic acid exhausting the activity of fibrine, by M. Béchamp. He shows that the fibrine loses somewhat; and that it no longer decomposes oxygenated water nor fluidifies starch, nor gives bacteria.—On the signification of the polar cells of insects, by M. Balbiani.—On the vaso-dilator reflex of the ear, by MM. Dastre and Morat.—Phenomena of death from cold in Mammalia, by MM. Richet and Rondeau. The respiratory and cardiac functions may be suspended for half an hour, without death certainly ensuing. (Rabbits, shaved (*rasés*), were inclosed in flexible tin tubes, through which flowed salt water cooled to − 7° C.).—Analogies and differences between curare and strychnine, as regards their physiological action, by M. Couty.—On the causes of migration of sardines, by M. Launette. Each migration is normally under the two conditions of food and temperature combined.—Contribution to the geological history of the iron of Pallas, by M. Mennier.

Vienna

Imperial Academy of Sciences, October 12.—V. Oppolzer, finding the reduction to infinitesimal arcs of vibration at pendulum-observations.—H. Kreuter, computation of the trajectory of the comet 1771.—V. Barth and T. Schreder, on the behaviour of benzoic acid if dissolved in caustic potash.—N. Herz, the theory of computations of the trajectory of a comet.

October 19.—R. v. Lendenfeld, on a new self-registrating thermometer.—V. Oppolzer, on an eclipse of the sun mentioned by Archilochos.—W. Demel, on the Dopyrlerite of Ausser (Styria).—W. Gintl and F. Reinitzer, on the constituents of the leaves of *Fraxinus excelsior*, L.

CONTENTS

Page
The Challenger Reports 73
Light 75
Our Book Shelf:—
Brown's "Practical Chemistry" 75
Lupton's "Elementary Chemical Arithmetic" 76
Britten's "Watch and Clockmaker's Handbook"—H. Dent Gardner 76
Duncan's "Heroes of Science" 76
Letters to the Editor:—
Physics of the Earth's Crust.—Rev. O. Fisher 76
Shadows after Sunset.—E. Douglas Archibald 77
An Abnormal Fruit of Opuntia Ficus-Indica.—Dr. A. Ernst (*With Illustration*) 77
The Comet.—B. J. Hopkins 78
Soda Flames in Coal Fires.—Major J. Herschel 78
Complementary Colours—A Mock Sunset.—I. H. 78
A Lunar Halo—J. Rand Capron 78
A Correction.—E. L. Layard 78
Thomson's Mouse-Mill Dynamo.—J. T. Bottomley 78
"Weather Forecasts"—Charles W. Harding 79
Age of Dogs.—R. Cordiner 79
Waterspouts on Land.—James Hosack 79
Meteorology of the Malay Archipelago 79
The Comet. By J. K. Rees; Prof. S. C. Chandler, Jun. (*With Diagram*) 80
Influence of "Environment" upon Plants. By W. T. Thiselton Dyer, C.M.G., F.R.S. 82
The Magnetic Storm and Aurora. By W. H. M. Christie, F.R.S.; Prof. C. Piazzi Smyth, Astronomer Royal for Scotland; G. M. Whipple; J. Rand Capron, R. H. Tiddeman; J. Edmund Clark; H. Clifford Gill; Henry Robinson; A. M. Worthington; Thos. Gwyn Elger; Miss C. Rose Ingleby; C. H. Romanes; E. Brown; Frank Stapleton; Rev. Stephen H. Saxby; Dr. Hubert Airy; Major J. Herschel; A. S. P.; H. D. Taylor; T. P. Barkas; W. Makeig Jones; George Rayleigh Vicars; Prof. J. P. O'Reilly; John L. Dobson 82
The Chlorophyll Corpuscles of Hydra. By Prof. E. Ray Lankester, F.R.S. 87
Notes 88
Biological Notes:—
Apparent Bird-tracks by the Sea-Shore 91
Australian Freshwater Sponges 91
Earth-worms in New Zealand 91
The Genesis of the Hypophysis in Petromyzon Planeri 91
Formic and Acetic Acid in Plants 91
Geographical Notes 92
The Pelagic Fauna of Freshwater Lakes 92
University and Educational Intelligence 93
Scientific Serials 93
Societies and Academies 94

THURSDAY, NOVEMBER 30, 1882

THE INDIAN SURVEY.

General Report on the Operations of the Survey of India during 1880-81. 61 pp. Report, 93 pp. Appendix, and 22 Plates. (Calcutta, 1882.)

THIS Report for 1880-81 (the fourth since the various branches of the Indian Survey were amalgamated) shows as usual a good amount of useful work done in the year, and contains also many points of general interest. There were in all twenty-nine field-parties and six large head-quarters' offices. The whole out-turn of work cannot be shortly stated, and the total cost is not given; but it appears that there were 22,765 square miles surveyed topographically, 6141 square miles in great detail, besides much minor and special work, also that eleven Revenue Branch parties surveyed 11,326 square miles at a cost of about 81,000*l*.

The principal triangulation of India proper as designed by Col. Everest, *has now been finished.* The result is shown on a skeleton map, which is itself a wonderful sight. There is a continuous "chain" of triangles right round India proper, connected across by *many* meridional and east to west "chains," the longest being from Mussoorie to Cape Comorin (say 1600 miles north to south), and from Chittagong to Kurrachee (say 1800 miles east to west). Outside India proper there are five important extensions, viz. (1) to Kandahar and Khelat; (2) over Káshmír; (3) up the Indus beyond Leh; (4) up the Brahmaputra to Sudya; and (5) a coast "chain" from Chittagong to Tenasserim. This great work, now finished, is one of which India may well be proud.

Certain important changes of procedure are being introduced in the general survey work, viz.: (1) All the topographical work is being brought to a uniform system; (2) Fieldbooks are gradually giving way to direct plotting of detail in the field with advantage in speed of work and economy; (3) Special riverain surveys will in future be made; (4) Local agency is being tried for detail work; this last measure is expected to effect great economy in the survey of Burma, for which at present a staff of 2500 men is taken from Calcutta and back again each season.

Great difficulties often beset the parties in the wilder parts of the country. Many parts are extremely unhealthy, and the parties often suffer severely from fever, &c. In a few parts the roughness of the country, in others climatic conditions, render travelling exceptionally difficult, *e.g.* travelling in the hot wind across the "Rann" (great salt desert) of Katch is dangerous to both man and beast. In a few parts even in India proper, *e.g.* among the wild Bhíls, the surveyors are looked on with suspicion and sometimes attacked. The greatest praise is due to men who carry out their field-work through such difficulties as these.

In general a survey party now accompanies every military expedition; thus some extension of geographical and trigonometrical work was done in 1881 by parties sent with the Mahsúd Wazírí expedition and with the troops at Kandahár. A curious difficulty has arisen in that the modern use of the heliostat in military signalling almost precludes its use for survey stations along with an army.

Self-registering tide-gauges with 5 feet barrels have been set up at fourteen places, and have worked well as a whole. Tide registrations (mostly from older instruments) for twenty-three years in all have been analysed by the harmonic analysis at enormous labour. The discussion shows (for the first time) the existence of a "lunar fortnightly tide" as had been expected from the tidal theory. Tide-tables for 1882 were published for fifteen ports.

The tidal stations of Madras and Bombay have been connected by "levelling" right across the peninsula with the curious result that the mean sea-level at Madras *appears* to be 3 feet above that of Bombay. The cause of this is still a subject of inquiry. It is really a very curious question. Thus, it is said that "there can be no sensible differences of level," *i.e.* as determined by *levelling*, "because the causes by which they would be produced must equally affect the spirit-levels of the instruments and the water-levels of the ocean," so that had the "levels been carried, without error, along the coast line from Bombay . . . to Madras, they must have shown identity of sea-level, &c." On the other hand it is also said that "the Western Gháts are a source of attraction, which, if not counteracted, must raise the sea-level at Bombay no less than 31 feet above the mean sea-level at Madras." The difference (which should be zero?) is attributed to *observation-error*, and chiefly to the effect of the oblique sunlight illuminating the two ends of the instrument-bubble unequally: thus it is said that an error of only 1·2 seconds in levelment (a very minute quantity) at even one-fourth of the instrumental stations would produce the total error in question.

An interesting improvement has been introduced in the engraving branch, viz. in steel-facing the copper-plates, and is said to be very successful. Apparently engraving on copper is still largely used (as also in the British Ordnance Survey), but it would seem that this tedious and costly process must give way to some of the rapid and cheap photographic processes. The Indian Survey is also utilising the latter very largely, with the wonderful result that "at present publication may, and frequently does, follow the survey in a few days." There is a curious instance of the possible saving in departmental manufacture, in that about 334*l.* has been saved by making up collodion in the office instead of purchasing it.

Of underground temperatures it is noted that at Dehra the maxima occurred at the three depths, 6·4, 12·8, 25·6 feet, about September 20, October 15, and November 15 respectively (the maximum in the air being probably in June?).

An extraordinary outburst of solar spots, covering 630 million square miles, was observed to take place on July 25, 1881, within a period of thirty-seven minutes; it is rare that so grand an outburst is so closely located in time.

The Indian Survey was well represented at the Venice Geographical Exhibition. The whole collection sent seems to have excited great interest, especially the tidal instruments which were connected with the Main Canal so as to be shown in actual work. This exhibition brought to light a striking difference in recent practice of construction of instruments in England and on the Continent in that recent improvements in graduating circles are so great as to lead to the general adoption (by continental makers) of small circles with powerful reading micrometers in place of large circles with verniers.

A very simple process of making relief maps in Germany is described, viz. by cutting out contour strips from a contoured map, and pasting each on to cardboard cut to same outline. Altogether the Report is a very interesting one.

ALLAN CUNNINGHAM

GREEN'S "GEOLOGY"

Geology. By A. H. Green. Part I. Physical Geology. Third and Enlarged Edition. (London: Rivingtons, 1882.)

STUDENTS of Geology will welcome this third and much enlarged edition of Prof. Green's excellent text-book, though they may at first sight regret the exchange of the old convenient manual form of the book for that of the present handsome and well-printed octavo. One of the first features that strikes the reader in this new issue of the work is the large augmentations made to the lithological sections. In fact this part of the treatise may be said to have been re-cast and almost wholly re-written. The author devotes 150 closely printed pages to crystallography and the description of minerals. It may be open to question whether the full details which he gives to the crystallographic characters of minerals are not rather out of place in a geological treatise. They are not ample enough for the mineralogical student, and the geologist who takes up the subject must necessarily study text-books of mineralogy, where they are given at much greater length. Prof. Green, however, has put them so clearly and succinctly that this portion of his book cannot fail to be of use.

Some changes have been made in the arrangement of rocks. The non-crystalline or derivative rocks now come first—a grouping which no doubt has its advantages in teaching, particularly in elementary classes, but which is not that usually employed in petrographical works After briefly describing the lithological character of the non-crystalline rocks, the author, following his original plan, proceeds to discuss the mode of formation of these rocks, dealing first with denuding agents and their work, and then considering the manner in which the denuded material is aggregated into rock-masses. In these sections he brings his subject abreast of the onward march of the science. Another change in the original treatment of his subject occurs in the author's chapter on the "confusedly crystalline rocks." He has not been so happy in his choice of a title for them as he has been in his description of their general characters. After giving an account of the lithological features he proceeds to discuss their modes of origin, dealing first with volcanoes recent and extinct, then with earthquakes (though one wonders what these have to do with a description of crystalline rocks), next with plutonic rocks which, however, are rather inadequately discussed. The chapter on metamorphic rocks has been carefully revised, and may be commended to the student as an admirable summary of what is at present known on this subject. The chapter upon the way in which rocks came into their present positions was one of the best in the first edition of the book. Its excellence has now been increased by a thorough revision. For practical insight into the structure of the earth's crust it is unsurpassed in any treatise known to us.

Prof. Green more than makes up for the curious omission in the first edition of any mention of mineral veins. We doubt, however, the advantage of inserting minute descriptions of metallic ores in a general geological text-book. The author would do well in his next edition to give references to the Continental works on mining, particularly to some of the numerous treatises which have been published in Germany. Chapter XIII. retains its place as a valuable account of how the present surface of the ground has been produced. The last two chapters discuss the former fluidity and present condition of the earth's interior, the cause of upheaval, contortion, and metamorphism, and the origin of the changes of climate which have taken place during geological time. These parts of the book are exceedingly well done. The author has held the balance fairly between contending disputants, and sums up the evidence with conspicuous and judicial impartiality. Altogether, he may be congratulated on the appearance of this edition of his text-book, which sustains and extends his reputation as an exponent of his favourite science.

OUR BOOK SHELF

A History of British Birds. By the late William Yarrell. Fourth Edition, Revised to the end of the Wryneck, by Alfred Newton, M.A., F.R.S., continued by Howard Saunders, F.L.S., F.Z.S. Part XV. (London: John Van Voorst, November, 1882.)

THE fifteenth part of the new edition of what the British ornithologist fondly calls his "Yarrell" contains the final contribution of Prof. Newton to this work, and the first pages of the portion which Mr. Howard Saunders, his successor in the editorship, has undertaken. Few of the subscribers, we believe, will be much pleased with the change of authorship of their favourite work of reference. No living writer, it may be confidently asserted, is so competent to prepare a new edition of "Yarrell's British Birds" as Prof. Newton, and the conscientious care with which he has laboured upon the two volumes now completed must be patent to all who consult them. At the same time it should not be forgotten that time is an element in all human matters not even excepting books on British birds. When, therefore it is considered that nearly eleven years have elapsed since Prof. Newton commenced his new edition, and that only the first half of the work is now completed, it is obvious that the Professor has not acted unwisely in surrendering the second half to an editor who is able to devote more time to the undertaking.

Mr. Howard Saunders, it is generally understood, intends to issue the two final volumes of the new edition in two years, and if his health and strength permit, will doubtless accomplish his task within the allotted period. In this his large practical knowledge of the bird-life of Southern Europe, as well as his well-known familiarity with modern ornithological literature, are likely to be of the greatest assistance.

Mr. Saunders commences his second volume with the pigeons, and gives us an excellent account of the four British species, as also of the American passenger pigeon, which can only be looked upon as one of our rarest stragglers from the New World. When, however, he says that all true pigeons lay two eggs he must have forgotten that the crowned pigeons, and the numerous forms of fruit-pigeons are, so far as is known, content to lay but one. There is therefore no good reason for calling the

Columbæ "Bipositores," as one of our systematists has proposed to do! After the pigeons Mr. Saunders places the sand-grouse as an intermediate order between the Columbæ and Gallinæ. This is certainly a better plan than that adopted by some of the more ardent reformers of the ornithic system—of uniting the sand-grouse in the same group with the pigeons, and thus spoiling the symmetry of the order Columbæ. In this and in other particulars the new Editor of "Yarrell's Birds" shows a judicious spirit, which cannot fail to make the results of his labours generally acceptable.

Episodes in the Life of an Indian Chaplain. By a Retired Chaplain. (London: Sampson Low, Marston, Searle, and Rivington, 1882.)

THIS interesting narrative of the adventures and vicissitudes of a devoted and single-minded Indian Chaplain, appears to be addressed to two classes of readers. A considerable portion must be considered more or less theological, and hence not applicable to the columns of NATURE; but running throughout the unambitious work is a considerable residue of facts and observations relating to zoology, which are never tiresome and sometimes original. In the days of his boyhood our author's leisure time was given to his "different collections of natural history and antiquities," and after many years' official duties he seems to have once more resumed his early tastes, on his appointment to the curatorship of the museum and secretaryship of the public gardens belonging to the Maharajah of Travancore. It is whilst employing his leisure in this vocation that the reader experiences more of the naturalist and less of the chaplain, but both phases are so kindly and modestly described, as to disarm criticism and at the same time promote an amiable impression of the writer.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Sir George Airy on the Forth Bridge

SIR GEORGE AIRY'S letter (*vide* NATURE, vol. xxvi. p. 598) criticising Messrs. Fowler and Baker's design for the Forth Bridge is so important, that I think it but right, as I am not without experience on the subject, to make some remarks on the subject of it. Sir George Airy states:—

1. "That the proposed construction is, as applied to railway bridges, entirely novel." This is not quite exact. There are a number of cantilever bridges in America; and I have, myself, used practically similar principles of construction and erection, on a large scale, with entire success, and find them so satisfactory that, for a very long span, I would not think of using any other.

2. "The magnitude of its parts is enormous." Undoubtedly they are—and all the more credit to the men who had the nerve to design them.

3. "There has been no succession of instances of the construction with rising degrees of magnitude which might furnish experimental knowledge of some of the risks of construction." If this reason were sound, the same objection would have prevented the construction of the Conway, Britannia, and Saltash bridges, and *Great Eastern* steamer; but so far from the statement being correct, the engineering profession has gained ample experience in the erection of the St. Louis, Kentucky River, Douro and Minnehaha bridges to give assurance that the Forth Bridge can be made a perfect success.

4. "The safety of the bridge depends entirely on a system of end thrusts upon very long rods." This is a very singular statement. What would become of the safety of the bridge in case there was no answering and complementary tension system equally exposed to danger from a "system of end pulls upon very long rods" does not appear from Sir George's letter; nor does he seem to remember that the tests of the last few years show conclusively, that iron exposed to compression within its buckling limit is compacted in texture and strengthened by such use while, if subjected to continuous tension beyond two-thirds of its elastic limit, it is attenuated and weakened.

5. "No reference is made to theory applied to the buckling of rods under end thrusts." None was necessary. Mr. Baker has designed struts, or columns—not rods. These members in the Forth Bridge are presumed to have such a proportion of diameter to length that the question of buckling does not come into consideration. In America, columns of many shapes—in full-sized sections—have been tested in lengths of from 10 to 70 diameters, and the value of these shapes, in pounds of resistance per square inch of section for each $\frac{\text{length}}{\text{diameter}}$ is definitely known.

These results are now the common property of all English-speaking engineers. Sir George Airy's remarks on long struts are the more extraordinary, as there is in England, in the upper chord of the Saltash Bridge, an example of a long strut without lateral support which, is greater in its ratio of length to diameter than any member that I know of in the Forth design. Moreover, it is 455 feet long, near enough to the length of St. Paul's Cathedral for him to contemplate in connection with that edifice, in presenting a picture to the people of London.

6. "The liability to ruinous disturbance by the lateral power of the wind acting with the leverage of the long brackets appears to be alarmingly great." This liability to destruction by wind is common to all large spans; but the danger is greater in the case of a suspension bridge than in any other (I speak with some knowledge on this point, having made the effects of tornadoes a special study for a number of years past, and having visited most of the bridge wrecks which have occurred in the States, from this cause, since 1858). So far as destruction by wind can be guarded against in the Forth design, it has apparently been done; and the bridge will be vastly stronger in this regard than many other bridges in England which can be easily named, and about the strength of which there is supposed to be no question.

To conclude:—The opinion of those American engineers with whom I have conversed on the subject, and whose experience in building long-span bridges makes that opinion valuable, is uniformly to the effect that the design of Messrs. Fowler and Baker is well digested, perfectly practicable as to execution, and thoroughly permanent in character when finished.

I may also add that three years since, when called on to design a railway bridge for the crossing of the Great Colorado cañon, which was to be 900 feet span and 750 feet above the river, I investigated the relative merits and cost of the various systems—arch-suspension and cantilever with mid-span. Working drawings were made of each, and the result was, that the cantilever was adopted as being equally strong and stable—less liable to be affected by wind and thermal changes, and decidedly more economical in first cost and easier of erection than either of the others. I am, therefore, not surprised that the engineers of the Forth Bridge should have reached the same conclusion.

CHARLES SHALER SMITH

St. Louis, Mo., November 11

The Aurora

I HAD not the good fortune to see the very unusual phenomena which took place during the aurora of Nov. 17. It was, however, well seen by four of the students of this College, Messrs. Sykes, Wildeblood, Thornhill and Wackrill. Although you are doubtless inundated with letters on the subject, I send a short account of the observation, as such an opportunity of determining the height of an auroral light very rarely occurs. The commencement of the movement of the "Whitehead-torpedo-shaped" streak of light does not appear to have been noticed by them; it passed however just below the moon, one observer thinks that its upper edge just grazed the lower edge of the moon. The light when close to the horizon bore due south-west, a position which has since been verified by bearings taken by a prismatic compass. The spot where the observers stood is, by the new ordnance map in lat. 51° 25′ 57″ N., and long. 0° 34′ 5″ W.

HERBERT MCLEOD

Royal Indian Engineering College, Cooper's Hill, Nov. 24

AT Ilford, Essex, on the 17th instant, at 6h. 4m. p.m. by a watch which was within 2m. of G. M. T., I witnessed, during the auroral display, the extremely singular phenomenon which has been described by several of your correspondents. It looked exactly like a white cloud, about 20° long and 2° wide, tapered somewhat from the middle to each end; but it was more luminous than a cloud could well have been at that time. When first seen, its nearest end may have been 30° east of the moon. Its length was nearly parallel to the horizon, and continued so till lost sight of about as much to the west of the moon; and its passage over an area of some 80° occupied probably less than a minute. It passed very near to the moon, but I cannot say whether over it or not. CHARLES J. TAYLOR

Toppesfield Rectory, Halstead, Essex, Nov. 25

FOLLOWING up my last week's letter concerning the electric meteoroid, if one may so term it, of the 17th inst., I have sifted all the testimony within my knowledge, assigning a numerical weight to each report from internal evidence of its probable value, and correcting for latitude where the altitude of the moon was made the standard of comparison. With data so precarious, and triangles so ill-conditioned, the results can of course only be regarded as a very rough approximation to the truth; for what they are worth, however, they are as follows:—1. That the course of the meteoroid was about S. 70° W. Probably it was 71° 45′, the complement of the magnetic declination. 2. That there was a proper motion of a little more than a mile a minute. 3. That the path was vertically over a line upon the earth's surface, whose least distance from Greenwich was 72 miles. 4. That the actual elevation was 44 miles. On this reckoning the body would seem to have crossed in the zenith in North Belgium, the Boulogne district, Cherbourg, and the north coasts of Brittany. STEPHEN H. SAXBY

East Clevedon Vicarage, Somerset, November 28

MY observation at Ramsbury, near Hungerford, was to the effect that while watching the northern aurora, my attention was called, at ten minutes past six, to this monster meteor, then slowly approaching in a direct line to the moon, which was shining most brilliantly. It seemed to pass exactly over the disc, and reappeared on the side, much reduced in size, as if going away from us; and at a distance of about 6° from the moon, scarcely seemed to measure more than 5° in length, it being then about 6h. 8m., which corresponds with the position over Sidmouth at that time. It was very definite in form, like a torpedo. I estimated its length at 15°, and 3° in breadth. I hope to have a hand-made photograph of its appearance ready for publication, by the Autotype Company, in a few days, and on the same sheet is a hand-delineation of the great comet to the same scale. ALFRED BATSON

The Rookery, Ramsbury

Lavoisier, Priestley, and the Discovery of Oxygen

In the last number of this journal my friend Mr. Tomlinson has criticised my observations on the respective claims of Lavoisier and Priestley to the discovery of oxygen. Without examining, or attempting to refute one of my arguments, and without the citation of any warrant, or authority, he has stated his opinions with an asseveration worthy of a 15th century Professor of Dogmatic Theology. His letter consists of *five* general statements, and *nine* dogmatic assertions. I have endeavoured to show that of the former, *two* are self-evident truths, or at least universally-admitted conclusions, while the remaining *three* are misstatements; and that of the latter *five* are completely erroneous, while *three* are open to question, and *one* is correct.

1. The universally admitted conclusions are:—(*a*) that "chemistry has no nationality," and that "discoverers are mutually dependent." Nothing that I have said can possibly be construed into the expression of a shadow of doubt concerning the truth of either of these statements.

2. The three misstatements are that (*a*) I have "thought it necessary to revive the old oxygen quarrel," (*b*) that I have "taken an unpatriotic part against Priestley," and (*c*) "endorsed the complacent statement of Wurtz, that chemistry is a French science founded by Lavoisier." If it be reviving a quarrel and acting an unpatriotic part against a man, to show that by the light of evidence hitherto overlooked one of the greatest scientific men of the last century has been unfairly accused of dishonesty, I am quite willing to be considered unpatriotic and a quarrel-monger. As to endorsing the statement of M. Wurtz, all I say is that he did not say it "without reason." Many people regard the assertion as quite unreasonable. I confess I do not, but at the same time I do not mean to say that I fully accept it.

[As to my "forgetting, perhaps, that the title 'La Chimie Française' was invented by Fourcroy, and objected to by Lavoisier," I may say that I do not see that this bears the least upon the question. Lavoisier's own words are "Cette théorie n'est donc pas, comme je l'entends dire la theorie des chimistes français, elle est *la mienne*, et c'est une propriété que je réclame auprès de mes contemporains et de la postérité." (*Œuvres de Lavoisier*, tome 2. 1862, p. 104) Dr. Thomas Thomson (*Hist. of Chem.* p. 101, vol. ii.) says, "Lavoisier's objection, then, to the phrase *La Chimie Française*, is not without reason, the term *Lavoisierian Chemistry* should undoubtedly be substituted for it." But this does not affect the question whether or no chemistry is a French science as M. Wurtz puts it, for surely Lavoisier was a Frenchman of the French. I say nothing, however, as to the justification of the remark that chemistry *is* a French science]

3. "That the compound is always equal to the sum of its elements was known long before Lavoisier" remarks Mr. Tomlinson. I have nowhere asserted that it was not, but the statement is new to me, and I should like to have references.

4. . . . "So early as 1630 Rey gave the true explanation of the increase of the weight of metals by calcination." Any one who will take the trouble to read through Rey's essay "*sur la recherche de la cause pour laquelle l'estain et le plomb augmentent de poids quand on les calcine*," cannot fail to observe how very vague his ideas on the subject were. He indeed attributed the increase of weight to thickened air (*l'air espessi*), but the following, as I have elsewhere stated, seems to have been his mode of reasoning:—Air possesses weight; it may be produced by heating water, which during distillation separates into a heavier and a lighter part; hence as air approximates to a liquid nature, it may be supposed to be separated into a heavier and a lighter part by the action of heat; now the heavier part (the "dregs") of air is more nearly allied to a liquid than air, for it has assumed a "viscid grossness," and this part attaches itself to calces during the process of calcination, and causes such of them as possess much ash to be heavier than before calcination. If we calcine a vegetable or animal substance there is no gain of weight, because the assimilated thickened air weighs less than the volatile matter expelled by heat; but in the case of a metal the assimilated air weighs more than the volatile matter expelled, hence there is a gain of weight. Thus he imagined that all calces, from a vegetable ash to a metallic calx, attract this thickened air. It can scarcely be said that a man with these extremely crude notions "gave the true explanation of the increase of weight of metals by calcination."

5 and 6. "Lavoisier's note of 1772 was, as he admitted, based upon Priestley's earlier experiments, begun in 1744." I can nowhere find in Lavoisier's writings any admission of the kind alluded to. (Will Mr. Tomlinson give references?). On the other hand, I do find a note by Lavoisier at the end of Chap. VI. *De la calcination des metaux*, published in the *Opuscules Physiques et Chimiques* (1774), (*Œuvres*, Vol. I., p. 621), in which he says, "Je n'avais point connaissance des experiences de M. Priestley, lorsque je me suis occupè de celles rapportées dans ce chapitre. Il a observé, comme moi et avant moi, . . . &c., &c." This would seem to sufficiently disprove the former statement.

Mr. Tomlinson speaks of Priestley's "earlier experiments begun in 1744." Now Priestley was born in 1733, and although no doubt a clever fellow he certainly did not begin to experiment at *eleven* years of age! His first paper on gases was published thirty-nine years later, viz. in 1772.

7. That "the acceptance of Lavoisier's doctrine was mainly due to the capital discovery of the composition of water by Cavendish in 1784," I utterly deny; and if desirable will show cause why. Nevertheless, as it has been so asserted, we may, for the present at least, regard it as an open question.

8. Mr. Tomlinson calls Black, Priestley, and Cavendish, "the founders of pneumatic chemistry." Surely John Mayor and Stephen Hales have a better right to the title.

9. "Priestley discovered oxygen in 1774." This, no doubt, is true in a sense because everybody says so. If it means that he got a gas from red oxide of mercury it is true. But let us not forget:—(*a*) that he discovered it by a random experiment, "by

accident" as he confesses; (*b*) that he regarded it as air containing nitrous particles; (*c*) that he remained in complete ignorance of its nature till March, 1775, before which time Lavoisier was well acquainted with its principal properties, and had recognised many of its functions.

10. "Cavendish discovered hydrogen in 1784." On the contrary, he described it in his "Experiments on Fictitious Air," published in 1766.

11. "Davy abjured Lavoisier's *principe oxygène*, and by his numerous discoveries gave the chemical edifice so rude a shake that it had to be taken down and rebuilt." From our point of view, *in spite of* the numerous discoveries of Davy, the edifice erected by Lavoisier, and which is still standing, had not to be taken down and rebuilt, except in one small part. The theory of acidification was a small part of Lavoisier's labours, and it was Berthollet who called chlorine *oxymuriatic acid*, and who thought that he had proved it to be a compound of muriatic acid and oxygen.

12. Mr. Tomlinson after asserting that "chemistry has no nationality," and that discoverers are mutually dependent," goes on to say with strange inconsistency that chemistry "had no proper existence for us until Dalton discovered its laws." Surely this is almost as if he slightly altered the "complacent statement of Wurz," and said, "Chemistry is an English science; it was founded by Dalton of immortal memory." We do not think that many will differ from us when we say that chemistry was a science long before the time of Dalton.

Thus we have endeavoured to show that of the nine dogmatic assertions given above (numbered 4-12):—*one*, viz. 9, is correct; *three*, viz. 7, 8, and 11, are open to grave question; while *five*, viz. 4, 5, 6, 10, 12, are altogether erroneous.

There is no possible excuse for us to remain any longer in ignorance of the mighty works done by Lavoisier. The fine quarto volumes, 1862-1868, published by the French government, are a fitting monument to the genius of the man. The petty jealousies which disfigure the history of science during the end of the last, and commencement of the present century, ought to find no place in our minds. The Republic of Science is large enough for every man to receive his due.

G. F. RODWELL

The Comet

IT would scarcely perhaps be civil to take no notice of Mr. Backhouse's letter in NATURE, vol. xxvii. p. 52, the object of which seems to be principally to discredit my account of the disappearance of the comet in a moonlit sky. Still less, however, would it be reasonable to take offence at it—albeit, Mr. Backhouse is wrong. Indeed, a little more reflection might have shown him that ample time having elapsed without any correction from me appearing in your columns, the presumption must have been strong that I had nothing to correct. I have in fact seen the comet frequently since—as well as many times before—and am moreover really experienced enough not to have made quite so gross a blunder; or at least to have found it out, if I did make it, when so many subsequent opportunities permitted. Besides that, I have fortunately the following testimony in corroboration. One of my sisters wrote, "What you did not see of the comet agrees exactly with F.'s experience. She looked out at Court-Lodge: splendid night; many, even small, stars, though moon shining bright; but the comet *wasn't to be seen*, though she and Miss B. scanned the whole fine expanse of east and southeast sky." Another wrote about the same time that though visible two days later, it was so pale that she did not wake a nephew who wished to see it. My drawing of the 23rd October has two stars above the nucleus, with one of which it made the base of an isosceles triangle, the other being at the vertex. These two stars were plainly visible all the morning of the 30th, but not so high above the roof across the way, but what the motion of the comet since I last saw it (23rd) *may* have lowered it enough to conceal the nucleus. In fact, either I am wholly right as to the disappearance, nucleus and all, under moonlight, or at least the nucleus must have been concealed. There is no other alternative. As to the great sweep of tail—let us be reasonable in our guesses as to the fallibilty of others however improbable their evidence. May not something for instance be ascribed to the London atmosphere as likely to increase the amount of moonlight reflected? It was for this that I wished the observation made public, viz. as a real phenomenon having a real cause; all the more interesting that it was so surprising—nay, as it seems, so incredible. My only regret is that I have been now tempted into so long a reply.

Before I leave the comet, may I presume to express my surprise that the question as to this comet's return is still *sub judice*. It is said that three well observed places are enough to determine the elements of a comet's orbit. But this one has surely furnished more nearly a score since its perihelion, to say nothing of those before—which no doubt belong to a previous orbit. It is not without fear that I may be misunderstood, that I ask of those who are skilled in such things for an explanation, knowing that of all men they are most deeply interested in the early solution of such a question. It may be said that the observations at and about the time of perihelion have scarcely yet reached this country; but is not the fact that the comet was at one time, which I imagine is known with some certainty, *behind the sun's disc*, equivalent to an observation of its place sufficiently exact to rank with others in calculating the orbit? I do not presume to say that it is so. I merely formulate a question which, in its general bearing, must surely be agitating the minds of many besides myself, after all we have read about the possible past history and future fate of this remarkable comet. It has now been under observation during two months, in which time it must have traversed nearly one quarter of its entire orbit, if an elliptical one of moderate extension. Its present path in space must be so nearly straight that continued observation can hardly be expected to furnish improved data until, if ever, departure from that shall settle the question decisively in favour of an elliptical path. But is it for this that we must wait? I can hardly think so, for surely no comet has ever yet been *seen* in the neighbourhood of aphelion.

J. HERSCHEL

30, Sackville Street, November 18

An Urgent Need in Anthropology

BOTH zoology and geology possess a yearly "record" of the work achieved in their respective domains, but anthropology still remains without that aid to its proper advancement. All workers are of course cognisant of the current bibliography given in the German anthropological publications, and the supplemental information on the same subject contributed by Dr. O. Mason in the *American Naturalist*, and are not unappreciative of the same; but these lists are but partial, and necessarily incomplete, as must be evident when the peculiar nature and wide scope of the study of man is taken into consideration.

Compared with anthropology, the record of zoological work is simple in the extreme. Zoology possesses its accredited organs and regular channels of publication, and with trifling exceptions, its yearly work can be gleaned from these sources. But what is anthropology? It may be described as the very Talmud of humanity with its "Mishnah" of ethnological facts, and its "Gemara" of anthropological conclusions. Scattered up and down the bye-ways of literature, here and there recorded by the traveller, illustrated by the historian or accentuated by the essayist, hidden in blue-books, and awaiting extraction from medical reports, existing in the papers of the missionary and the publications of the statisticians are the unaccumulated and unrecorded facts and observations which form the foundation on which to rear a complete science of man. Our own savages afford as excellent illustrations of the comparative in civilisation as do the primitive peoples of the jungle or the swamp, and hence a large fund of information is still to be supplied and tabulated from our city alleys, prisons, and lunatic asylums. To the question, Is such a record needed? must be added, How is such a record possible?

It seems at once clearly impossible that such a work could be either intrusted to the care of one man, or to the men of one nationality. No individual can be expected to have perused the whole current literature of his country, and could such a phenomenon be discovered, it is still more unlikely that he would combine in himself those qualities which are necessary to detect the varied data that make for anthropology. An alternative course, however, is present, which is possible, and not too exhaustive as regards time and labour. In each country where anthropology is cultivated as a science, a few of its votaries could form an association for the purpose of abstracting from its literature such facts, arguments, and observations as appertain to the study of man, and these might, in a condensed and tabulated form, appear as a regular yearly contribution in the pages of the different publications of the varied ethnological and anthropological societies which now embrace so many nationalities. It is perhaps not presumptuous to say that these papers would not be the least valuable in the volumes in which they appeared. It seems work that anthropological societies might justly undertake, and we might then expect to hear less of the little interest felt in the science by the general public. When we have an "applied anthropology" to our daily life, and a system of anthropology taught in our public schools, we shall wonder how it was that the science so long remained in the esoteric stage. However, paradoxical as it may seem for the writer to admit, no science has been illustrated by so many excellent handbooks and compendiums as anthropology. From the time of Prichard to the works of Lubbock, Peschel, and Tylor, there have always been competent workers and writers, and the last-named works represent the very essence of our knowledge on the subject. In the face of this there is still a vast and unrecognised mass of material waiting extraction from the total annual literature of each country.

One other work requires compilation, and refers to the past. How frequently a traveller or missionary, anxious to write fully on the people he has visited, and wishing at the same time to have his views enlarged by the opinions of others, inquires for the list of authors and authorities who have written on the same subject. With very few exceptions such a desideratum is unprocurable, and yet if we would at present understand the social position of any tribe, however degraded or improved, the records of their earliest visitors must be compared with the narratives of their latest describers. This again can only be the work of a specialist, who, having carefully searched for and studied the literature relating to some particular tribe or race, would voluntarily present his "bibliography" to students at large, and for that purpose endeavour to have the same published by his local or some other anthropological society. These lists, if once begun, would slowly accumulate, and would not only confer lasting fame on their compilers, but also, by their publication in the *Transactions* of the societies devoted to the study of man, make the contents of those works more valuable by their presence, and at the same time promote the absence of some memoirs which a further knowledge of the subject would render somewhat unnecessary.

It is, however, only in the hope of further suggestions from other workers, that I have ventured to obtrude these remarks in the columns of NATURE. W. L. DISTANT

A Modification of the Gold-leaf Electroscope and a Mode of Regulating its Charge

THOSE who experimentalise with the usual form of gold-leaf electroscope must know well that the instrument requires a vast amount of preparation and drying before it is ready for use, and also that in wet weather it keeps its charge but a little while. At the same time the electroscope when in good order is a beautifully sensitive instrument and of great value in demonstration. I have made a slight addition to the present form of instrument, which makes it useful in all states of the weather. A flat spiral is cut out of sheet ebonite with a fret saw, about 8 mm. wide, and 4 mm. thick; the diameter of the spiral is the same as the internal diameter of the glass shade; the spiral is cemented to the shade just below the line at which its dome begins, the centre of the spiral carries the brass rod to which the gold leaves are attached; the rod comes up through the top of the shade without touching it; thus a very long insulator is placed between the charged leaves and the surface of the shade; on a damp day the leaves are powerfully divergent two to three hours after being charged. If instead of the spiral a little tube of ebonite takes the place of the usual varnish glass tube, the charge will be kept a fairly long while.

If the same angle of divergence of the gold leaves be required in two similar electroscopes, charged, say, with electricity of opposite sign, this can be effected by fully charging each instrument, and then bringing a lighted candle about ten centimetres above the brass disc or knob of each; by lowering or raising the candle, the charge can be drawn off as slowly as you please. It is well known that a flame has been used ATTACHED to an electrometer in testing atmospheric electricity. Volta used a flame connected to an exploring rod, and in Sir W. Thomson's electrometer a slow-burning match is used; but it will be noticed that in the experiment I have described for regulating the charge, the flame is only held near the disc or knob, but is NOT allowed to TOUCH IT. I also find, and it is very remarkable, that electroscopes can be fully charged by placing them about a metre from a charged jar, if a taper be now placed on the top of the jar, by means of an insulator the leaves instantly diverge and the electroscopes remain charged. FREDERICK JOHN SMITH

Taunton, November 18

Palæolithic Gravels

THE subject of the preservation of human remains in drift beds has been so fully discussed by every author who has written on the antiquity of man, that it would be mere waste of space to reprint what has been so many times printed before. No doubt the day will one day arrive when we shall have plenty of examples of the osseous framework of palæolithic man; at present but few of his bones have been found for study. Human bones are extremely liable to decay, but no doubt some of our palæolithic precursors are preserved somewhere; they will be lighted on some day.

In 1878 I had an opportunity of removing the stones from several cairns at Cynwil Gaio, in Carmarthenshire; the kist-vaens or stone graves were then exposed. On carefully removing the covering stones from each kist, the place in which the human body was originally deposited was laid bare. The soft, smooth bed of fine clay (brought from a distance) was there on which the body was placed at the time of burial, but not in a single instance was there a trace of a bone, or tooth, or any relic whatever of the body; it had entirely vanished. Now if we can find nothing in a grave that is only a very few thousands of years old, what can we expect from one that is tens or possibly hundreds of thousands?

When Prof. T. McK. Hughes lectured on the Antiquity of Man before the Victoria Institute he said (reprint p. 8): "I will not waste time to discuss whether the objects we refer to man, now found in numbers in post-glacial river-gravels, are really of human work." The Professor was quite right, for any one who can see any art in the Parthenon, or any human work in Raphael's Cartoons, ought to see art in palæolithic implements; and, of its class, uncommonly good art too. But none are so blind as those who won't see, and many persons have not strength of mind or courage enough to accept the teachings of their own reason. WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, North

Ancient Monuments

WHILST in North Wales last autumn, I visited the famous Kist-Vaen, on Tynycoed Farm, Capel Garmon, not far from Bettws-y-coed. This is a sort of double subterranean cromlech, the single cap-stone now remaining being on a level with the ground. On two of the large upright supporting stones, two blockheads had painted their names in green oil-paint from top to bottom of the stones. The trouble of taking the green paint and brushes to this place must have been considerable, and I hope now that General Pitt-Rivers is appointed Inspector of Ancient Monuments, he will find these parties out, and make them take a pailful of turpentine, and rub out the offensive inscriptions.

I also visited the two circles of stones, termed on the Ordnance Map Maenan-hirion, by Penmaenmawr, and looked out for the two outlying stones stated to be on the north-east side of the larger circle. I could not see them; there is a large naturally-imbedded boulder on the east-south-east side, but the intermediate one has been removed. Whilst I was at the smaller circle, I noticed that one of the stones had recently been pulled out of its setting, and was lying beside the hole.

The great camp on Penmaenmawr was plentifully bestrewn with sandwich papers and empty bottles, but the immense walls and hut circles of our forefathers defy the efforts of excursionists to a great extent. I, however, saw several of these terrible persons on the top, taking off the stones from the ancient walls and throwing them down below.

I noticed several other stones in the neighbourhood of the circles that had recently been thrown over.

In some of the more romantic and rocky situations in Wales—places visited by "cheap trips" (as near Bettws)—the rocks and even highly-esteemed antiquities—as the elaborately carved road-side cross at Carew, Pembrokeshire—are plastered over with printed bills about auctions, tea-meetings, sermons, and quack medicine. WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

Shadows after Sunset

IN reference to Mr. Douglas Archibald's letter, I may say that in 1873 I made three drawings of the "Sheaf rays" at the Isle of Wight. In these they are marked as "converging in the east," but the point is apparently below the visible horizon. Shortly after I had, however, the opportunity of seeing the true convergence, as we were crossing the Peasmarsh, a large common near here. It was after rain, and there appeared a very bright spot in the east opposite the true sun, which to the best of my recollection was setting and not set, for I momentarily took the appearance to be some form of reflection of the sun itself. The rays were quite strong in the east and west, and though fainter could be distinctly traced across the sky. I believe that there were no clouds and that the ray intervals were equidistant, though I will not be certain on this point. I notice that one of my drawings also shows this peculiarity, though I confesss my impression has been hitherto that these rays were due to the interference of clouds. J. RAND CAPRON

Guildown, Guildford, Nov. 24

On the Isomerism of Albuminous Bodies

AMONG organic compounds there are large number of bodies hoving the same composition, but different constitution. They are called isomerides. The number of these isomerides increases in proportion as the number of atoms which they contain increases.

Prof. Cayley has already calculated the possible number of isomerides of hydrocarbons. From his result it can be easily seen that the increase of isomerides in proportion to the complexity of the composition is an exceedingly rapid one.

Now the number of atoms which the so-called albuminous bodies contain are very large. The number of isomerides which they can give therefore must be exceedingly large, in fact almost innumerable.

Prof. Schorlemmer, in his "Rise and Development of Organic Chemistry," says: "The enigma of life can only be solved by the synthesis of albuminous compounds." If then these albuminous bodies are really the basis of life, the different species of living beiags must come from innumerable sources, for albuminous bodies have innumerable isomerides. According to this theory, we can say that the different species of living beings, whether animals or plants, were developed out of the chemical compounds having the same composition, but different constitution, but cannot assert, as some do, that they were developed out of the same source, or a few sources

Tokio, Japan, October 12 SHIGETAKÉ SAGIURA

An Extraordinary Lunar Halo

ON Monday evening, November 20, an unusual halo surrounded the moon from 6.15 to 6.25. The moon was not quite full, and the halo to some extent assumed the form of the moon. The halo consisted of a succession of concentric rings. The ring next the moon was equal to four diameters of the moon, and had a soft yellow-white radiance, almost equalling the moon in brilliancy; it was surrounded by a succession of prismatic rings, red commencement, and proceeding outward orange, yellow, green, blue, indigo, and violet. At 6.15 the chromatic rings were pretty sharply defined, with the exception of the outer one, which was faint and evanescent. Outside of the ring was a corona-like envelope. This aspect continued about five minutes, and during the next five minutes rapidly changed; the edges of the rings became irregular, radii shot from the rings towards the moon, and at 6.25 the phenomenon disappeared.

Newcastle-on-Tyne, November 24 J. P. BARKAS

Meteor

A BRIGHT meteor was seen here about 4.30 p.m. in the east. It did not explode, but dissipated itself with scintillations. It reached a very low level before it disappeared.

Oxford, November 27 W. L. HARNETT

Flame in Coal Fire

THE flame referred to by Major Herschel (NATURE, vol. xxvii. p. 78) is simply that of carbon monoxide, which may be observed in most coal fires, after the hydrocarbons are consumed, burning with a pale blue flame. Any yellow tint is of course due to sodium present in the coal. The production of carbon monoxide depends more upon the arrangement of, than the quality of, the coal. Major Herschel will find the reason of its presence give in any text-book on chemistry.

I cannot understand what advantage is obtained by removing the slit of the spectroscope, especially if one wishes to show that a flame is mono-chromatic. When burnt at ordinary pressure, carbon-monoxide has no definite spectrum. SM.

Rugby, November 24

Waterspouts on Land

I AM of opinion that the phenomena referred to by Mr. Hossack are not the effect of waterspouts, but are rather to be attributed to landslips. I may mention a case which may throw some light on the matter. About 1872 (I cannot give the exact date) a landslip occurred on the banks of the Tay, about seven miles north of Dunkeld. close to Guay Station on the Highland Railway, and on the east side of that line. I lived close by at the time, and shortly afterwards saw the effects. Local opinion attributed it to the following causes:—Along the top of the gravelly slope planted with oak and other trees, ran a brook. Immediately above the place where the landslip occurred, the banks of the brook had been burrowed by rabbits. When the sudden flood occurred which caused the landslip, the water of the brook entered these holes, undermined the gravelly slope or terraced beach, and precipitated it across the highway into the field below, devastating fully an acre of it. The trees, turf, &c., were deposited in the field much as they grew upon the slope. I was surprised that they had not been overturned, but it would appear that they had slid down. The effects are still quite visible to passengers on the railway. Had they been photographed at the time, they would have formed a capital illustration for a geological text-book.

Guildhall Offices, Carlisle JOHN GEDDES MCINTOSH

NOTES FROM THE LETTERS OF CAPTAIN DAWSON, R.A., IN COMMAND OF THE BRITISH CIRCUMPOLAR EXPEDITION

MAY 21. *On board the s.s. "Nova Scotian."*—A grey sky, a grey foam-flecked sea, floating ice-floes, fog and rain, with a thermometer a few degrees above freezing—such are the features of the Gulf of St. Lawrence this morning, and a cheerful welcome to the New World. Our course has been a long way to the south of Newfoundland on account of the ice, consequently our passage has been a long one. Yesterday was quite lovely, several icebergs were in sight eight or ten miles off, looking like peaks of snow mountains at a distance; now we are in the midst of ice fields delaying us a good deal, as at times it is difficult to find a passage.

May 22. *Quebec.*—We sighted land last night, and saw such a lovely sunset as we went up the St. Lawrence. We have been steaming up the river eighteen hours, but we cannot yet see the land on both sides. We have just passed the *Peruvian*, which left Liverpool a fortnight before us, but she got among the ice and broke her screw, and has been twenty-seven days on the voyage. Another of the Allan line steamers ran into an iceberg. So we feel lucky in getting across without mishap. At the end of the week I start for Winnipeg—2,500 miles by rail—a long journey of five days and four nights.

I find Quebec quite wintry after England; indeed, the snow is still lying in sheltered places where it has drifted, and no trees are in leaf.

June 3. *On Lake Huron.*—On reaching Toronto we went back again into summer—everything was green and spring-like, and the air was quite soft and balmy.

We left Toronto for Sarnia, where we embarked for Duluth, on the west end of Lake Superior—thence it is about twenty-four hours' journey to Winnipeg. Toronto was looking very well. There are groves of horse-chestnut trees in the principal streets, which have a very good effect. At Toronto I was introduced to the Canadian Premier, who took a great interest in my expedition. I also dined with the chief of the observatory there, and they gave me some wine at dinner which was made from their own vines in the suburbs. To Sarnia is about six hours—a most fertile country. The weather, however, is very rainy at present—this is the wet time of the year.

Every train and steamer is full of young Englishmen on their way out to Winnipeg, where they expect to make their fortunes, and no doubt it is a great place to make money just now.

We were two days running up Lake Huron, for the most part out of sight of land, and the land we did see was flat and ugly, till in the evening of the second day we reached the river joining the two lakes (Lake Huron and Lake Superior), and anchored at Sault St. Marie for the night. The river runs between rocky pine-covered shores, and in the evening we had one of those sunsets one only seems to see in this country—a blood-red sky overhead, orange at the horizon, with the pine woods rising black against it, and the broad reaches of the river winding away westward, all a blaze of golden light—just the subject for Turner.

The next morning we went through a lock into Lake Superior, and twenty-six hours' run took us to Thunder Bay, a barren looking place. Hard by is Thunder Cape, a bold headland 1,300 feet high. The water of Lake Superior never rises more than two or three degrees above freezing-point, and in old days it would certainly have been thought an enchanted lake, so strange are the effects of the mirage. At one moment you see a long line of cliffs, a minute later they have turned into a reef of rocks hardly above the water, or a little table-topped mountain on the horizon suddenly splits into two sharp peaks, and anon takes the shape of an hour-glass.

June 8. *Winnipeg.*—When we awoke this morning we were on the prairies—just like the sea, only grass instead of water—a green plain losing itself in the far horizon. The journey along the Northern Pacific Railway, from Duluth, by the side of the rapid river St. Louis was lovely.

Winnipeg is a flourishing place with 20,000 inhabitants, where a few years ago there was nothing but a few huts. It stands on the Red River of the north—a fine river about the size of the Rhine. All the people here are Cree Indians, who speak their language and don't understand English, but they are dressed in European dress, so they look more like gipsies than anything else.

June 27. *Fort Carlton.*—We arrived here yesterday, such lovely country, like an English park, with wild roses and other flowers growing in great profusion. The river rather reminds me of the Thames at Richmond. The Saskatchewan is a magnificent stream, far larger than the Red River, flowing between pine forests. The weather is simply perfect, except that the sun is rather hot in the middle of the day. The fare is rather rough. No milk or fresh bread—chiefly fish, biscuit, and salt meat. It was slow work getting up the rapids. A boat with a crew of Indians takes out a hawser a mile long, which is made fast to a tree above the rapids, then the other end is brought down to the steamer, and fastened to the capstan, and we slowly drag ourselves up. The steamer is propelled by an enormous paddle wheel at her stern, and at the bow is a great arrangement of spars for lifting her off sandbanks, should she run aground; and though she carried 150 tons of cargo she only drew three feet of water.

On the 23rd we reached the Forks of the Saskatchewan, where the river divides; we took the northern branch and warped up the rapids to the settlement of "Prince Albert," where the country looks quite like England. Land is to be had here for 2 dollars or 8*s.* an acre, and it seems wonderfully fertile—the soil looks so rich. It is certainly the place I should recommend any enterprising emigrant to come to if he only has a little capital to start with—300*l.* would be plenty. The soil wants no clearing; you have only to build a house and plough and sow your land. The climate is one of the finest in the world. I was talking to a retired officer of the 50th who has been here seven years, who says he has never had an hour's illness, and feels as though he were growing younger every year.

Fort Carlton is the *beau idéal* of a Hudson's Bay fort, with a stockade twenty feet high and towers at the corners. But the days when the Blackfeet made their raids are over, and the Cree or Ojibbeway Indians, whose "lodges" one sees all around, are very pacific. A great many speak a little French, but no English.

July 14. *Ile à la Crosse.*—We left Carlton on the 30th, *i.e.* my own party and two missionaries. I had a train of ten Red River carts drawn by horses and oxen. I drove in a light American waggon. The scenery was at first like English country, only without hedges. There was plenty of deep grass and vetches, which afforded splendid fodder for the animals. There were quantities of snipe, duck, and prairie chicken. The land was gay with wild flowers; orange-lilies were most conspicuous, and lots of wild strawberries The mosquitoes were the only drawback, at times forcing us to wear veils and gloves, and to eat our meals in the smoke of our camp fires. After three days we reached a hill, from whence we saw the great sub-arctic forest stretching away like a sea to the north. It extends nearly to the Arctic Circle, and from the Atlantic to the Pacific.

On the 9th we reached Green Lake, but it blew so hard that we did not start till the 11th. Our conveyance was a Hudson's Bay Company's inland boat; our crew was of Crees and Chipewyans. The latter speak a language like the ancient Mexicans, quite unlike any other I have heard; it is like the noise of a person choking. It takes years to learn even a smattering of it. We drifted down the stream all night, our boats being lashed together, and we slept as best we could in the bottom of them.

Ile à la Crosse is on an island in the middle of a lake, and is comparatively free from mosquitoes. I had a splendid boat's crew—seven oars and a steersman; we pulled nearly fifty miles the first day. We rested on Sunday, and the day after crossed Buffalo Lake most fortunately—a fair wind sprang up just in time to take us across, as it cannot be crossed against the wind. Then we began to ascend the Rivière la Loche, which took us all the next day, there being two portages or places where the contents of the boats and sometimes the boats themselves have to be taken overland. Thence we entered Methy Lake, about thirty miles long at the north, and a narrow creek took us to the beginning of Portage la Loche, or the Long Portage, which is a road some twelve or fourteen miles long, leading to the Clear Water River which flows into the Athabasca and ultimately into the Mackenzie, so we are on the Arctic Slope at last.

July 22. *Portage la Loche.*—I rode over last night in company with the Hudson's Bay Company's officer in charge of the post. The road leads through pine woods, and passes a pretty lake, and ultimately descends a hill of about 400 feet into this valley. I am writing this in my tent, pitched on the bank of the Clear Water River, which flows past about three yards off. Across the river are wooded hills 600 feet high; to the left the river disappears among the pine woods in a dark ravine; to the right it winds away in the distance among blue hills. It is all so green and pretty that it is difficult to believe that in a few months all will be ice and snow. All the last week the heat has been intense, the thermometer over 86° in the shade all day. This morning we saw a bear prowling about opposite. We are now among the Chipewyan Indians; they are very different from the Crees; in appearance they remind me a little of drawings of the Esquimaux, with round greasy faces. About here they are mostly Roman Catholics, as there is a large mission at Ile à la Crosse.

The best description of this country in general is by saying that it is like Switzerland without mountains, but with big rivers and lakes. The plants are much the same, and the climate is much the same. The trees are very fine, and, as elsewhere, strawberries, raspberries, cranberries, black and red currants, and gooseberries grow wild.

There is a fine view down the valley from the top of the hill; it was mentioned by Sir J. Frankland, who has been through all this country.

July 24.—The Athabasca boats arrived last night, so we are off this morning.

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE[1]

II.

In the preceding investigation nothing has been said as to the units in which the quantities m and H are measured. It will be convenient, before proceeding further, to consider shortly the measurement of magnetic and electrical quantities in absolute units, and particularly the centimetre, gramme, second (c.g s.) system now generally adopted.

According to [illegible] is called the electro-magnetic system, all magnetic and electrical quantities are measured by units which are derived from a magnetic pole chosen as the pole of unit strength. This pole might be defined in many ways; but in order to avoid the fluctuations to which most arbitrary standards would be subject, and to give a convenient system in which work done in the displacements of magnets or conductors, relatively to magnets or to conductors carrying currents, may be estimated without the introduction of arbitrary and inconvenient numerical factors, it is connected by definition with the absolute unit of force. It is defined as *that pole which, if placed at unit distance from an equal and similar pole, would be repelled with unit force.* The poles referred to in this definition are purely ideal, for we cannot separate one pole of a magnet from the opposite pole of the same magnet: but we can by proper arrangements obtain an approximate realisation of the definition. Suppose we have two long, thin, straight, steel bars, which are uniformly and longitudinally magnetised; their poles may be taken as at their extremities; in fact, the distribution of magnetism in them is such that the magnetic effect of either bar, at all points external to its own substance, would be perfectly represented by a certain quantity of one kind of imaginary magnetic matter placed at one extremity of the bar, and an equal quantity of the opposite kind of matter placed at the other extremity. We may imagine, then, these two bars placed with their lengths in one line, and their blue poles turned towards one another, and at unit distance apart. If their lengths be very great compared with this unit distance, say 100 or 1000 times as great, their red poles will have no effect on the blue poles comparable with the repulsive action of these on one another. But there will be an inductive action between the two blue poles which will tend to diminish their mutual repulsive force, and this we cannot in practice get rid of. The magnitude of this inductive effect is, however, less for hard steel than for soft steel, and we may therefore imagine the steel of our magnets so hard that the action of one on the other does not appreciably affect the distribution of magnetism in either. If, then, the two blue poles repel one another with a unit of force, each according to the definition has unit strength.

The magnitude of unit pole is by the above definition made to depend on unit force. Now unit force is defined, according to the system of measurement of forces founded on Newton's Second Law of Motion, the most convenient system, as that force which, acting for unit of time on unit of mass, will give to that mass unit of velocity. Our unit pole is thus based on the three fundamental units of length, mass, and time. According to the recommendations of the B.A. Committee, and the resolutions of the Paris Congress, it has been resolved to adopt generally the three fundamental units already in very extended use for the expression of dynamical, electrical, and magnetic quantities, namely, the centimetre as unit of length, the gramme as unit of mass, and the second as unit of time. With these units, therefore, the unit force is that force which, acting for one second on a gramme of matter, generates a velocity of one centimetre per second. This unit of force has been called a *dyne.* The unit magnetic pole, therefore, in the c.g.s. system of units is that pole which, placed at a distance of 1 centimetre from an equal and similar pole, is repelled with a force of 1 dyne. Each of the poles of the long thin magnets of our example above is therefore a pole of strength equal to one c.g.s. unit, if the mutual force between the poles is 1 dyne.

The magnetic moment m of anyone of the deflecting magnets is equal to the strength of either pole multiplied into the distance between them, which for magnets of such great length in comparison with their thickness is nearly enough the actual length of the magnet. Therefore either pole has a strength of $\frac{m}{2l}$ units. If r and l are measured in centimetres, and W in grammes, the strengths of the magnetic poles deduced from equation (4) or (6) will be in c.g s. units.

A magnetic field is the space surrounding a magnet or a system of magnets, or a system of conductors carrying currents, at any point of which, if a magnetic pole were placed, it would be acted on by force. From the definition of unit magnetic pole we get at once the definition of magnetic field of unit intensity. *Unit magnetic field is that field in which unit magnetic pole is acted on by unit force*, and in the c.g.s. system, therefore, it is that field in which unit magnetic pole is acted on by a force of one dyne. In the theory of the determination of H, given above, the horizontal force on either pole of the needle due to the horizontal component of the earth's field is taken as $\frac{m'}{2l'}H$, and again the horizontal force on either pole of the deflecting magnet as $\frac{m}{2l}.H$. H is, therefore, the strength in units of magnetic field intensity of the horizontal component of the earth's field. By formula (5) or (7), when r and l are taken in centimetres, and W in grammes, H is given in dynes; that is, it is the number of dynes with which a unit red pole would be pulled towards the north, and a unit blue pole towards the south if acted on only by the earth's magnetic field. We can now go on to the measurement of currents.

According to the theory of electro-magnetic action given by Ampère, every element of a conductor in which a current is flowing acts upon a magnetic pole with a force which varies inversely as the square of the length of the line joining the centre of the element with the pole, and directly as the strength of the current and as the length of the projection of the element on a plane at right angles to that line. The direction of this force is at right angles to a plane drawn through the pole and the element, and acts towards one side or the other of that plane, according as the current in the element is in one or the opposite direction, and according as the magnetism of the pole is red or blue. From this it is easy to obtain a definition of unit current in the electro-magnetic system. It is that current which, flowing in a wire of unit length bent into an arc of a circle of unit radius, acts on a unit magnetic pole placed at the centre of the circle with unit force. Thus the current of unit strength in the complete circle of unit radius would act on a unit pole at the centre with 2π units of force, in the c.g.s. system with 2π dynes. This force acts towards one side or the other of the plane of the circle, according to the nature of the pole and the direction of the current. If the current, considered as flowing from the copper plate to the zinc plate of a Daniell's cell, were made to circulate round the face of a watch in the direction opposite to that in which the hands move, a red pole placed at the centre would be moved out through the face of the watch, and a blue pole in the opposite direction; and the opposite would be the case if the current were reversed. This is easily remembered by those familiar with the representation of couples in dynamics, by observing that when the direction of the current is the same as that in which a positive couple tends to turn a body, the direction in which a red pole is urged is that in which the axis of the couple is drawn. Or, the direction of the force may be found at any time, by remembering that the earth may be imagined to be a magnet turned into position by the action of a current flowing round the magnetic equator in the direction of the sun's apparent motion.

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[1] Continued from p. 35.

From the definition of a magnetic field we see that unit current may also be defined as that current which, flowing in a wire of unit length bent into an arc of a circle of unit radius, produces at the centre of the circle a magnetic field of unit intensity. The direction of the resultant magnetic force at that point is by Ampère's law at right angles to the plane of the circle, and the side towards which it acts in any particular case may be found as stated above.

If we take then the simple case of a single wire bent round into a circle and fixed in the magnetic meridian, with a magnet, whose dimensions are very small in comparison with the radius of the wire, hung by a torsionless fibre so as to rest horizontally with its centre at the centre of the circle, we may suppose that each pole of the magnet is at the same distance from all the elements of the wire. A current flowing in the wire acts, by Ampère's theory, with a force on one pole of the needle towards one side of the plane of the circle, and on the other pole with an equal force toward the other side of that plane. The needle is thus acted on by a couple tending to turn it round, and it is deflected from its position of equilibrium until this couple is balanced by the return couple due to H. Let us suppose the strength of each pole of the needle to be m units, r the radius of the circle, and C the strength of the current in it. Then by Ampère's law we have for the whole force without regard to sign, exerted on either pole of the needle by the current, the value $Cm\frac{2\pi r}{r^2}$ or $Cm\frac{2\pi}{r}$. If l be the length of the needle the couple is $Cm\frac{2\pi}{r}l$, before any deflection has taken place. After the needle has been deflected through the angle θ the arm l of the couple has become $l\cos\theta$, and therefore the couple $Cm\frac{2\pi}{r}l\cos\theta$; and the return couple due to H is $mHl\sin\theta$. Hence we have equilibrium when

$$Cm\frac{2\pi}{r}l\cos\theta = mHl\sin\theta$$

and therefore

$$C = \frac{Hr}{2\pi}\tan\theta \quad . \quad . \quad . \quad . \quad . \quad (8)$$

if θ be the observed angle at which the needle rests in equilibrium when deflected as described from the magnetic meridian. If instead of a single circular turn of wire we had N turns occupying an annular space of mean radius r, and of dimensions of cross-section small compared with r we should have

$$C = \frac{Hr}{2\pi N}\tan\theta \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In practice the turns of wire of the tangent galvanometer may not be all contained within such an annular space. It is necessary then to allow for the dimensions of the space occupied by the wire. For a coil made of wire of small section we may suppose that the actual current flowing across a unit of area is everywhere the same. Hence if C be the current strength in each turn, and n the number of turns in unit area, we have for the current crossing the area A of an element E the value $2\pi nCA$. Taking a section of the coil through the centre, let BC be a radius drawn from the centre C in the plane cutting the coil into two equal and similar coils, and taking $CD\,(=x)$ and $DE\,(=y)$ at right angles to one another, we have $A = dx\,dy$ and $CE^2 = x^2 + y^2$. Hence the force exerted on a unit magnetic pole at the centre C by the ring supposed at right angles to the plane of the paper, of which this element is the section, will be $\frac{2\pi nCy\,dx\,dy}{x^3 + y^2}$ in the direction at right angles to CE and in the plane of the paper. If we call the component of this force at right angles to BC, dF, we have

$$dF = \frac{2\pi nCy^2\,dx\,dy}{(x^2 + y^2)^{\frac{3}{2}}}.$$

Hence for the whole force at right angles to BC we have

$$F = 2\pi nC\int_{-b}^{b}\int_{r-c}^{r+c}\frac{y^2\,dx\,dy}{(x^2 + y^2)^{\frac{3}{2}}},$$

where r is the mean radius of the coil, $2b$ its breadth, and $2c$ its depth in the plane of the circle.

Integrating, and putting N for the whole number of turns $4nbc$, we get

$$F = \pi NC\,\frac{1}{c}\log\frac{r + c + \sqrt{(r + c)^2 + b^2}}{r - c + \sqrt{(r - c)^2 + b^2}} \quad . \quad (10)$$

If θ be the angle at which the deflecting couple is equilibrated by the return couple due to H, we have as before the equation

$$F = H\tan\theta.$$

Hence, substituting the above value for F and solving for C, we have finally

$$C = \frac{H\tan\theta}{\pi N\,\frac{1}{c}\log\frac{r + c + \sqrt{(r + c)^2 + b^2}}{r - c + \sqrt{(r - c)^2 + b^2}}} \quad . \quad (11)$$

When the value of r is great in comparison with b and c this reduces to the equation

$$C = \frac{Hr\tan\theta}{2\pi N} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

which we found before by assuming all the turns to be contained in a small annular space of radius r. In practice, in galvanometers used as standards for absolute measurements, generally neither b nor c is so great as $\frac{1}{10}$ of r, and in these cases the difference between the values given by equations (11) and (12) is well within the limits of errors of observation, and the correction need not be made. The value of C given by (12) is then to be used.

In this investigation the suspension fibre has been supposed torsionless. If a single fibre of unspun silk is used as described below for this purpose, its torsion may for most practical purposes be safely neglected. The error produced by it may however be easily determined and allowed for by turning the needle, supposed initially in the magnetic meridian, once or more times completely round, and noting its deviation from the magnetic meridian in its new position of equilibrium. The amount of this deviation, if any, may be easily observed by means of the attached index and divided circle, or reflected beam of light and scale, used as described below, to measure the deflections of the needle. From the result of this experiment the effect of torsion for any deflection may be calculated in the following manner.

Let α be the angular deflection, in radian[1] measure, of the magnet from the magnetic meridian produced by turning the magnet once round, then the angle through which the thread has been twisted is $2\pi - \alpha$. The couple produced by this torsion has for moment $Hlm\sin\alpha$.

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[1] A *radian* is the angle subtended at the centre of a circle by an arc equal in length to the radius. It has generally been called in books on trigonometry hitherto by the ambiguous name *unit angle in circular measure*.

Hence, by Coulomb's law of the proportionality of the force of torsion to the twist given, we have for the couple corresponding to a deflection θ the value

$$\frac{\theta}{2\pi - a} H m l \sin a.$$

If then under the action of a current in the coil the deflection of the needle is θ, the equation of equilibrium is

$$C m \frac{2\pi}{r} l \cos\theta = m H l \left(\sin\theta + \frac{\theta}{2\pi - a} \sin a\right)$$

and therefore instead of (9) we have

$$C = \left(1 + \frac{\theta}{2\pi - a} \frac{\sin a}{\sin\theta}\right) \frac{H r}{2\pi N} \tan\theta \,. \quad . \; (13)$$

If a be an angle of say 1°, and θ be 45°, $\frac{\theta}{2\pi - a}$ is very nearly $\frac{1}{8}$ and $\frac{\sin a}{\sin\theta}$ is $\frac{1}{57\cdot3} \times \frac{1}{\cdot707}$ or $\frac{1}{40\cdot5}$. Hence

$$C = \left(1 + \frac{1}{324}\right) \frac{H r}{2\pi N}.$$

The error therefore is somewhat less than $\frac{1}{3}$ per cent.

The accuracy of the measurements of currents, made according to the method of which I have just given the theory, of course altogether depends on the careful adjustment of the standard galvanometer, and the care and skill of the observer. The standard galvanometer should be of such a form that the values of its indications can be easily calculated from the dimensions and number of turns of wire in the coil. Such a galvanometer can be made by any one who can turn or can get turned a wooden, or, preferably, brass ring with a rectangular groove round its outer edge to receive the wire. It is indeed to be preferred that the experimenter should at least perform the winding of the coil and the adjustments of the needle, &c., himself, to make sure that errors in counting the number of turns or in determining the length of the wire, or in placing the needle at the centre of the coil, are not made. The breadth and depth of this groove ought to be small in comparison with its radius, and each should not be greater than $\frac{1}{10}$ of the mean radius of the coil, which should be at least 15 cms. The size of the wire with which the coil is to be wound must be conditioned of course by the purposes to which the instrument is to be applied, but it should be good well insulated copper wire of high conductivity, and not so thin as to run any risk of being injured by the strongest currents likely to be sent through the instrument. For the exact graduation of current as well as potential galvanometers directly by means of the standard instrument, it is convenient sometimes to have two coils—one of comparatively high, the other of low resistance. The latter may very conveniently be a simple hoop of say 15 cms. radius, made of copper strip 1 cm. broad and 1 mm. thick. To form electrodes to which wires can be attached the ends of the strip are brought out side by side in the plane of the ring with a piece of thin vulcanite or paper between for insulator. Insulated wires are soldered to the ends of the circle thus arranged, and are twisted together for a sufficient distance to prevent any direct effect on the needle from being produced by a current flowing in them. In constructing a coil the operator should first subject the wire to a considerable stretching force, and then carefully measure its electrical resistance and its length. He should then wind it on a moderately large bobbin, and again measure its resistance. If the second measurement differs materially from the first the wire is faulty and should be carefully examined. If no evident fault can be found on the removal of which the discrepance disappears, the wire must be laid aside and another substituted. When the two measurements are found to agree the wire may then be wound on the coil. For this purpose the ring may either be turned slowly round in a lathe or on a spindle so as to draw off the wire from the bobbin, also mounted so as to be free to turn round. The wire must be laid on evenly in layers in the groove, and the winding ended with the completion of a layer. Great care must be taken to count accurately the number of turns laid on. The resistance should now be again tested, and if it agrees nearly with the former measurements the coil may be relied on. The ring carrying the coil thus made should now be fixed to a convenient stand in such a manner that if necessary it can be easily removed. The stand should be fitted with levelling screws so that the plane of the coil may be made accurately vertical. A shallow horizontal box with a glass cover and mirror bottom should be carried by the stand at the level of its centre. Within this the needle and attached index are to be suspended. The needle should be a single small magnet about a centimetre long, hung by a single fibre of unspun silk about 10 cms. long from the top of a tube fixed to the cover of the shallow box, so that the centre of the needle when the coil is vertical is exactly at the centre of the coil. To allow of the exact adjustment of the height of the needle, the fibre should be attached to the lower end of a small screw spindle, made so as to be raised or lowered, without being turned round, by a nut working round it above the cap of the tube. The needle should carry a thin glass index, about 6 inches long, made by drawing out a bit of thin glass tube at the blowpipe. In order that the zero position of the index may not be under the coil, the index should be fixed horizontally with its length at right angles to the needle, so as to project to an equal distance in both sides of it. To test that this adjustment is accurately made, draw a couple of lines accurately at right angles to one another on a sheet of paper. Then suspend a long thin straight magnet over the paper, and bring one of the lines into accurate parallelism with it. Remove then the magnet and put in its place the little needle and attached index If the index is parallel to the other line the adjustment has been carefully made. The needle may then be suspended in position and the box within which it hangs closed to prevent disturbance from currents of air.

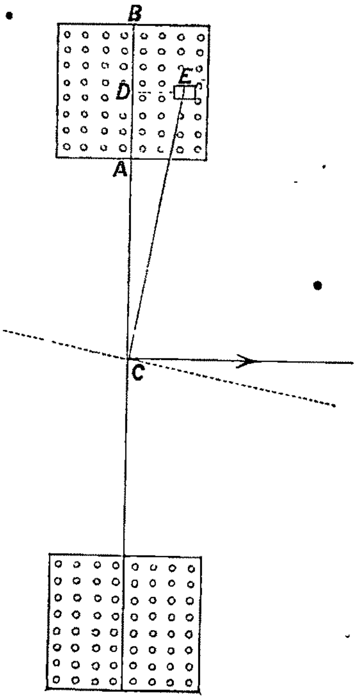

Fig. 3.

A circular scale graduated to degrees, with its centre just below the centre of the coil and its plane horizontal, is placed with its zero point on a line drawn on the mirror bottom of the box at right angles to the plane of the coil, so that when the needle and coil are in the magnetic meridian the index may point to zero. The accuracy of the adjustment of the zero point is to be tested by finding whether the same current produces equal deflections on the two sides of zero. To test whether the centre of this divided circle is accurately under the centre of the needle supposed at the centre of the coil, draw from the point immediately under the centre of the needle two radial lines on the mirror bottom, one on each side of the zero point and 45° from it, and turn the needle round without giving it any motion of translation. If the index lies along these two radial lines when its point is at the corresponding division on the circle the adjustment is correct.

When taking readings the observer places his eye so as to see the index just cover its image in the mirror bottom of the box, and reads off the number of degrees and fraction of a degree, indicated on the scale by the position of the index. Error from parallax is thus avoided.

A mirror with attached magnets may be used, as in the magnetometer, instead of the needle and index. When this arrangement is employed the coil is in the magnetic meridian, when equal deflections of the spot of light on the scale on the two sides of zero are observed. These scales, as has been already remarked, should always be carefully glued to a wooden, piece instead of being, as they frequently are, fixed with drawing pins.[1]

ANDREW GRAY

(*To be continued.*)

PROFESSOR HENRY DRAPER, M.D.

THE late Professor Henry Draper, whose death we announced last week, was born in Virginia in 1837, but three years later removed to New York, at the time when his father, Prof. J. W. Draper, was appointed to the Chair of Chemistry in New York University. At this University Dr. Draper was educated, graduating in Medicine in 1858, after which he travelled abroad. In 1860 he was elected to a professorship in his own University, which he retained till his death the other day. In 1866 he was elected Professor of Physiology in the Medical Department of the University and managing officer of the institution, a position he resigned in 1873.

Dr. Draper's scientific work began with a series of experiments in 1857 on the function of the spleen, carried out by the aid of microscopic photography, an art then in its infancy. On his return from Europe, stimulated by a visit paid to Lord Rosse's 6-foot reflector, he began the construction of a 15½-inch reflecting telescope, and with this, when completed, he took photographs of the moon. A full account of the methods of grinding and polishing reflecting mirrors and the system of testing them was printed in 1864 in the Smithsonian "Contributions to Science."

Dr. Henry Draper subsequently constructed an equatorial reflecting telescope of 28 inches aperture, making both the mounting and the silvered glass speculum himself. The object for which this instrument was intended, and which it succeeded in accomplishing in 1872, was photographing the spectra of the stars, a work which has been carried on with such success by Dr. Huggins in this country. Since the invention of the gelatino-bromide dry process the difficulties of this research have much decreased; all the more credit is therefore due to Draper and the other pioneers in this branch of inquiry; he had taken more than a hundred spectra of various stars.

In 1872 Dr. Draper produced a photograph of the diffraction spectrum of great excellence. It comprised the region from below G, wave length 4350, to O, wave length 3440, on one plate.

In 1874 Draper was appointed by the United States Transit of Venus Commission, Superintendent of its Photographic Department, and his duties in this connection were so satisfactorily performed, that in the fall of that year the United States Government caused a special gold medal to be struck in his honour at the Mint in Philadelphia, bearing the inscription, "Decori Decus Addit Avito." This was the first time that such a public recognition had ever been accorded to a scientific man in the United States by the Government.

In 1877 Dr. Draper printed his paper on the "Discovery of Oxygen in the Sun and a New Theory of the Solar Spectrum." This research has given rise to as much interest as any in recent times; whatever the future verdict may be upon it, it was the result of several years' work and most costly and elaborate apparatus. In 1877 Dr. Draper went to the Rocky Mountains, and made experiments on the transparency and steadiness of the atmosphere at elevations up to 11,000 feet. In the succeeding summer he took a party into the same region to observe the total eclipse of the sun, and was fortunate enough to photograph the diffraction spectrum of the solar corona, which on this occasion was shown to be continuous.

During the last autumn and winter he took photographs of the nebula in Orion. These were the first he ever made, and required an exposure in the telescope up to 140 minutes, even when the most sensitive of Eastman's gelatine plates were used.

Dr. Draper's work has been done mainly at his observatory at Hastings-on-Hudson, and at his laboratory in New York. In the former he had three large telescopes.

Dr. Draper's genial nature won him many friends and many English men of science well know the hospitable home at Dobb's Ferry. These and many more will sympathise with Mrs. Draper in the loss which not only she but science has sustained in the death of so earnest a seeker after truth.

THE COMET

WE have received the following communications on this subject:—

The latest information indicates that the September comet was first seen on the 3rd of that month at Auckland.

The sketch, No. 1, represents the appearance of the spectrum of this comet on October 15 and 16, and subsequent mornings. The spectroscope used was one of Browning's direct-vision, with five prisms. It was attached to the comet-seeker, which has a 4-inch object-glass, the focal length of the instrument making a distinct general view of the spectrum easy. As the spectroscope was not furnished with any means of comparing spectra, the positions of the bands, as shown in the sketch, were obtained by adjusting the viewing telescope so that each band was, in succession, just in the edge of the field, clamping the telescope, and then viewing the spectrum of a candle. This operation was repeated several times on October 16, and subsequently on the 25th. The position of the band in the orange-yellow was referred directly to the sodium line in the candle-flame. The band in the middle of the green was much the brightest, and on the least refrangible side was sharply defined; but, in the other direction, gradually diminished in brightness. When the slit of the spectroscope was gradually closed, the light was gradually diminished, but no separate line made its appearance, as the well-defined edge of the band would have led one to expect.

The other two bands were of about equal brightness; both of them fading rapidly on the more refrangible side, but much more slowly in the other direction.

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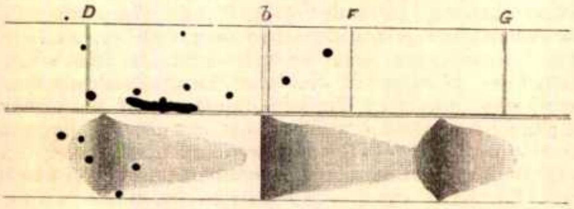
[1] ERRATUM.—In the preceding part of this article, p. 30, col. 1, line 24 from top, for $2r$ read r.

It will be remembered that the first spectroscopic observations, by M. Thollon, at Nice, reported the spectrum of the nucleus as continuous, very brilliant, and much extended toward the violet. The head gave the sodium lines very brilliant, clearly double, and appearing displaced toward the red. This report was confirmed on the same day by a similar one from Mr. Lohse, with the additional remark, that he saw *many bright lines*, the sodium being the brightest, and all apparently displaced toward the red.

October 15. This state of things had entirely changed. The change had probably been gradual, and was dependent upon the distance of the comet from the sun.



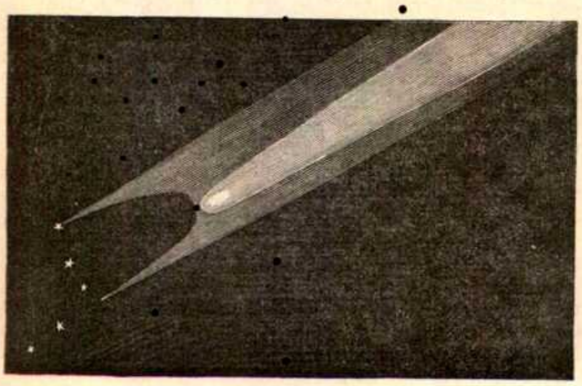
No. 1.—Spectrum of Crul's Comet, October 15 and 16.

The first observation made on September 18, when the comet was near the sun, gave a continuous spectrum, which was due to the strong reflected light, while the bright lines were due to the vapours developed by the intense heat of the sun.

On October 15 the spectrum resembled the spectra of the comets of 1868; of the sodium line there was no trace, although the spectrum contained light of about the same refrangibility. The tail of the comet gave a faint but apparently continuous spectrum brightest in the green.

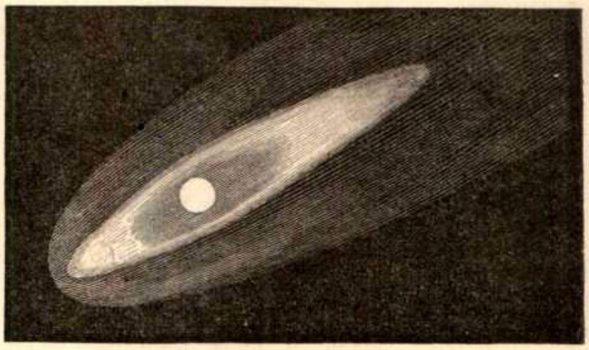
A somewhat similar change was observed, I believe, in the spectrum of the Comet Wells as it approached the sun, except that its nucleus gave always a continuous spectrum, to which was added the sodium line as the comet neared the sun. If we are soon to witness a return of the September comet, it is desirable that many observers should be prepared to watch the changes as the comet approaches the sun.

Sketch No. 2 represents the comet as it appeared in the comet-seeker on the morning of October 10, which was particularly clear. This outer envelope I first noticed on the morning of the 8th, when I traced it far beyond the head of the comet in the direction of the sun, but only on the east side. On the 10th it appeared as represented in the sketch. The outer edges were perfectly sharp and parallel to the axis of the comet, thus forming a cylinder whose diameter was about four times the diameter the tail measured several degrees from the head. When I again looked for this envelope on the 25th, it could be traced only on the east side, but retained the same relation to the tail. The greatest length to which this attained was about four degrees beyond the head.



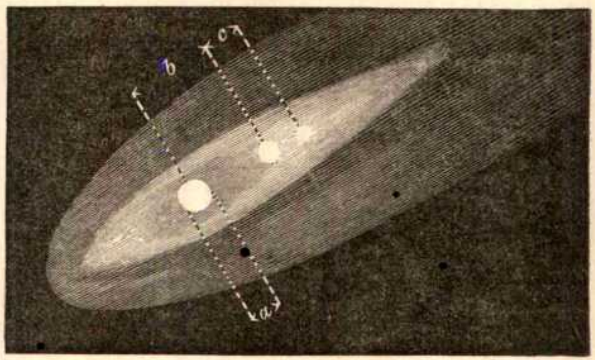
No. 2.—October 10, 1882.

Sketch No. 3 represents the head of the comet as it appeared on October 25 in the 26-inch equatorial. Owing to the low altitude of the comet, this instrument had not been used before. The head appeared more elongated than at any time before but instead of the uncertain, delusive appearance which it presented in the 10-inch equatorial, the image brought out the peculiarities of each portion of the head, and left little doubt that there was, at that time, an essential difference between the central portion or nucleus proper, and the other two portions. The central part was circular, of considerable apparent diameter, and of quite uniform brightness throughout. The other two portions were irregular in shape, much less bright than the nucleus, and their brightest parts were also irregular in shape. Both were apparently separated from the nucleus, though on its side they were joined to each other. The portion extending in the direction of the tail was the longest and brightest.



No. 3.—Nucleus of Comet, October 25, as seen in 26-inch Equatorial.

The next opportunity to examine the comet was on November 3, when it had decidedly changed in appearance, as represented in No. 4. The nucleus remained as when last seen, as did also the portion of the coma nearest the sun. The other portion showed a circular condensation almost as distinct as the original nucleus, and about two-thirds its size; still further in the direction of the tail was another condensation, smaller and less distinct than the second. On November 6 these condensations were still more pronounced, and shone with a much stronger light than the coma in which they were enveloped.



No. 4.—Nucleus of Comet, Nov. 6, 1882, as seen in 26-inch Equatorial.

The following micrometric measurements were made on the 3rd and 6th, using the 26 in. equatorial:—

$$\left.\begin{array}{r}\text{Distance } a = 0''{\cdot}66\\ ,,\quad b = 1{\cdot}40\\ ,,\quad c = 0{\cdot}84\end{array}\right\}\text{ November 3.}$$

$$\left.\begin{array}{r}\text{Distance } a = 0{\cdot}74\\ ,,\quad b = 1{\cdot}66\\ ,,\quad c = 1{\cdot}03\end{array}\right\}\text{ November 6.}$$

Assuming 0·1705 and 0·1714 to be log. Δ on those days respectively, the distance *a* would be about 4000 miles, the distance *b* about 10000 miles, and the distance *c* about 5500 miles.

It would not be surprising, judging from the history of this comet, if another condensation developed in the portion of the coma nearest the sun, thus forming four nucleuses.

W. T. SAMPSON,
Commander U.S.N.

Naval Observatory, Washington, November 11

Since my first communication, with sketch of the comet, on October 21, which appeared in NATURE, vol. xxvi. p. 622, I have had good views on 21 out of 31 days. The fine weather and clear atmosphere of this place give exceptional facilities for the continued and frequent observations which are needed to obtain a knowledge of so anomalous and surprising an object. Some windows of my villa command an extensive sky and sea view (including at times the mountains of Corsica, 120 miles distant), and from my bedroom—sometimes even from my bed—I have been able to watch the comet with ease for from a quarter of an hour to an hour, on each of those twenty-one days; using only a good field binocular in occasional aid of a strong natural sight. I have more powerful telescopes, but for this object they give no help; and I am not astronomer enough to avail myself of other instruments.

The comet was seen in all its brightness on October 20, 21, 23, and 24, with its nucleus like a star of first magnitude, but elongated and nebulous—its tail beginning with slender stem, slightly curved, with downward convexity, and gradually expanding to its extremity, the diameter of which was about five times that of the head. The lower, slightly convex margin, was brighter, and more defined; but a strong nebulous light pervaded the length and breadth of the tail, shaded along the upper margin in gradually diminishing haze. The tail ended in an elongated crescent, the lower or eastern horn of which was longer than the other. Both horns were prolonged in faint lines, hardly perceptible, a few degrees further (as noticed by your correspondent, Mr. Larden). No such prolongation could be seen from the hollow of the crescent, which terminated by a narrow fringe of diminishing light, beyond which was an oval patch of shade, *obviously darker than any other portion of the visible sky*. This appeared to me nothing else than a *shadow* projected by the comet on the space beyond the end of its tail. I cannot admit the correctness of Major Herschel's *suspicions*, "that this impression was produced by contrast only" (NATURE, vol. xxvii. p. 4). The still greater contrast between the brightness of the lower margin and the adjoining sky produced no such shade there *at that time*: later I shall allude to such a shade appearing there also. The ultra-caudal patch was obviously darker than any other spot of the sky: so it appeared to me, and my experience in landscape painting has given me some skill in appreciating lights and shades. I am quite aware of the difficulty of physically explaining the existence of light and shadow in the vacuity of space, but this is a question of pure observation, to which I invite further attention. Two of your correspondents, Mr. Larden of Cheltenham, and Mr. Cecil of Bournemouth, describe "a black rift in the sky," and "a strong apparent shadow" behind the comet—seemingly in confirmation of my observation.

When the comet was next seen, after an interval of bad weather, on the 29th it had lost in dimensions, but still more in brightness, and its form was changed. The upper margin from the head upwards had expanded and become more feathery; so had the end of the tail, which had lost its crescentic form; the shadow beyond had quite disappeared, and was replaced by an ill-defined luminosity, losing itself in the darkness of the sky. The lower margin of the tail had lost less of its brightness and definition; and now if there was a shadow anywhere, it was along this edge, down even to the head of the comet; but the shade was much less marked than had been that beyond the tail, and I might have ascribed it to contrast but that it was not present when this margin was brighter and the contrast greater. This shadow is noticed by Mr. Cecil in NATURE, vol. xxvii. p. 52.

The comet was well seen on October 30 and 31, and November 2, 3, 4, 6, and 7, gradually diminishing in brightness and in the definition of its outline, its light being now further paled by moonlight. So faint was it that I am not surprised at Major Herschel's description of its non-appearance in the London [illegible] of November 5; but I cannot help "suspecting" that this was due not to moonlight only (as the testimony of others proves), but also to the gas-lit haze of the London atmosphere, which from fifty years' experience I know to be, at its clearest, quite sufficient to mask a faded comet, even although the brighter light of stars may still remain visible. On the 8th the comet was seen before moonrise, more distinct, although pale and hazy in outline; lower margin still the brightest, with a slight attendant shade. It was seen every day (except the 13th, 14th, and 15th) until the 22nd, with little other changes than that it was gradually becoming fainter, although still a conspicuous object in the dark sky from 2 to 5.30 a.m. On the 21st I made a careful portrait of it in oils, with its attendant stars, by the side of one that I had painted from the sketch taken October 21, when it was in its glory. The alteration which has taken place in the month is such that it now seems the mere ghost of its former self. The comparison strikes one as showing how much more it has lost in brightness and compactness, than in length and breadth. Is not this in exact conformity with what has been ascertained (see NATURE, vol. xxvii. p. 58) that the comet has been receding more rapidly from the sun than from the earth.

C. J. B. WILLIAMS

Cannes, November 23

THE APPROACHING ECLIPSE OF MAY 6, 1883[1]

THE sixth of May next year will witness, in the distant regions of Oceania, one of the rarest and most important astronomical phenomena of the century, viz. a total eclipse of the sun, which, owing to the respective positions, but rarely realised, of the sun and the moon, will have a duration quite extraordinary.

Now, in the present state of science, when the most important questions as to the constitution of the sun and that of the unexplored spaces near him, and the existence of those hypothetical planets which Le Verrier's analysis indicated within the orbit of Mercury, are still pending, a phenomenon which presents to us, for long minutes, all those regions, with the sun's dazzling brilliancy withdrawn, and renders them accessible to observation, is one of the first order.

We shall presently examine the conditions under which this rare solar occultation will be produced; let us first see what is the state of the questions which have to be considered on this occasion. One of the most important is that regarding the constitution of the space immediately bordering on the envelopes of the sun at present known.

The great Asiatic eclipse of 1868, came wonderfully *à propos*, both by its long duration and by the maturity of the problems that had to be attacked, enabled us in some sort to tear the veil which hid from us the phenomena existing beyond the visible surface of the sun. It was then that was solved the enigma so long pondered over regarding the nature of those roseate protuberances which surround in such a singular way the limb of the eclipsed sun.

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[1] Report to the Bureau des Longitudes, by a Commission consisting of MM. Fizeau, Admiral Cloué, Lœwy, and Janssen (reporter).

Spectral analysis taught us that they were immense appendages belonging to the sun, and formed almost exclusively of incandescent hydrogen gas. Almost immediately, the method suggested by this same eclipse, and which allows of a daily study of those phenomena, revealed the relations of those protuberances to the solar globe. It was perceived that the protuberances are merely jets, expansions of a layer of gas and vapours, 8″ to 12″ in thickness, where the hydrogen preponderates, and which is at a very high temperature, by reason of its contact with the surface of the sun. This low atmosphere is the seat of frequent eruptions of vapours coming from the solar globe, and among which one chiefly observes sodium, magnesium, and calcium. We may even suppose that, in the lowest part of this *chromosphere*, as it has been called, most of the vapours, which in the solar spectrum, produce the dark lines it presents, exist in the state of high incandescence.

The eclipse of 1869, which was visible in America, allowed indeed of the important observation (always confirmed since) of the reversal of the solar spectrum at the extreme border of the disc, that is to say, at the points where the photosphere is immediately in contact with the chromosphere; a phenomenon which does not signify that the photosphere itself may not contain the same vapours and concur in the production of the solar spectral lines.

Thus the discovery of a new solar envelope, the recognised nature of the protuberances, and the knowledge of their relation to the sun; lastly, the conquest of a method for the daily study of those phenomena; such were the fruits of spectrum analysis applied to the study of this long eclipse of 1868.

But a total eclipse presents other manifestations completely unexplained up to the time of which we speak. There is seen, beyond the protuberances and the chromospheric ring, a magnificent aureole, or luminous corona, of soft brightness and silvery tint, which may reach as far as an entire radius of the dark limb of the moon.

The study of this beautiful phenomenon, by methods which had given such magnificent results, was immediately undertaken, and occupied the astronomers during the eclipses of 1869, 1870, and 1871.

But the aureole or corona, though constituting a brilliant phenomenon, has in reality but weak luminous power. Hence the difficulty of obtaining its spectrum with its true characters. Thus the astronomers differed at first as to the real nature of the phenomenon. In 1871, and by the use of an extremely luminous instrument, it was definitively proved that the spectrum of the corona contains the bright lines of hydrogen, and the green line called 1474 of Kirchhoff's maps, an observation which demonstrates that the corona is a real object constituted of luminous gases forming a third envelope round the solar globe.

If indeed the phenomenon of the corona were a simple phenomenon of reflection or of diffraction, the coronal spectrum would merely be a weakened solar spectrum. On the other hand, the characters of the solar spectrum are here quite subordinate, and the spectrum is that of protuberantial gases and of matter still unknown, indicated by the line 1474.[1]

The subsequent eclipses of 1875 and 1878, and that recently observed in Egypt, have yielded confirmation of these results.

But if the constitution of the sun is being thus rapidly unveiled, there still remain great problems to be solved, both as to this last solar envelope, and as to the region near it.

First of all, have the immense appendices which the corona has presented during some eclipses, an objective reality, and are they a dependance of this immense coronal atmosphere, or might they rather be streams of meteorites circulating round the sun (as one of the members of the Bureau has suggested)?

We do not forget the zodiacal light, the relations of which to those dependances of the sun remains to be determined.

But these problems are not the only ones we have now to attack, during the occultations of the solar globe. Do the regions with which we are occupied contain one or several planets, which the illumination of our atmosphere, so bright in the neighbourhood of the sun, may have always concealed from us? Leverrier long studied this question, and his analytical researches led him to suppose their existence.

On the other hand, several observers have alleged that they have observed transits of round and dark bodies in front of the sun; but these observations are far from being certain. The surface of the sun is often the seat of small, very round spots, which appear and disappear in a time often short enough to simulate the passage of round bodies before that star.

The question is of capital importance; hence it at present justly engages the thoughts of all astronomers.

May the analysis of Leverrier enrich the solar world towards its central regions, as it has done with such a magnificent success in the most distant regions?

We have but two means of solving the problem, whose solution is more particularly incumbent on French astronomy; the attentive study of the solar surface, or the examination of the circumsolar region when an eclipse renders their exploration possible to us. This last means seems the most efficacious, but on the condition that the occultation is long enough to allow of a minute exploration of all the regions where the small star may be met with.

This gives a capital importance to the eclipse of May 6 next, one of the longest of the century.

We will now examine the circumstances of this great eclipse, and the means that it would be well to employ for observation of it.

The total eclipse of May 6 next will have a duration of 6 minutes at the point where the phase is maximum (5m. 59s.); a time triple that of ordinary eclipses.

The central line is wholly comprised in the South Pacific Ocean, and we can only hope to observe it in the islands of that ocean.

After an attentive study of the question, it has appeared to us that two islands would do about equally well for observation; those of Flint and Caroline.

Flint Island (lat. 11° 30′ S., and long. 151° 48′ W. of Greenwich) is the nearest to the central line. Calculation gives for the duration of totality in this island 5m. 33s. Caroline Island is 150° 6′ W., and 9° 50′ S.; the duration of the totality there will be 5m. 20s.; that is, only 13 seconds less than in Flint Island.

It will be seen that the astronomical conditions of the phenomenon are extremely favourable in these islands, and it is to these stations we should propose to the Bureau to send an expedition.

This expedition should start from Paris, go to New York, traverse the American Continent by the railway to San Francisco, and there find a steamer (of a French service about to be established), which should carry it to the Marquesas Islands. There a man-of-war of the French station should take it up, and deposit one portion at Caroline Island, the other at Flint Island. This ship which, further, should be provided with all that is necessary for the establishment of the stations, the safety and the subsistence of the observers, should not leave those regions before bringing the mission to Tahiti, where our *envoyés* would find means of transport for their return, either by the way they went, or (which would seem preferable), by way of Australia.

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[1] One of us has expressed the idea (Notice du Bureau des Longitudes, 1879) that the coronal atmosphere which is in dependence on the chromosphere and photosphere must present a much more agitated appearance at the epoch of maximum of spots and protuberances, since the jets of hydrogen which then penetrate it are much more numerous and rich Ulterior observations, and especially those which have been made during the last eclipse at the moment when the solar eruptions were abundant, have confirmed this prevision.

THE TRANSIT OF VENUS ON WEDNESDAY, DECEMBER 6

At the Transit of Venus in 1874 the tables of the planet prepared by Prof. Hill appeared to have a decided advantage over those of Leverrier. The correction to the tabular place deduced from the observations of the transit is in close accordance with that shown by a meridian observation at Washington on the day preceding the phenomenon. Although the entire discordance was not negatived by the tables of Prof. Hill, they went far towards removing it in 1874, and as the coming transit (December 6) will take place in nearly the same point in the planet's orbit, we shall assume in what follows, that the tables of the American astronomer will again be fairly correct. Prof. Newcomb assumes and probably with much reason, that the error of Leverrier's tables will prove to be an increasing one, and is therefore inclined to apply a still larger correction to the place deduced from them. It may be mentioned that the calculations of the transit in the *Nautical Almanac*, the *Connaissance des Temps*, and the *Berliner Astronomisches Jahrbuch*, depend upon Leverrier's tables. For the diameter of the planet we adopt that found by Prof. Auwers from heliometric measures in Egypt during the last transit, combining it with the diameter of the sun, inferred by Leverrier from his discussion of the transits of Mercury.

Direct calculations for Greenwich, Edinburgh, and Dublin give with the elements so obtained the following *Greenwich* mean times of the first external contact, and the respective angles from the sun's vertex for direct image:—

	h	m.	s.	°	′
Greenwich	2	0	42·2	126	59·4
Edinburgh	2	1	7·7	130	49·3
Dublin	2	1	9·6	131	21·2

For a limited area like that of these islands we may apply to these times and angles, the method of distribution of predictions given by Littrow, and subsequently by Woolhouse. Putting the latitude of any place within the above area $= 50° + L$, and its longitude in minutes of time $= M$, + if *east* of Greenwich, − if *west*, we get the following equations:—

G.M.T. of first external contact ... $= 2\text{h. } 0{\cdot}62\text{m.} + [8{\cdot}7453]\,L - [8{\cdot}1402]\,M.$

Angle from sun's vertex, direct image... $= 126°{\cdot}3 + [9{\cdot}669]\,L - [9{\cdot}136]\,M.$

The quantities within the square brackets are logarithms, but of course if preferred the factors for L and M may be expressed as numbers. As an example of the application of these formulæ, suppose the time of first contact and the corresponding angle are required for Norwich, the position of which place may be taken in latitude + 52° 38′, longitude 1° 18′ or + 5m. 12s., we have then

$$L = +\,2°{\cdot}633 \quad M = +\,5{\cdot}20\text{m.}$$

	+ 8·7453		− 8·1402
Log. L ...	+ 0·4205	Log. M ...	+ 0·7160
	+ 9·1658		− 8·8562
No.	+ 0·146	No.	− 0·072
	− 0·072		
	2 0·62		
	2 0·69	or	h. m. s 2 0 41 G.M.T.
	Longitude E		5 12
			2 5 53 Norwich M.T.

For the angle—

	+ 9·669		− 9·136	+ 1°·23
Log. L	+ 0·421	Log. M	+ 0·716	− 0·71
				126·3
	+ 10·090		− 9·852	126·8 ... angle from vertex.

So that according to the calculation the limb of the planet comes into first contact with that of the sun at 53° from his lowermost point towards the left, as we view the phenomenon with the naked eye. It will be remarked that there is less than a half minute difference in absolute time between Greenwich and Dublin, and considering the possibility of error of many seconds in any prediction that can be made for geometrical contact and the difficulty of always determining what is geometrical contact in the observations, our formulæ for time of first contact is more than a sufficient one.

For first *internal* contact, it may be assumed that 21 minutes have to be added to the time of external contact at any place in these islands; while for the angle from N. point of first external contact may be taken in all cases 147°.

In the national ephemerides the times of the contacts are given for a particular meridian as they would be noted at the centre of the earth, and formulæ are appended to reduce these geocentric times to any point upon the earth's surface. It is obvious that where, as in a transit of Venus, predictions are required for such widely distant stations, this method possesses the greatest convenience.

NOTES

The following are the Lecture arrangements at the Royal Institution for the ensuing Session.—The Christmas Lectures will be given by Prof. Tyndall, on Light and the Eye. Before Easter—Prof. W. C. Williamson on the Primeval Ancestors of Existing Vegetation, and their Bearing upon the Doctrine of Evolution; Prof. R. S. Ball, four lectures on the Supreme Discoveries in Astronomy; Prof. Dewar, nine lectures on the Spectroscope and its Applications; Mr. R. Bosworth Smith, four lectures on Episodes in the Life of Lord Lawrence; Dr. W. H. Stone, three lectures on Singing, Speaking, and Stammering; Mr. H. H. Statham, two lectures on Music as a Form of Artistic Expression. After Easter—Courses will be given by Professors Tyndall, McKendrick, A. Geikie, and Turner (of St. Petersburg). The Friday Evening Discourses will probably be given, among others, by Mr. R. B. Smith, Dr. G. J. Romanes, Sir W. Thomson, Mr. M. D. Conway, Prof. W. C. Williamson, Mr. W. H. Pollock, and Prof. Tyndall.

A committee, consisting of the Right Hon. J. T. Ball, LL.D., D.C.L., the Very Rev. W. Reeves, D.D., Dean of Armagh, J. L. E. Dreyer, Ph.D., Astronomer of Armagh Observatory, has been appointed by the Governors of the Armagh Observatory to raise a fund for the purpose of erecting a memorial instrument in the observatory at Armagh, where the late Rev. Dr. Robinson spent fifty-eight years, engaged in those scientific investigations with which his name will be for ever associated. The Committee addresses its appeal not only to the inhabitants of Ulster, or of Dublin, but to Robinson's friends and admirers all over the United Kingdom. The services rendered to astronomy by Dr. Robinson are well known, and doubtless many of our readers will be glad to aid in paying a tribute to his memory. It is proposed that the memorial take the form of an equatoreal refractor, say of eight or nine inches aperture, which could be had for about 500*l.*, and could find room in one of the existing domes at Armagh. With such an instrument, much valuable and interesting work could be done. Subscriptions should be sent to Dr. J. L. E. Dreyer, Observatory, Armagh.

WE regret to announce the death, on November 24, of Mr. Andrew Pritchard, M.R.I., F.R.S. Edin., &c., of Highbury, London, whose name will be best remembered in connection with several improvements of the microscope, the use of "test objects," and as being the author of "A History of Infusoria," the fourth edition of which, enlarged to nearly 1000 pages, was published in 1861. Born in London in December, 1804, he was almost entirely brought up by his grandfather, one of the chief cashiers in the Bank of England. On the foundation of the Mechanics' Institution in Southampton Buildings, by Dr. Birkbeck, Mr. Pritchard entered as a student. The microscope was then a very imperfect instrument, and Mr. Pritchard worked hard at the achromatisation of lenses, and was the first to propose to take advantage of the high refracting power of the diamond, ruby, and sapphire for the manufacture of single lenses, these giving good definition without the coloured borders incidental to ordinary flint glass. Between the years 1829 and 1837 he published several works on the microscope, in which he was aided by Dr. Goring, particularly the "Microscopic Illustrations," "Micrographia," and the "Microscopic Cabinet," for which several good plates were prepared. In the year 1836 Mr. Pritchard was elected a Member of the Royal Institution, being proposed by Faraday, and in the previous year joined the British Association at Dublin, taking part in the deliberations of this body until comparatively recent times. In 1873 the Royal Society of Edinburgh conferred upon him their fellowship, in recognition of his scientific attainments, as evidenced by his great work, the "History of Infusoria," a memorial of patient industry and biological research.

THE Lancashire friends of the late Prof. Jevons are to hold a meeting at the Manchester and Salford Bank on Thursday next, to consider a proposal for a Jevons memorial. It has been suggested that an appropriate form of the memorial would be the establishment of a Professorship of Political Economy at the Victoria University, Manchester. Prof. Jevons was a Lancashire man, and was associated for many years with the Owens College and with the Manchester Statistical Society.

MR. BARNARD, of Nashville, Tennessee, and Prof. Wilson, of the Cincinnati Observatory, both noticed that the nucleus of the comet had separated into three fragments on the morning of October 5. While this separation was not observed at other observatories, probably owing to cloudy weather, we learn by the last steamer from Central America, that on the same morning the comet, as visible to the naked eye, at Escuintla, Guatemala, was divided into five distinct bodies, thus leading many to suppose that a whole family of celestial visitants had suddenly appeared. Subsequent observations in different parts of the world have led to the belief that the fragments were re-united. This statement appears in the *Panama Star and Herald*.

THE transit of Venus, on December 6, will be observed at Paris with the heliostat in several places, to exhibit the phenomenon to a large audience. M. Joubert, director of the Observatoire Populaire of the Trocadero, is taking steps for that purpose, and will send out special invitations. Lectures will be delivered during the transit. M. Janssen, before leaving for Oran, left instructions for similar observations to be exhibited before a number of visitors at Meudon Observatory. A requisition has been sent to M. Bouteiller, the president of the Municipal Council, asking him to order that the leading pupils of public schools and their principal teachers should be invited to Montsouris Observatory in order to witness the transit.

WE are glad to learn that Prof. Mendeleeff has published a new edition (the fourth) of his "Principles of Chemistry." The new edition is thoroughly revised, and contains many important additions and modifications, bringing it up to the latest data of science. The high standard of this book is well known. The aims the author has pursued may be seen in the following words of his preface: "By comparing the past of the science with the future, the particulars with the generalisations, and our necessarily limited experience with our natural tendency towards the infinite, and by refraining from asking the student to accept without test any doctrine, however attractive, I tried to develop in the reader the faculty of independent judgment on scientific subjects which is necessary for a true use of science, and for acquiring the possibility of working for its further development." The work may be regarded as not merely a text-book of chemistry, but an exposition of the methods of natural science altogether.

ALGERIA is becoming increasingly popular as a winter resort for invalids affected with chest disease; but probably not many of our readers are aware that in the same easily accessible country gout and rheumatic patients may find what is scarcely to be met with in Europe, a comfortable residence with abundance of waters adapted to their special complaints. At Hammam R'Irha, about sixty miles south-west of Algiers and fifteen miles in a direct line from the coast, such patients will find waters both for bathing and drinking comparable with those of the best European resorts, and in addition a climate which renders outdoor exercise a pleasure all the winter through. Hammam R'Irha is beautifully situated among the hills of an outlying spur of the Lesser Atlas, and we understand has every possible convenience and comfort that invalids can require. Naturally enough the people of Algiers look with some jealousy on this pleasant spot as a rival, and attempt we believe to ignore it; but in the opinion of the highest authorities on the subject of climate and waters, no place can equal Hammam R'Irha as a winter resort for gout and rheumatic patients. As it becomes better known we are sure it will grow in favour, especially with English and Americans, who will find on the spot competent medical advice. The station is within three or four hours' rail of Algiers.

THE remarkable phenomenon which was seen on Friday week in several parts of this country, was also seen in Sweden. At Eskibstuna, 54 miles south of Stockholm, it was observed three hours after sunset in the western heavens, it being dark at the time, about 45° above the horizon, and was then hidden in a lurid cloud of purple colour. When approaching the zenith an oblong object, somewhat resembling a bow, became distinctly visible, which gradually passed out of sight. The stars were visible through the object. The moon in her first quarter shone faintly in the south, 45° elevation above the horizon, while heavy clouds covered the eastern and northern skies. Auroræ were frequent and intense all over Scandinavia during the week.

HERR BERNHARD BLECHMANN, a pupil of Prof. Stieda, of Dorpat, has been making researches on the anthropology of the Jews. He took 100 Jews of West Russia and the Baltic Provinces, and as a result of his observations, he finds that there are both blonde and dark Jews of the primitive type, that Jews have narrower chests than Europeans under similar conditions; that there are two types, Spanish and Germano-Polish; and that they appear to be brachycephalic.

THE third German "Geographentag" will be held at Frankfort on March 29-31, 1883. As at former meetings, both the scientific and educational aspects of geography will receive attention, and intending contributors of papers should communicate with Prof. Rein, Marburg, before the end of January. There will, as usual, be an exhibition of teaching *matériel* in geography, which will be open for two or three weeks.

IMMENSE forest fires are reported from the neighbourhood of St. Petersburg. Near Pawlowsk and the villages Skolpino, Stepanowka, and Podberesche near Gatchina, then along the

Warsow railway line between Pljusse and Pleskau, also along the Moscow Railway line the forest was on fire. Thousands of people had been ordered out to try and extinguish the flames, but all attempts in this direction proved futile, the only thing that could be done was to confine the limits of the fires.

Dr. King, the Superintendent of the Royal Botanic Gardens, Calcutta, has recently issued his report for the year 1881–82. The Calcutta Garden may be said to be the centre of botanical work in India, and none can probably claim a greater antiquity, as the report before us is stated to be the ninety-fifth annual report of these Gardens. Like its predecessors the report opens with a description of the changes and improvements in the Garden itself, points which are, of course, only of local interest. On the subject of india-rubber yielding plants—a subject of very great importance—Dr. King says: "Clara rubber (*Manihot Glaziovii*) continues to grow well here; our trees are beginning to seed, and from their produce I was able to distribute during the year a good many seedlings to tea-planters in Assam, Chittagong, and elsewhere. A species of *Landolphia*, which is one of the sources of the rubber collected in Eastern Africa, has (thanks to the exertions of Sir John Kirk, Her Majesty's Consul-General at Zanzibar) been introduced to the Garden. From the seeds sent by Sir John Kirk a number of young plants have been raised, and these at present look very healthy. The cultivation of the plant yielding Para rubber (*Hevea brasiliensis*) has been abandoned, as the Bengal climate proves quite unsuitable for it. Of *Castilloa*, another South American rubber-yielder, we have as yet only eight plants, but it is being propagated as fast as possible." Another important subject is that of the production of materials for paper-making, and of these plantain fibre seems to have occupied some attention. It seems that during the dry months, simple exposure of the sliced stems to the sun is a sufficient preparation for the paper-maker, provided the paper-mill be on the spot. What is still wanted is some cheap mode of removing the useless cellular tissue, so that the fibre may be shipped to England without the risk of fermentation during the voyage. The cultivation of the plantain for its fruit is so universal over the warmer and damper parts of India, and its growth is so rapid, that the conversion into a marketable commodity of the stems at present thrown away as useless would be an appreciable addition to the wealth of the country. The paper mulberry of China and Japan (*Broussonetia papyrifera*) is being tried in the Garden, as well as in the Cinchona plantations in Sikkim, as it is well known that the bark yields a splendid paper material. A plant which appears to be at present unknown, but which Dr. King thinks will prove a species of *Eriophorum*, is also favourably reported upon. Under the head of "Other Economic Plants," mahogany, the rain-tree, and the Divi Divi, are said to be in considerable demand. A large interchange of seeds and plants has been effected during the year, with other parts of India, as well as with England and the Colonies.

No further news of the wreck alleged to have been seen near the Island of Waigatz has come to hand. Capt. Burmeister, of the *Louise*, who parted from the *Dijmphna* and the *Varna* in September last, is of opinion that the vessel seen is the *Varna* in her winter-quarters, simply with masts and yards lowered, which seem to be corroborated by the recent discovery, that the original message says *west* of Waigatz Island, where the wreck could not have drifted.

Parts 11 to 16 of Dr. Chavanne's edition of Balbi's Geography (Vienna, Hartleben) have appeared; they are largely devoted to the Austro-Hungarian monarchy.

We need scarcely mention that Oxford is the seat of the New Science Club, to the meeting at Trinity College in connection with which we referred last week.

In the last sitting of the Syndicat d'Électricité M. Jablochkoff described a new element which he has invented, and which consists of sodium for the electro-positive plate, the negative being, as usual, carbon. M. Jablochkoff does not use any exciting liquid but merely sends into his elements by the instrumentality of an aspirator, a current of air saturated with moisture. He says that soda is dissolved and falls to the bottom of the box where his elements are kept so that it may be easily collected and sold at a high price, being pure except for a small quantity of carbonate and of nitrate. According to his statement the electromotive force of this element is about 4 volts.

The additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♀) from West Africa, presented by Mrs. Gretton; a Northern Lynx (*Felis lynx*) from the Carpathian Mountains, presented by the Count Constantin Branicki; an Isabelline Lynx (*Felis isabellina*) from Tibet, presented by Capt. Baldock; a Forster's Milvago (*Milvago australis*) from Falkland Islands, presented by Dr. A. M. McAldonie; an Annulated Snake (*Leptodira annulata*) from Honduras, presented by Mr. R. E. Seabrooke; a Short-tailed Wallaby (*Halmaturus brachyurus* ♀) from West Australia, three Blue-crowned Hanging Parrakeets (*Loriculus galgulus*) from Ceylon, deposited; a Moloch Monkey (*Callithrix moloch*) from Brazil, two Snowy Owls (*Nyctea nivea* ♂ ♀), European, a Shore Lark (*Eremophila alpestris*), British, purchased; a Great Bustard (*Otis tarda*), European, received in exchange.

ON THE TRANSITS OF VENUS.[1]

Transits of Venus usually occur in pairs; the two transits of a pair being separated by only eight years, but between the nearest transits of consecutive pairs more than a century elapses. We are now on the eve of the second transit of a pair, after which there will be no other till the twenty-first century of our era has dawned upon the earth, and the June flowers are blooming in 2004. When the last transit season occurred the intellectual world was awakening from the slumber of ages, and that wonderous scientific activity which led to our present advanced knowledge was just beginning. What will be the state of science when the next transit season arrives God only knows. Not even our children's children will live to take part in the astronomy of that day. As for ourselves, we have to do with the present, and it seems a fitting occasion for noticing briefly the scientific history of past transits, and the plans for observing the one soon to happen.

When the Ptolemaic theory of the solar system was in vogue, astronomers correctly believed Venus and Mercury to be situated between the Earth and the Sun, but as these planets were supposed to shine by their own light, there was no reason to anticipate that they would be visible during a transit, if indeed a transit should occur. Yet, singularly enough, so far back as April, 807, Mercury is recorded to have been seen as a dark spot upon the face of the Sun. We now know that it is much too small to be visible to the naked eye in that position, and the object observed could have been nothing else than a large sun-spot. Upon the establishment of the Copernican theory it was immediately perceived that transits of the inferior planets across the face of the Sun must occur, and the recognition of the value of transits of Venus for determining the solar parallax was not long in following. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks; but Halley was first to work it out completely, and to him is usually assigned the honour of the invention. His paper, published in 1716, was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus of 1761 and 1769, from which our first accurate knowledge of the Sun's distance was obtained.

When Kepler had finished his Rudolphine tables they furnished the means of predicting the places of the planets with some approach to accuracy; and in 1627 he announced that

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[1] An address delivered before Section A of the American Association for the Advancement of Science, on August 23, 1882, by Prof. Wm. Harkness, Chairman of the Section, and Vice President of the Association.

Mercury would cross the face of the Sun on November 7, 1631, and Venus on December 6 of the same year. The intense interest with which Gassendi prepared to observe these transits can be imagined when it is remembered that hitherto no such phenomena had ever greeted mortal eyes. He was destitute of what would now be regarded as the commonest instruments. The invention of telescopes was only twenty years old, and a reasonably good clock had never been constructed. His observatory was situated in Paris, and its appliances were of the most primitive kind. By admitting the solar rays into a darkened room through a small round hole, an image of the Sun nine or ten inches in diameter was obtained upon a white screen. For the measurement of position angles a carefully divided circle was traced upon this screen, and the whole was so arranged that the circle could be made to coincide accurately with the image of the Sun. To determine the times of ingress and egress, an assistant was stationed outside with a large quadrant, and he was instructed to observe the altitude of the sun whenever Gassendi stamped upon the floor. Modern astronomical predictions can be trusted within a minute or two, but so great did the uncertainty of Kepler's tables seem to Gassendi that he began to watch for the expected transit of Mercury two whole days before the time set for its occurrence. On the 5th of November it rained, and on the 6th clouds covered the sky almost all day. The morning of the 7th broke, and yet there was no respite from the gloomy pall. Gassendi continued his weary watch with sickening dread that the transit might already be over. A little before eight o'clock the sun began to struggle through the clouds, but mist prevented any satisfactory observation for nearly another hour. Towards nine the sun became distinctly visible, and turning to its image on the screen, the astronomer observed a small black spot upon it. It was not half as large as he expected, and he could not believe it was Mercury. He took it for a sun-spot, and carefully estimated its position at nine o'clock, so that he might use it as a point of reference for the planet, if indeed he should be fortunate enough to witness the transit. A little later he was surprised to see the spot had moved. Although the motion was too rapid for an ordinary sun-spot, the small size of the object seemed to forbid the idea that it was Mercury. Besides, the predicted time of the transit had not yet arrived. Gassendi was still uncertain respecting the true nature of the phenomenon when the sun again burst through the clouds and it was apparent that the spot was steadily moving from its original position. All doubt vanished, and recognizing that the transit, so patiently watched for, was actually in progress, he stamped upon the floor as a signal for his assistant to note the sun's altitude. That faithless man, whose name has been forgotten by history, had deserted his post, and Gassendi continued his observations alone. Fortunately the assistant returned soon enough to aid in determining the instant of egress, and thus an important addition was made to our knowledge of the motions of the innermost planet of the solar system.

After this success in observing Mercury, Gassendi hoped he might be equally fortunate in observing the transit of Venus on December 6, 1631. He knew that Kepler had assigned a time near sunset for first contact, but the tables were not sufficiently exact to forbid the possibility of the whole transit being visible at Paris. Alas, alas! these hopes were doomed to disappointment. A severe storm of wind and rain prevailed on December 4th and 5th, and although the sun was visible at intervals on the 6th and 7th, not a trace of the planet could be seen. We now know that the transit happened in the night between the 6th and 7th, and was wholly invisible at Paris.

Transits of Venus can occur only in June and December, and as the two transits of a pair always happen in the same month, if we start from a June transit the intervals between consecutive transits will be 8 years, 105½ years, 8 years, 121½ years, 8 years, 105½ years, and so on. This is the order which exists now, and will continue for many centuries to come, but it is not always so. The path of Venus across the sun is not the same in the two transits of a pair. For a pair of June transits, the path at the second one is sensibly parallel to, and about twenty minutes north of, that at the first; while for a pair of December transits the parallelism still holds, but the path at the second one is about twenty-five minutes south of that at the first. Hence it happens that whenever Venus passes within about four minutes of the sun's centre at a June transit, or within about eight minutes at a December transit, she will pass just outside the sun's disk at the other transit of the pair, and it will fail. Thus the intervals between consecutive transits may be modified in various ways. If the first transit of a June pair fails, they will become 129½ years, 105½ years, 8 years, 129½ years, etc. If the second transit of a June pair fails, they will become 113½ years, 8 years, 121½ years, 113½ years, etc. If the first transit of a December pair fails, they will become 8 years, 113½ years, 121½ years, 8 years, etc. If the the second transit of a December pair fails, they will become 8 years, 105½ years, 129½ years, 8 years, etc. And finally, if either the first or second transit of a pair fails both in June and December, they will become 113½ years, 129½ years, 113½ years, 129½ years, etc.

When Kepler predicted the transit of 1631, he found from his tables that at her inferior conjunction on December 4, 1639, Venus would pass just south of the sun, and therefore he believed the second transit of the pair would fail. On the other hand, the tables of the Belgian astronomer, Lansberg, indicated that the northern part of the sun's disk would be traversed by the planet. In the fall of 1639 this discrepancy was investigated by Jeremiah Horrocks, a young curate only twenty years old, living in the obscure village of Hoole, fifteen miles north of Liverpool, and he found, apparently from his own observations, that although Kepler's tables were far more accurate than Lansberg's, the path of the planet would really be a little north of that assigned by Kepler, and a transit over the southern portion of the sun would occur. He communicated this discovery to his friend William Crabtree, and these two ardent astronomers were the only ones who had the good fortune to witness this, the first recorded transit of Venus.

Horrocks had great confidence in his corrected ephemeris of Venus, and it forbade him to expect the ingress of the planet upon the sun before three o'clock in the afternoon of Sunday, November 24, old style (December 4, new style); but as other astronomers assigned a date some hours earlier, he took the precaution to begin his observations on the 23rd. The 24th seems to have been partially cloudy, but he watched carefully from sunrise to nine o'clock; from a little before ten until noon; and at one o'clock in the afternoon; having been called away in the interval by business of the highest importance—persumably the celebration of divine service. About fifteen minutes past three he was again at liberty, and as the clouds had dispersed, he returned to his telescope and was rejoiced to find Venus upon the sun's disk, second contact having just happened. Only thirty-five minutes remained before sunset, but during these precious moments he made determinations of the position of Venus which are even yet of the highest value. Crabtree was less fortunate. At his station, near Manchester, there was but a momentary break in the clouds a quarter of an hour before sunset. This sufficed to give him a glimpse of the transit, and he afterwards made a sketch from memory.

The years sped swiftly by, and as the transit of 1761 approached, Halley's paper of 1716 was not forgotten, although he himself had long been gathered to his fathers. In deciding to what extent his plans could be followed, it was first of all necessary to know how nearly the real conditions would approximate to those he had anticipated. Passing over a paper by Trébucket calling attention to errors in Halley's data, Delisle was first to point out the exact conditions of the transit, and the circumstances upon which the sucess of the observations would depend. In August, 1760, less than a year before the event, he published a chart showing that inaccurate tables of Venus had misled Halley, both as to the availability of his method, and in the selections of stations. The occasion could be more effectively utilized by a change of plan, and Delisle considered it best to observe at suitably selected localities from many of which only the ingress, or only the egress, would be visible. Ferguson, in England, seems to have arrived independently at similar conclusions.

The two methods proposed respectively by Halley and Delisle have played so important a part in the history of physical astronomy that it will not be amiss to state briefly the distinction between them. The sun causes Venus to cast a shadow which has the form of a gigantic cone, its apex resting upon the planet, and its diameter continually increasing as it recedes into space. All the phenomena of transits are produced by the passage of this shadow cone over the earth, and as each point of the cone corresponds to a particular phase of a transit, any given phase will encounter the earth, and first become visible, at some point where the sun is just setting; and will leave the earth, and therefore be last visible, at some point where the sun is just rising. Between these two points it will traverse nearly half the earth's circumference and in so doing will consume about twenty minutes.

The only phases dealt with by either Halley's or Delisle's method are the external and internal contacts, both at ingress and at egress. Delisle's method consists in observing the times of contact at stations grouped about the regions where either ingress or egress is soonest and latest visible. The longitudes of the stations must be well determined, and then by combining them with the observed times of contact the rate at which the shadow cone sweeps over the earth becomes known, and from it the solar parallax results. At many of the stations best suited for Delisle's method, only the beginning or only the ending of the transit will be visible; but for the application of Halley's method, both the beginning and the ending must be seen. The theory of the latter method is so complicated that it is difficult to explain it briefly and at the same time accurately; but the the following considerations will suffice to indicate its nature. The duration of a transit at any point on the earth's surface depends partly upon the length of path, and partly upon the velocity, of that point while within the shadow cone. The length of path is affected by the latitude of the point, and the velocity by the earth's diurnal motion, which in some regions accelerates, and in others retards, the progress of the shadow. The result is that throughout one-half the earth's surface the duration of the transit is lengthened, while throughout the other half it is shortened; the maximum lengthening and shortening occurring at the respective poles of the hemispheres in question. Although these poles are not situated at the extremities of the earth's axis, it usually happens that one of them is shrouded in night; but upon the sunlit side of the earth, from which alone observations can be made, localities may exist at some of which the duration of the transit will be twenty minutes or more greater than at others. This inequality is the condition upon which Halley's method depends, and when such localities are accessible it may be advantageously applied. Briefly then, Halley's method consists in observing the duration of a transit at two or more stations so selected as to give durations of widely different lengths; while Delisle's method consists in employing a common standard time to note the instant when the transit begins, or ends, at two or more stations so chosen as to give very different values for that instant.

The transit of 1761 was visible throughout Europe and was well observed by astronomers in all parts of that continent. Besides this, England sent expeditions to St. Helena and to the Cape of Good Hope; and English astronomers observed at Madras and Calcutta; French astronomers were sent to Tobolsk, Rodriguez, and Pondicherry; Russians to the confines of Tartary and China; and Swedes to Lapland. No less than 117 stations were occupied by 176 observers; and of these, 137 published their observations. When this mass of data was submitted to computation, the result was far from satisfactory. Values of the solar parallax were obtained ranging from 8.49 seconds to 10.10 seconds; and in their disappointment the astronomers of the eighteenth century concluded that too much reliance had been placed upon Delisle's method.

The transit of 1769 drew on apace; and, to avoid a repetition of the fancied mistake of 1761, attention was directed almost exclusively to Halley's method. The conditions of the transit were carefully discussed by Hornsby in England, and by Lalande and Pingré in France; and it was found that its duration would be greatest in Lapland and Kamschatka, and least in the Pacific Ocean, California and Mexico. Astronomers were dispatched to all these regions. England sent the famous Capt. Cook to Otaheite, France sent Chappe to California; the King of Denmark sent Father Hell to Lapland; and in addition numerous observations were made in Europe, North America, China, and the East Indies. The preparations were most elaborate, and the result better than in 1761, but still not satisfactory. The black drop and other distortions disturbed the contacts in this transit as they had done in the previous one, and the values of the parallax deduced by the best computers ranged from 8.43 seconds to 8.85 seconds.

Thus the matter rested till 1825 and 1827 when Encke published abstracts of his discussion of the transits of 1761 and 1769, from which he deduced a parallax of 8.58 seconds. This discussion was not printed in full till 1835, when it immediately commanded the attention of astronomers, and its result, which Encke had modified to 8.57 seconds, was universally accepted for more than a quarter of a century. As time wore on, certain gravitational investigations led to a strong suspicion that the sun's distance had been over-estimated by at least three million miles, and the observations of Mars at its opposition in 1862 converted this suspicion into a conviction. The eighteenth century transits were again rediscussed and a parallax of 8.83 seconds was found from them by Powalky in 1865, and 8.91 seconds by Mr. E. J. Stone in 1868. Newcomb's paper, in 1867, also produced a marked impression.

The transit of 1874 was then approaching, and in the discussion as to how it should be utilized Halley's and Delisle's methods once more played a prominent part. It was recognized that the uncertainty in the observed times of contact of the eighteenth century transits was largely due to the black drop, and the causes of that phenomenon were carefully considered. Among them, most astronomers believed that irradiation played an important, if not the principal, part; but at the same time there was a general feeling that the telescopes of a century ago were bad, and that the magnificent instruments of the present day would give better results. In view of all the circumstances it was determined that the contacts should be observed with equatorially-mounted achromatic telescopes of from 4 to 6 inches' aperture, or with reflectors of not less than [illegible] inches' aperture, and that magnifying powers of from 150 to 200 diameters should be employed. The Germans and Russians adopted heliometers of about three inches' aperture for making exact determinations of the positions of Venus during transit, but other nations did not follow their example.

Photography, an agency undreamed of in the eighteenth century, was also available, and all saw the desirability of employing it; but there was much difference of opinion as to how should this be done. The European astronomers preferred instruments modelled upon the Kew photoheliograph, whose objective has 3.4 inches aperture and 50 inches focus, giving an image of the sun 0.482 of an inch in diameter, which is enlarged by a secondary magnifier to 3.93 inches. On the other hand, the American astronomers contended that photographs taken with such instruments would be affected by troublesome errors due to the secondary magnifier, that position angles could not be measured from them accurately enough to be of any use, and that it would be exceedingly difficult to determine the exact linear value of a second of arc. They advocated the use of horizontal photoheliographs, which are free from all these disadvantages; and the instruments which they adopted had apertures of 5 inches, and focal distances of 38½ feet, giving images of the sun slightly more than 4 inches in diameter. Notwithstanding this radical difference of opinion respecting the best form of photoheliograph, the astronomers of the old and new worlds were in perfect accord as to how the instruments should be employed. Between the first and second contacts, and again between the third and fourth contacts, photographs about five minutes square, showing the indentation cut by the planet into the sun's limb, were to be taken at intervals of a few seconds; and from these it was hoped the true times of contact could be deduced with great accuracy. Between the second and third contacts, pictures of the entire sun were to be taken at short intervals, and the positions of Venus relatively to the sun's centre were to be obtained from them by subsequent measurements. In the latter case, the photoheliograph took the place of a heliometer, and was superior to that instrument in its power of rapidly accumulating data.

The question of instrumental outfit having been disposed of, stations were selected, and parties dispatched to almost every available point. The United States, England, France, Germany, Russia, Holland,—in short, nearly all the nations of the civilized world,—took part in the operations. The weather was not altogether propitious on the day of the transit, but nevertheless a mass of data was accumulated which will require years for its thorough discussion. When the parties returned home the contact observations were first attacked, but it was soon found that they were little better than those of the eighteenth century. The black drop, and the atmospheres of Venus and the Earth, had again produced a series of complicated phenomena, extending over many seconds of time, from among which it was extremely difficult to pick out the true contact. It was uncertain whether or not different observers had really recorded the same phase, and in every case that question had to be decided before the observations could be used. Thus it came about that within certain rather wide limits the resulting parallax was unavoidably dependent upon the judgment of the computer, and to that extent was mere guesswork. Attention was next directed to the photographs, and soon it began to be whispered about that those taken by European astronomers were a failure. Even yet I am not aware that the Germans have published anything official on the subject; but the English official report has appeared, and it frankly declares that "after laborious measures and calculations it was thought best to abstain from publishing the results of the photographic measures as comparable with those deduced from telescopic view." From the way in which these photographs were taken, Sir George Airy saw that they could not yield position angles of any value, and therefore differences of right ascension and declination could not be determined from them; but they did seem capable of giving the distance between the centres of Venus and the sun with considerable accuracy. Upon trial this proved not to be the case. No two persons could measure them alike, because "however well the sun's limb on the photograph appeared to the naked eye to be defined, yet on applying to it a microscope it became indistinct and untraceable, and when the sharp wire of the micrometer was placed on it, it entirely disappeared." In short, the British photographs are useless for the present, but Sir George Airy hopes that in the future some astronomer may be found who will be capable of dealing with them.

We turn now to the American photographs. They present a well defined image of the sun about 4.4 inches in diameter, and are intended to give both the position angle and distance of Venus from the sun's centre. A special engine was at hand for measuring them, but when they were placed under the microscope only an indistinct blur could be seen. Here again was the same difficulty which had baffled the English, but fortunately its cause was soon discovered. The magnifying power of the microscope was only 37½ diameters, which seemed moderate enough, but was it really so? The photographic image of the sun was about 4.4 inches in diameter, and this was magnified 3.31 times by the objective of the microscope, thus giving an image 14.56 inches in diameter. To yield an image of the same size, a telescopic objective would require a focus of about 1563 inches, and if the eye-piece of the microscope, which had an equivalent focus of 0.886 of an inch, were applied to it, a power of 1764 diameters would be produced. This then was the utterly preposterous power under which the image of the sun was seen when the photograph was viewed through the microscope, and no useful result could be expected from it. Means were immediately provided for reducing the power of the microscope to 5.41 diameters, and then the photograph seen through it appeared as the sun does when viewed through a telescope magnifying 255 diameters. After this change all difficulty vanished, and the photographs yielded excellent results. The measurements made upon them seem free from both constant and systematic errors, and the probable accidental error of a position of Venus depending upon two sets of readings made upon a single photograph is only 0.553 of a second of arc. To prevent misunderstanding it should be remarked that this statement applies only to pictures taken between second and third contact, and showing the entire sun. The small photographs taken between first and second contact and again between third and fourth contact, proved of no value.

These investigations consumed much time, and before the result from the American photographs was generally known, an international convention of astronomers was held in Paris to consider how the transit of 1882 should be observed. The United States was not represented at this conference, and guided only by their own experience, the European astronomers declared that photography was a failure and should not be tried again. They knew that the contact methods are attended by difficulties which have hitherto proved insurmountable, but under the merciless pressure of necessity, they decided to try them once more. Unfettered by the action of the Paris Conference, the United States Transit of Venus Commission took a very different view of the case. Its members knew that the probable error of a contact observation is 0.15 of a second of arc, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. They also knew that the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only 0.55 of a second of arc. If the sun is visible for so much as six minutes between the second and third contacts, by using dry plates thirty-six negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. These were the reasons which led the American Commission to regard photography as the most hopeful means of observation, and thus it happens that the astronomers of the old and new worlds differ radically respecting the best means of utilizing one of the most important astronomical events of the century. The Europeans condemn photography, and trust only to contacts and heliometers; the Americans observe contacts because it costs nothing to do so, but look to photography for the most valuable results

In 1716, Halley thought that by the application of his method to the transit of 1761, the solar parallax could certainly be a determined within the five hundredth part of its whole amount. Since then, three transits have come and gone, and the contact methods have failed to give half that accuracy. From the photographic method, as developed by the U. S. Transit of Venus Commission, we hope better things, and perhaps fifty years hence its results may be regarded as the most valuable of the present transit season. In 1874, as in 1761, exaggerated views prevailed respecting the value of transits of Venus, but no competent authority now supposes that the solar parallax can be settled by them alone. The masses of the Earth and Moon, the moon's parallactic inequality, the lunar equation of the earth, the constants of nutation and aberration, the velocity of light, and the light equation, must all be taken into account in determining the solar parallax, and it cannot be regarded as exactly known until the results obtained from trigonometrical, gravitational, and phototachymetrical methods are in perfect harmony. It may be be many years before this is attained, but meanwhile practical astronomy is not suffering. Its use of the solar parallax is mainly confined to the reduction of observations made at the surface of the earth to what they would have been if made at the Earth's centre; and for that, our present knowledge suffices. The real argument for expending so much money upon transits of Venus is that being an important factor in determining the solar parallax, their extreme rarity renders it unpardonable to neglect any opportunity of observing them. Let us do our whole duty in this matter that posterity may benefit by it, even as we have benefited by the labours of our predecessors.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Oxford.—The professoriate has been strengthened by the election of Dr. Burdon Sanderson to the new Chair of Physiology on the Waynflete foundation in connection with Magdalen College. The Biological side of the Museum will now be divided into two departments.

The Brakenbury Natural Science Scholarship at Balliol College has been awarded by the Examiners to Mr. Walker Overend, of the Yorkshire College of Science and St. Bartholomew's Hospital.

Cambridge.—Messrs. J. W. Hicks and F. Darwin are appointed members of the Botanic Garden Syndicate; Dr. Ferrers (Master of Caius), and Prof. Stuart, of the Museums and Lecture-Rooms Syndicate; Prof. Stuart is also specially re-appointed to the Local Examinations and Lectures Syndicate; Dr. Ferrers and Mr. Routh are appointed on the Observatory Syndicate; Prof. Humphry and Mr. Vines, on the State Medicine Syndicate; Mr. Trotter, on the Special Board for Medicine; Mr. Besant, on the Special Board for Mathematics; Mr. Shaw, on the Special Board for Physics and Chemistry; Mr. Vines, on the Special Board for Biology and Geology.

Messrs. J. C. Saunders and J. W. Hicks are approved as Teachers of Botany and Chemistry respectively for the purpose of certificates for Medical Students.

The following colleges have offered open exhibitions or scholarships for natural science, with examinations in December or January next: Trinity, examination, December 12, one exhibition of 50*l*. for two years; candidates to be under nineteen on March 25 next. St. Johns, one exhibition, 50*l*., for three years, examination December 12; Caius, Jan. 8; Christ's, Emmanuel, and Sidney, January 12, a joint examination; candidates for all these must be under nineteen years of age. Particulars may be obtained from the tutors of the respective colleges.

Glasgow.—The following appointments to Scholarships, &c., have been made in accordance with the results of the Competitive Examinations:—George A. Clark Scholarship in Mental Philosophy (£200 for four years), John S. McKenzie, M.A.; William Ewing Fellowship in Mental Philosophy (£80 for three years), James A. McCallum, M.A.; Eglinton Fellowship in

Mathematics and Natural Philosophy (£100 for three years), John Weir; James Ferguson Bursary in Mathematics (£70 for two years), William Weir; Breadalbane Scholarship in Mathematics (£50 for three years), James Hamilton, M.A.; John Clark (Mileend) Scholarship in Natural Science (£50 for four years), William Huntly, M.A.; John Clark (Mileend) Scholarship in Classics (£50 for four years), James McMillan.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 16.—On *Megalania prisca*, part 4, by Prof. Owen, F.R.S.—The author, referring to a remark during a discussion following a previous communication (April, 1880), on the great horned Saurian of Australia, viz. that the skull then described might have belonged to a Chelonian, not to the genus and species founded on fossil vertebræ from localities remote from the formation yielding the cranial evidence, proceeded to describe his latest received fossils from the river-bed in Darling Downs, which included, besides evidences of the pelvis and limbs of *Megalania*, also dorsal vertebræ identical in size and character with those on which the former existence of such large Saurian had been predicted (1858). The contiguity of the last discovered vertebra, by Mr. G. F. Bennett, to the cranial and caudal fossils previously found and transmitted by him to the author, and the absence of any remains of a Chelonian reptile in the whole extent of the dried up bed of the river so perseveringly explored by that gentleman, would permit no doubt, the author believed, as to the conclusions which had been admitted in the previous volumes of the *Philosophical Transactions*.

Linnean Society, November 16.—F. Crisp, LL.B., Treasurer, in the chair.—Mr. O. T. Olsen and Surgeon J. N. Stone, R.N., were elected Fellows.—Dr. W. C. Ondaatje exhibited and made remarks on some Ceylon plants, among these, the fruit of *Randia dumetorum*, a remedy for dysentery; the leaves of *Sethia acuminata*, anthelmintic, and the resin of *Semecarpus gardneri*, from which a black varnish is prepared.—Mr. W. T. Thiselton Dyer called attention to a specimen of *Cycas beddomei*, a new species from Southern India.—Mr. F. P. Balkwill exhibited a series of British Foraminifera under the microscope, and said a few words on the special mode of mounting the same.—Mr. F. J. Hanbury showed a large fungus grown in a City wharf cellar, and which Mr. G. Murray pronounced to be a species of *Lentinus*.—Mr. C. Stewart exhibited a specimen of *Pilobolus*, explaining his observations on the projection of its sporangia.—Mr. J. G. Baker read a paper on the flora of Madagascar. It contains descriptions of 140 new species of poly petalous dicotyledons, obtained by the Rev. R. Baron and Dr. G. W. Parker. Some are of widely-diffused tropical genera, such as *Eugenia*, *Crotalaria*, &c.; others are of more familiar temperate types—*Alchemilla*, *Clematis*, and *Polygala*. Still others are characteristic of the Cape flora now noted for the first time from Madagascar, such as *Sphedamnocarpus* and *Sparmannia*. There is an interesting new genus of Malphigiaceæ (*Microsteira*) allied to the American *Hiræa*. Representatives of *Hibbertia* and *Rulingia* are interesting, from their being characteristically Australian genera. Mr. Baron has rediscovered *Rhodolæna alterola*, a showy plant, originally found by Du Petit Thouars a century ago. Dr. Parker has paid special attention to the drugs, esculents, and timber trees of the island, and catalogued 300 native names of the same.—A note by Mr. E. P. Ramsay, on the type specimen of Finsch's Fruit Pigeon was read.—Dr. Maxwell Masters read a communication on the Passifloræ collected in Ecuador and New Granada by M. Ed. André. Of *Tacsonia* 9 species are mentioned; of *Passiflora* 29 species, four being new. Some are of special interest structurally, the excellence of the specimens enabling ample examination of the curious flower structure to be made.—A paper was read on cerebral homologies, by Prof. Owen. He compares the brain of the locust and the cuttle-fish with that of the fish, and other higher forms, and summarises as follows:—That the homologies of the primary divisions of the brain in molluscs are the parts known in Articulates as the supra- and sub-œsophageal ganglions, with their commissural cords. That the topical relations of these parts to the gullet are the same in both great divisions of invertebrates; and that the homologies of the aforesaid parts with the primary divisions of the vertebrate brain are affected solely by the altered relation thereto of the gullet and mouth.—A paper was read on *Cassia lignea*, by W. T. Thiselton Dyer.—Thereafter, the sixteenth contribution to the mollusca of the *Challenger* Expedition by the Rev. R. Boog Watson, was read, in which were described the families Fissurellidæ and Cocculinidæ.

Chemical Society, November 16.—Prof. Dewar, vice-president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting (December 7).—The following communications were made:—Contributions to the chemistry of tartaric and citric acids, by the late B. J. Grosjean. This paper has been compiled by Mr. Warington from the laboratory note-books left by the author. It contains investigations as to the loss of water by different specimens of citric acid, the determination of citric acid in lemon, bergamot, lime, and orange juices, the influence of heat on solutions of a totartaric acid, influence of sulphuric acid on the crystallisation of tartaric acid, action of solutions of potassium and sodium sulphates on calcium tartrate, determination of free sulphuric acid in tartaric acid liquors, determination of tartaric acid by precipitation as bitartrate, &c.—Contributions to the chemistry of bass fibres, by C. F. Cross and E. J. Bevan. The authors detail further experiments, showing that lignified fibres are to be regarded as a chemical whole rather than the mixture which was necessitated, viz. the incrustation theory.—On the oxidation of cellulose, by C. F. Cross and E. J. Bevan. By the action of boiling 60 per cent. nitric acid, cellulose is converted into an amorphous substance $C_{18}H_{26}O_{16}$, oxycellulose.—On the analysis of certain vegetable fibres, *Annanassa*, *Musa*, *Phormium*, *Linum*, *Urtica*, &c., by C. S. Webster.—On the constitution of some bromine derivatives of naphthalene (third notice), by R. Meldola. The author concludes that Glaser's α dibromnaphthalene and Meldola's metadibromnaphthalen are isomeric, and not identical. The author has also obtained by the diazo reaction β dibromnaphthalene, m.p. 81°, a new tribromnaphthalen, m.p. 113–114, a second melting at 63°, &c.—On the constitution of lophin (second notice), by F. R. Japp. The author brings forward fresh proofs that this body has the constitution of an anhydrobase, and not that ascribed to it by Radziszewski.

Geological Society, November 15.—Dr. J. Gwyn Jeffreys, F.R.S., vice-president, in the chair.—John Edmund Thomas and Joseph Williams were elected Fellows of the Society.—The following communications were read:—The drift-beds of the North-west of England and North Wales; part 2, their nature, stratigraphy, and distribution, by T. Mellard Reade, C.E., F.G.S. The author stated that the first part of this paper, read in 1873, treated of the low-level boulder clay and sands, specially in relation to the contained shells. Since that time he has been diligently collecting information to enable him to treat of the nature, origin, and stratigraphy of the drift lying between Liverpool and St. Bees and Liverpool and Carnarvonshire. The author's conclusions are that an ice-sheet, radiating from the mountain district of the English lakes and the south of Scotland, produced the planing and grooving of the rock and the red sand and rubble *débris*; then the ice melted back into local glaciers, and the submergence began. The low-level boulder-clay and sands were, during a slow submergence, laid down probably at depths of from 200 to 300 feet, and the author considers that all the phenomena can be satisfactorily accounted for by ordinary river-action and fraying of the coasts by the sea, combined with frost and ice due to a severer climate bringing down the materials of such river-basins to the sea, while icebergs and coast-ice sailed over, dropping on the sea-bottom their burdens of erratic stones and other materials from the mountain-districts of the north. He pointed out, also, that the great majority of the well-glaciated rocks were specially those that could be traced to the high lands. This fact was forced upon his notice after making a large collection of glaciated boulders and pebbles Among the rocks he had been able to identify, with the help of Prof. Bonney and Mr. P. Dudgeon, of Dumfries, Scawfell granite (Eskdale, of Mackintosh) was the most abundant granite; then came grey granites from Dumfries, syenite from Buttermere, which occurred all over the area described, and up to 1200 feet on the Macclesfield Hills, and syenite from Cannockfell. Other probable identifications were also named. The whole series of rocks from the Silurian to the New Red Marl were represented in the low-level boulder-clay; a few flints also occurred, and one piece of what was believed to be chalk. The paper concluded with an appendix by Mr. David Robertson, giving a list of the Foraminifera and other organisms found in the various beds of boulder-clay in the Atlantic Docks, Liverpool.—On the evidences of glacial action in South Brecknockshire and East Glamorganshire, by Mr. T. W. Edgeworth Davy. Communicated by Prof. J. Prestwich, F.R.S. The area which is included in this paper is about 200 square miles, extending north and south from the Brecknockshire Beacons to a line between Cowbridge and the mouth of the Rhymney, of which the Cly valley has been more particularly studied. Most of the rocks in this district, and particularly the Millstone Grit, retain traces of glacial markings. The whole area has a *moutonnée* aspect.

Anthropological Institute, November 14.—Mr. Hyde Clarke, vice-president, in the chair.—Mr. R. W. Felkin exhibited a Darfur boy who was rescued from slavery and brought to England by him in 1879.—Mr. Francis Galton exhibited a box about the size of a backgammon board, containing a geometric series of test weights for comparing the sensitivity of different persons. The test lay in ascertaining the smallest difference that could be perceived when handling them. The lowest weight was 1000 grains, which gives no uncertain sense of heaviness, and the highest weight was far short of what would fatigue the hand. Consequently, by Weber's law, the difference in the sense of heaviness produced by handling any two of the weights is the same, or nearly so, whenever those weights are separated by the same number of terms. For example, a person who could just and only just distinguish between the 4th and the 8th weight would do the same as regards the 2nd and the 6th, and the 6th and the 10th, the number of terms interval being 4 in each case. Again, as the only interpretation possible to the phrase that "the sensitivity of A is *r* times as great as that of B," is that A can perceive *r* grades of difference when B can only perceive one, it follows that the relative sensitivity of two persons is inversely proportionate to the number of terms between any pair of weights that they can respectively just discriminate. The unit of ratio was 2 per cent., but in the earlier terms there was a sequence of half units. The weights were made exactly alike in outward appearance; they were common gun-cartridges, stuffed equably with shot and wads and closed in the usual way. The term in the series to which each weight belonged was written on the wad that closed it. It was shown that the best economy of time was to present the weights in threes, to be sorted in their proper order. By making a proper selection, a wide range of testing power could be obtained by 30 cartridges ranged in 10 trays. The same principle admits of being extended to testing the delicacy of other senses, as taste and smell. Some provisional results were mentioned: (1) that intellectually able men had, on the whole, high powers of discrimination; (2) that men had more discriminating power than women; (3) that highly sensitive women did not seem able to discriminate more grades than others, though both sensation and pain were induced in them by lower stimuli; (4) that the blind, as a whole, were not peculiarly sensitive to this test, but rather the reverse. A discussion followed, in which Prof. Croom Robertson, Dr. Camps, Mr. Sully, D. Mortimer Granville, Dr. Mahomed, Mr. C. Roberts, Prof. Thane, and others took part.

Royal Horticultural Society, November 14.—Dr. M. T. Masters in the chair.—*Proliferous Flowers:* The Rev. G. Henslow exhibited a *Rhododendron balsamiflorum aureum*, with flowers arising from the centre of the pistil. The latter organ had dehisced longitudinally, and a cluster of malformed orange-coloured petals protruded from the orifice. He observed that every flower on one bush in his garden, of a common pink kind, had, during the last season, formed a blossom within the pistil, though in the latter case the flowers so formed had perfect as well as petaloid stamens. In every case the flower sprang from the axis at the base of the ovary. *Carnation:* a blossom with a secondary flower arising from within the calyx. *Blue-bell:* Each flower was borne on a pedicel of about two inches in length, and produced a secondary flower from the axil of a perianth leaf. In the place of one flower a complete raceme had grown.—*Solomon's Seal:* Leafy racemes occupied the positions of normal flowers.—*Monstrous Flowers:* He also showed the following:—*Pistillody of calyx in Violets*, in which the organs were in part or entirely virescent and malformed, having the sepals abortively ovuliferous, and the petals often laciniated. The sepals bore papiliform structures on the margins and mid-ribs, resembling rudimentary ovules. The only recorded instance of ovuliferous sepals was that of the garden pea, figured and described in the *Gard. Chron.* 1866, p. 897. *Pistillody of stamens:* He showed drawings illustrating various stages of ovuliferous stamens of the Alpine poppy. *Syngenesism* in *Diplotaxis tenuifolia:* The anthers of every flower cohered laterally, so that the pollen could not escape; the consequence being that in no case did a flower set seed. *Placental protrusion* in *Begonia:* Portions of the placentas covered with ovules had protruded externally from the summit of the ovary, apparently being due to hypertrophy.—*Chrysanthemum*. Dr. Masters showed a blossom, half the florets being white, the other half yellow, a diameter separating the two kinds.

CAMBRIDGE

Philosophical Society, November 13.—On the structure of the spleen, by Mr. J. N. Langley.—On the continuity of the protoplasm in the motile organs of leaves.—Dr. W. H. Gaskell exhibited two pieces of muscular tissue from the ventricle of a tortoise, one of which had been taught to execute rhythmical contractions.

PARIS

Academy of Sciences, November 20.—M. Jamin in the chair.—A letter was read from the Minister of Public Instruction, giving an *arrêté* which fixes the conditions of the next Volta prize, to be awarded in 1887 (see p. 89).—Results of experiments made at the Exhibition of Electricity on incandescent lamps, by MM. Allard and others. In general and for the spherical mean intensity of 1·20 Carcel, only about 12 to 13 Carcels per h.p. of arc, or 10 Carcels per h.p. of mechanical work, can be counted on, from incandescent lamps. Electric candles give 40 Carcels per h.p. of arc, regulators nearly 100, so that, generally, the economic values of the three systems are nearly as 1, 3, and 7.—Researches on the iodide of lead, by M. Berthelot.—On the decomposition of cyanogen, by the same.—Researches relative to the vision of colour, by M. Chevreul.—On the relation of lunar to solar action in the phenomena of the tides, by M. Hall.—Chemical studies on Silesian beet (continued), by M. Leplay.—Electro-chemical deposits of various colours, produced on precious metals, for jewellery, by M. Weil. He presented pieces of gold and silver jewellery, polychromised industrially with oxides of copper, by his processes. The colours resist friction, dry or moist air, air vitiated by sulphuretted hydrogen or coal gas, and light. M. Edm. Becquerel recalled the colorations obtained by his father with oxides of lead and iron.—On a *sulphocarbometer* for determining the quantities of sulphide of carbon contained in alkaline sulphocarbonates, by MM. Gélis. A glass flask filled with a solution of bisulphite of soda or potash, has on its neck a metallic sleeve with internal screw-thread; into this is screwed a corresponding metallic tube with stopcock under the terminal bulb of a graduated and closed glass tube holding the sulphocarbonate to be examined. On opening the cock, the liquids mix. The reaction is completed when the sulphocarbonate becomes quite colourless; then the height of the column of sulphide of carbon is noted, and the weight of the sulphide of carbon that was in the sulphocarbonate may be deduced.—Results of treatment adopted in Switzerland with a view to destruction of Phylloxera, by M. Mayet (see p. 89).—On two standards of the *aune* and the *pied de Roi* recently found by M. Wolf. He found them in the maritime arsenal of Cherbourg. They are at present the sole representatives of an attempt at unification of French measures long before the birth of the metric system, and the only models of ancient measures preserved in their integrity.—Observations of the planet (216) Cleopatra, and of the great comet of 1882, at Paris Observatory (equatorial of the western tower), by M. Bigourdan.—Observations of the same comet at Algiers Observatory, by M. Trépied.—On the same, by M. Jaubert. He notes (from the Popular Observatory) that the central part, or true tail, had a paler envelope, which nearly ceased to be visible as the comet rose above the horizon and the tail shortened—except a part nearest *α* Hydræ, which seemed brighter than at first.—On the solar energy, by M. Rey de Morande. The great uniformity of terrestrial vegetation till the Cenomanian epoch, and gradual differentiation since, according to latitude, the gradual invasion of southern regions by trees with caducous leaves, and disappearance of all vegetation in Polar regions, are phenomena explicable by gradual contraction of the sun, but inexplicable by gradual cooling of the earth.—On the works of Frederic Houtman, by

M. Veth.—On the functions of a single variable similar to the polynômes of Legendre, by M. Hugoniot.—On the motion of a system of two electrified particles of ponderable matter, and on the integration of a class of equations with partial derivatives, by M. Lévy.—Production by the dry way of some crystallised uranates, by M. Ditte.—On the second anhydride of mannite, by M. Fouconnier.—Action of tri-ethylamine on symmetrical trichlorhydrine, and on the two isomeric dichlorhydric glycides, by M. Reboul.—Note on the study of *longrain*, and the measures of schistosity in schistous rocks by means of their thermic properties, by M. Jannettaz. The large axis of the isothermal surface is parallel to the *longrain* or second cleavage, and the small axis is perpendicular to the first cleavage or plane of schistosity.—Lithine, strontian, and boric acid in the mineral waters of Contrexeville and of Schinzmark (Switzerland), by M. Dieulafait.—Experiments on the calcination of alunite in powder, for manufacture of alum and sulphate of alumina, by M. Guyot.—On the anastomoses of striated muscular fibres in invertebrates, by M. Jousset de Bellesme. They insure simultaneous contraction (*e.g.* in gastric glands).—On the functions of the digitiform or superanal gland of Plagiostomes, by M. Blanchard. It appears to be a true pancreas.—Evolution of the epithelium of poison glands in the toad, by M. Calmels.—On two tertiary *Plagiaulax*, obtained in the neighbourhood of Rheims, by M. Lemoine.—On the *Tingis* of the pear-tree, by M. Carlet.—Some letters on the recent aurora were communicated.

BERLIN

Physiological Society, November 10.—Prof. Du Bois-Reymond in the chair.—Dr. J. Geppert gave an account of some experiments which he and Dr. A. Fraenkel had made on the effect of rarefied air on the organism, in order to test the statements which Prof. Paul Bert had made on respiration in rarefied air, and the accompanying deficiency of oxygen in the blood. The animal experimented on—a dog—was unfettered in a box of sheet iron, with a glass window, in which it was possible to produce every desirable pressure with an air-pump, and the ventilation could easily be accomplished during the attained rarefaction. If the gas pressure in the box was sinking, by degrees, the animal did not show any change in its behaviour or its functions until the pressure was reduced to 38 cm. Then and at a further lessening of the pressure, the animal became restless, the respiration grew deeper and faster; again, at a further rarefaction of air, the movements became uncertain and giddy; at a pressure of 25 cm., one-third atmosphere, the animal fell asleep like a normal sleeping animal, and could remain so six or seven hours without any hindrance to his later complete restoration; occasionally awakened, the animal had severe paroxysms of dyspnœa, which, however, soon passed, and it fell again fast asleep. By further rarefaction, nearly to 15 cm., severe paroxysms of dyspnoea and convulsions very soon caused the death. Nearly the same appearance and the same succession of phenomena aëronauts have described during balloon ascensions: in the first stage a quite normal feeling, then accelerated and deeper respiration, faintness of the limbs, which increased to paralysis; during the increasing inability of moving the voluntary muscles, drowsiness begins, from which there was no awakening if the balloon still rises to more rarefied regions, as was the case with the unhappy aëronauts Cravé, Spinelli, and Sivel. These symptoms differ in no way from the phenomena described in these experiments, but the stages begin much sooner, and the 25 cm. pressure at which the dog only fell into a deep sleep is the extreme limit of available rarefaction for the aeronaut. The cause of this more early beginning of the stages of disease may be first the low temperature of the higher regions which Mr. Glaisher showed to sink till –20°, and otherwise the continuous muscular activity causing stronger effects of the lower degree of rarefaction. Quite another appearance is presented by the mountain disease which is characterised by nausea, choking, and vomiting, besides the strong respiratory movements and increased heart's action. Dr. Geppert supposes that neither the often but moderate degree of rarefaction (60 cm. pressure) nor the trifling want of oxygen is the *vera causa*, but, as Mr. Dufour has already asserted, the extreme weariness of the body. (In the discussion which followed Dr. Geppert's communication, Prof. Du Bois-Reymond observed that some years ago he had advanced and proved the theory that the mountain disease, especially the vomiting on ascending high mountains, was a reflex phenomenon due to the very strong dazzling of the eyes by the vast intensely white and brightly insolated snow-fields.) Again, Dr. Geppert has made many measurements on the absorption of the oxygen by the arterial blood at varying gas tensions. The manner of blood-letting, the measuring of its volume, and the gas analysis, were much exacter and less objectionable than in the corresponding experiments of Mr. Bert, particularly for the measuring of the blood volume, an ingenious apparatus was used. The results of these experiments were that the proportion of oxygen in the arterial blood remained normal with decreasing oxygen tension, till the gas pressure was sunk to 40 cm. At further sinking of gas-pressure the proportion of oxygen in the blood decreased, and the deficiency of oxygen grew very considerable. Finally Dr. Gepper concluded that in the action of rarefied air on the proportion of the oxygen in the blood the physical absorption plays not so much a part as rather the chemical affinity for hæmoglobin for the oxygen.—Prof. Munk then reported briefly on experiments executed by Mr. Orshansky in his laboratory on the influence of anemia on the electric excitability of the brain; the anemia was produced by pouring the blood out of the femoral artery, and the excitability was tested in that part of the brain-surface which is the centre of motion of the fore and hinder legs, partly with the constant, partly with the inductive stream. In the first stages of the bleeding there was no change of excitability, then it increased, till about one-seventh of the total blood volume was poured out, then any further loss of blood continuously decreased the excitability, till finally, when about two-thirds of the blood was gone it sunk to zero. In every stage of anemia the maximum of corresponding change of excitability never appeared immediately, but some time after bleeding. Through all the changing of stages of excitability, except when the irritability was sunk to zero, re-creation and return to the normal state took place after an interval. No certain relation between the blood-pressure after the bleeding, and the rate of irritability corresponding to that state of anemia could be established.

VIENNA

Imperial Academy of Sciences, November 2.—T. Horbaczewski, on the synthesis of uric acid.—V. Patelt, on the development of the mucous membrane of the large intestine. —A. Tarolimek, on the relation between tension and temperature of saturated vapours and saturated carbonic acid.—E. Weiss, computations of the positions of the cometary nebulæ discovered by T. J. Schmidt, of Athens, on October 9.—Hr. Weidel and K. Hazura, on cinchonine.—R. Wegacheider, on isovanilline.—T. v. Oppolzer, on the criterion relating to the existence of three solutions of the cometic problem.—T. Wiesner, studies on withering leaves and leaf-shoot, a contribution to the knowledge of the transplantation of plants.

CONTENTS

PAGE
THE INDIAN SURVEY By Major ALLAN CUNNINGHAM, R.E. . . . 97
GREEN'S "GEOLOGY" 98
OUR BOOK SHELF:—
  Yarrell's "History of British Birds" 98
  "Episodes in the Life of an Indian Chaplain" 99
LETTERS TO THE EDITOR:—
  Sir George Airy on the Forth Bridge.—CHARLES SHALER SMITH . 99
  The Aurora.—Prof. HERBERT MCLEOD; Rev. CHARLES J. TAYLOR; Rev. STEPHEN H. SAXBY; ALFRED BATSON . . . 99
  Lavoisier, Priestley, and the Discovery of Oxygen—G. F. RODWELL 100
  The Comet.—Major J. HERSCHEL 101
  An Urgent Need in Anthropology.—W. L. DISTANT 101
  A Modification of the Gold-leaf Electroscope and a Mode of Registering its Charge.—FREDERICK JOHN SMITH 102
  Palæolithic Gravels.—WORTHINGTON G. SMITH 102
  Ancient Monuments—WORTHINGTON G. SMITH 102
  Shadows after Sunset.—J. RAND CAPRON 102
  On the Isomerism of Albuminous Bodies.—SHIGETAKE SAGIURA . 103
  An Extraordinary Lunar Halo.—J. P. BARKAS 103
  Meteor.—W. L. HARNETT 103
  Flames in Coal Fire.—SM. 103
  Waterspouts on Land.—JOHN GEDDES MCINTOSH 103
NOTES FROM THE LETTERS OF CAPTAIN DAWSON, R.A., IN COMMAND OF THE BRITISH CIRCUMPOLAR EXPEDITION 103
ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE, II. By ANDREW GRAY (*With Diagram*) 105
PROFESSOR HENRY DRAPER, M.D. 108
THE COMET. By Commander W. T. SAMPSON, U.S.N.; Dr. C. J. B. WILLIAMS, F.R.S. (*With Diagrams*) 108
THE APPROACHING ECLIPSE OF MAY 6, 1883 110
THE TRANSIT OF VENUS ON WEDNESDAY, DECEMBER 6 112
NOTES 112
ON THE TRANSITS OF VENUS. By Prof. WM. HARKNESS 114
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 117
SOCIETIES AND ACADEMIES 117

THURSDAY, DECEMBER 7, 1882

RECENT RESEARCHES IN THE METAMORPHISM OF ROCKS

IN the heart of many mountain-ranges, and likewise spreading over wide hilly areas of the northern part of our hemisphere, lies the strikingly distinct series of rocks to which the name of The Crystalline Schists has been given. The passing traveller who knows nothing of geology cannot fail to be struck with their strange, crumpled and gnarled beds, through which streaks of white quartz wind and twist in a network of interlacing veins. Sheets of the naked rock often present a silvery sheen as sunlight falls across them, and this glistening aspect may be traced down in the minutest flakes of silvery mica that lie packed in parallel leaves throughout the mass.

No group of rocks has given rise to more discussion than the Schists. An account of the oscillations of opinion regarding their origin would form a curious and interesting chapter in the history of geological speculation. They have been looked upon as parts of the aboriginal crust of the planet—traces of the first solid film that formed upon its fiery surface. By one school of writers they are believed to be original chemical precipitates from the waters of the primeval ocean. By another they are treated as masses of sedimentary or other material which have been crystallised and altered into their present condition by a process to which the name Metamorphism has been given. Between these two doctrines, with their various modifications, the pendulum of geological opinion has vibrated for somewhere about a century, and vibrates still. In England and America indeed, owing mainly to the commanding influence of Lyell, the metamorphic theory has so entirely prevailed that most English-speaking geologists have come to accept it as a demonstrated truth, and to look back upon the Wernerian doctrine of chemical precipitates as a singular and happily obsolete vagary of the geological imagination. They have written text-books in which that doctrine is not even so much as honoured with an allusion to its ever having existed, though here and there a solitary protest has now and then been raised among us in favour of the other view, like that uttered by De la Beche as far back as 1834, and those of Dr. Sterry Hunt in later years. In Germany, on the other hand, the old Wernerian dogma has always had its staunch adherents, but in gradually diminishing numbers, the theory of the metamorphic origin of the crystalline schists having been warmly espoused there also by an ever-increasing body of observers.

For some years past what has been called the orthodox metamorphic doctrine has been called in question by various writers who have cast doubts on the observations which were believed to prove the fact that wide areas of rock, originally of fragmentary or detrital composition, had undergone a conversion into crystalline schists. The time-honoured doctrine of chemical precipitates, tricked out in the finery of modern chemical analysis, has been resuscitated and defended with the warmth of the most devoted partisanship. Within the present year, however, several memoirs have appeared which powerfully support the doctrine of metamorphism, and as effectively oppose the rival hypothesis. The aid of the microscope, as well as of chemical analysis, has been invoked: new facts and arguments have been adduced, and the nature of the changes involved in metamorphism have been more clearly made known. Whether or not there may be any crystalline schists in the earliest or Archæan rocks of the earth's crust, which had their origin in the chemical precipitates of a primeval ocean, may remain a question for future discussion. But recent researches with all the manifold appliances of modern petrography demonstrate beyond the possibility of all further cavil, that ancient sedimentary strata have undergone such an alteration as to have assumed a more or less completely crystalline condition, that numerous silicates have been developed in them, often also with foliation, that these changes are seen round bosses of granite and other eruptive masses (contact metamorphism), but also far more strikingly over wide regions where eruptive rocks cannot have induced them (regional metamorphism), and that in the latter case the alteration is always connected with evidence of enormous mechanical pressure of the strata. To one or two of the more important recent papers, brief reference may here be made.

The Silurian schists, with their fossils and remarkably compressed conglomerates in the Bergen district, have been made the subject of a remarkable memoir by Mr. Hans Reusch.[1] In this essay the author traces the passage from ordinary shales into fine phyllite-schists and mica-schists, in which crystalline aggregates of mica have been porphyritically developed. In some of the altered fossiliferous beds microscopic crystals of rutile and tourmaline have appeared. The fossiliferous limestones have been converted into marble, wherein, however, the organic forms can still be detected. The fossils which occur in certain mica-schists, and have been specifically determined, leave no doubt that the whole series of rocks belongs to the lower part of the upper Silurian system. Yet they include intercalated bands of gneiss, hornblende-schist, talc-schist, and other foliated rocks. The author connects the crystalline condition of these masses with the effects of the enormous mechanical pressure which they have undergone, as shown, for example, by the extreme flattening of the pebbles in some of the associated conglomerates.

The Silurian rocks of the Christiania district have long been famous for the illustrations they afford of the phenomena of contact-metamorphism. They have been subjected to a detailed investigation by Mr. W. C. Brögger, lately of the Geological Survey of Norway, and now Professor of Geology in the University of Stockholm. He has lately published what we hope is only an earnest of the valuable work we have yet to expect from him.[2] His monograph embraces the stratigraphy, palæontology, structure, eruptive rocks, and contact-metamorphism of the district. This last-named feature is more minutely traced out than has yet been attempted for that region, though only a beginning in the study has been made, Mr. Brögger deeming it better to publish his first results now than to wait for leisure to extend and complete them. He points out, as had already been done by Kjerulf and others, that while there is a general alteration as the rocks approach the eruptive masses of granite and syenite, the special type of alteration depends in each case upon the original capacity of the rock for metamorphism. He has traced the Silurian zones from their ordinary unaltered condition until they assume their most metamorphosed character against the granite, and he compares the chemical composition and microscopic structure of the unaltered and altered strata. He points out that certain bands of rock appear to be endowed with a remarkable capacity for withstanding the effects of metamorphism. Thus the *Dictyograptus*-shales may be observed close to the granite and in the midst of the most intensely-metamorphosed beds, yet comparatively little changed. They become paler in colour and perhaps somewhat harder and more compact, but their graptolites are as well preserved, down even to the minutest details, as they are at a distance from the contact-zone. The dark alum-shales are converted into hard compact bluish "Knotenschiefer" and chiastolite-slates, still retaining their fossils. The chiastolite crystals may even be seen traversing the graptolite-stems, which are otherwise as well preserved in these as in the ordinary unaltered shales. The remarkable development of silicates in the Christiania limestones, where these rocks have been converted into marble near the granite, has long been a classical instance of contact-metamorphism. Mr. Brögger gives some interesting observations of his own among these rocks. He notes the occurrence of recognisable fossils even in those parts of the marble where the silicates have been abundantly developed, and he points to the suggestive fact that where a fourth or fifth part of the marble is made up of red garnet, the latter mineral, in well crystallised rhombic dodecahedra, may be found inclosing the valve of an *Orthis calligramma*.

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[1] "Silurfossiler og Pressede Konglomerater i Bergensskifrene." (Christiania: *Universitets programm*, 1882) This memoir was recently noticed in these columns (NATURE, vol. xxvi. p. 567).

[2] "Die Silurischen Etagen 2 and 3 im Kristianiagebiet und auf Eker." (Christiania: *Universitatsprogramm*, 1882).

The alternation of comparatively little-changed graptolite shales with fine crystalline schists and forms of hornfels, which Prof. Brögger reports from so many localities, is a fact of great significance in relation to the problem of the origin of the crystalline schists. That the crystalline character has been superinduced upon what were once ordinary marine mechanical sediments admits now of no doubt. The extent of the change appears on the whole to depend on the one hand upon the liability of the rock to metamorphism, and on the other upon relative proximity to the eruptive rock. The preservation of organic remains in the altered bands is exceptional, and depends, according to our author, 1st, upon the greater permanence of the substance of the organisms, the chitin of the graptolites, for example, being apparently undistinguishable in the altered beds from the same substance in the ordinary shales; 2nd, upon the replacement of the hard parts of the organisms by mineral matter, either before or during the process of metamorphism; and 3rd, upon the filling up of the original cavities of the fossils by some mineral, as graptolites by pyrites, and the interior of brachiopods by wollastonite, or upon the inclosing of the organisms in a crystalline matrix as in the case of the impressions of shells in garnet, just referred to. But, as a rule, fossils disappear even from the most richly fossiliferous bands as these are traced across the altered zone. Mr. Brögger modestly regards his observations as still too limited to warrant him in theorising on the phenomena of contact metamorphism. But the admirable methods he has followed, connecting in one broad microscopical and chemical research both the altered and unaltered condition of the same rock, mark a new starting-point for the further study of that great geological problem—metamorphism.

There is one further incidental but pregnant statement in this Memoir to which reference must here be made. So far back as the years 1875 and 1877 Prof. Brögger, in the course of his field-work in the Geological Survey of Norway, established the existence of graptolite-bearing beds among the crystalline schists of the Hardanger region. He now publishes some details of the section there visible, from which we learn that the graptolite band (*Dictyograptus*-schiefer) occurs among some black little-altered alum-slates lying at the very base of the enormous series of crystalline schists forming the Norwegian highlands! The alum slates pass under some bluish quartzose sandstone, overlaid by a white impure marble (possibly the Orthoceratite limestone), which in turn is covered by greenish micaceous clay-slates (phyllites). Above these basement strata come more and more crystalline schists, comprising diorite-schists, hornblende-schists, garnetiferous mica-schists, foliated rocks of many varieties, and true gneisses—the two last mentioned rocks sometimes several thousand feet thick. We learn further that in 1877 the same observer, in harmony with Naumann's observations, established the fact that the enormous series of crystalline schists of the Norwegian mountains is younger than the second stage of the Silurian (or Cambrian) rocks of the Christiania district. He refers to his friend Mr. Reusch's discovery of Upper Silurian fossils from the crystalline schists of Bergen, as a confirmation of his former supposition that the whole of the vast succession of crystalline schists in the Norwegian mountains is a metamorphic series.

When we remember that on the opposite side of the peninsula similar primordial fossiliferous strata emerge from underneath the vast overlying schists and crystalline rocks of the Swedish uplands,[1] it is evident that an enormous area of regional metamorphism extends across Scandinavia. The close parallel between the structure of this region and that of the Scottish Highlands is one of the most striking facts in the geology of North-Western Europe. In both areas recognisable Silurian fossils occur at the very base of the vast metamorphic series, and the rocks become progressively more and more crystalline as they are traced from bottom to top.

A third remarkable paper by Père Renard, of the Royal Museum, Brussels, must be cordially welcomed as one of the most important contributions of modern petrography to the study of metamorphism.[2] It deals with a portion of the singular belt of crystalline schists which runs through the French and Belgian Ardennes. Dumont as, far back as the year 1848, published an account of these rocks, the significance of which that accurate observer fully perceived. He showed that they occur in his

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[1] See A. E. Tornebohm, Bihang till Svensk Akad. Handl., 1873.

[2] "Les Roches Grenatifères et Amphiboliques de la region de Bastogne," par A. Renard. *Bulletin du Musée Royal d'Histoire Naturel de Belgique*, tome i. 1882.

Coblentzian division of the Lower Devonian rocks of that region, that they pass insensibly into ordinary sedimentary rocks, but towards their axis have been metamorphosed into more or less crystalline compounds in which various silicates (garnet, hornblende, mica, &c.) have been developed. He observed fossil plants and animals in some parts of these altered rocks. In one of his specimens of a rock full of garnet, Sandberger determined the presence of the characteristic Devonian shells, *Spirifer macropteris* and *Chonetes sarcinulatus*. Nothing can be more emphatic than the testimony borne by Dumont to the age of these rocks and the fact of their metamorphism. His essay upon them is hardly known to geologists generally, but it deserves to rank as one of the most precise and detailed contributions ever made by a field-geologist to the study of the phenomena of metamorphism.[1] His observations have been singularly confirmed by those of M. Renard. The metamorphic phenomena of the Ardennes are repeated on a greater scale in the extension of the Devonian rocks eastward into the basin of the Rhine, where they have been admirably described by Lossen,[2] whose pregnant memoirs on this and other geological problems deserve the closest study of the student.

Bringing all the assistance which chemical analysis and microscopical investigation now supply to the study of the origin of rocks, M. Renard, in the present communication, which fitly opens the first number of the newly-organised *Bulletin du Musée Royal de Belgique*, presents us with a detailed description of the garnetiferous and hornblendic rocks of Bastogne in the south-eastern portion of the Belgian Ardennes. It is impossible to give any adequate *résumé* of this memoir within the space here available. But attention may be directed to one or two of its more interesting features.

At the outset it should be noted that the band of metamorphosed strata here referred to occurs along a line of plication running in a general east-north-east and west-south-west direction; that it is not associated with any visible eruptive rocks, that it dies away into ordinary unaltered greywacke and shale on the outside, and becomes more and more crystalline towards the axis, until it presents the most intense metamorphism anywhere to be found in Belgium.

In subjecting to microscopic examination thin slices of some of these altered rocks, M. Renard noticed that the quartz-granules, presumably of clastic origin, have lost the liquid inclusions so generally found in the quartz-granules of old sedimentary strata. This fact (already observed by Sorby in the case of sandstone invaded by dolerite) seems to indicate that the sand-grains have not escaped the influence of the changes which have so profoundly affected the other constituents of the former sediment. The original carbonaceous matter of the rocks, now altered into graphite, is spread as a fine dust among the other constituents, generally coating the minerals, sometimes inclosed within them, frequently accumulated at certain points into black, brilliant irregular bands, occasionally as hexagonal flakes. This aggregation of the carbon recalls the way in which the graphite occurs in Archæan limestones. The garnet crystals are marked by a singularly interesting arrangement of lines of crystalline inclusions disposed along the crystallographic axes of the inclosing crystals. In certain rocks the garnets (about three millimetres in diameter) are traversed by a series of paralleled joints or fissures which run in a given direction through all the crystals. These cannot, of course, be cleavage lines. They are attributed by M. Renard to fracture produced by mechanical pressure, and he remarks that the minute flakes interspersed through the ground-mass of the rock are oriented in the same direction.

Taking a general view of the microscopic structure of these rocks the author divides the constituent minerals into two groups: those which represent more or less distinctly the original sediment of which the rocks were formed, and those which have been subsequently developed by metamorphism. The quartz grains, for example, have preserved the closest resemblance to those of the ordinary normal arenaceous rocks of the lower Devonian series. The presence of graphite and anthracite likewise connects these crystalline masses with the sandy strata containing diffused carbonised vegetable matter. But on the other hand the crystalline structure and the presence of such minerals as garnet, hornblende, mica, titanite, and others connects these undoubtedly Devonian rocks with the crystalline schists of the Archæan series, as possibly both referable to the like series of physical and chemical changes.

M. Renard unhesitatingly discards the doctrine of direct chemical precipitation. He admits that the evidence of the physical structure of the country, as Dumont so well enforced, demonstrates that these crystalline rocks lie in the Devonian system and pass laterally into ordinary sedimentary accumulations. He further insists that the study of the minute structure of the rocks under the microscope confirms, in the most satisfactory manner, the view of that geologist that the actual condition of the masses has been produced by metamorphic action, in what way soever this action may have been induced. He connects the metamorphism with the proofs of great plication traceable through the altered Devonian rocks of the Ardennes. The mechanical action involved in the process would, he believes, predispose the sedimentary materials to a more or less complete recrystallisation. As it crushed them under the enormous pressure and partly was itself transformed into heat, it would set into active motion the chemical affinities of the various mineral substances. In this way sand might finally pass into quartzite, argillaceous mud into phyllade or phyllite-schist, sandy clay into more or less schistose micaceous quartzites; the calcareous matter would enter into combination to form the various lime-silicates so characteristic of these garnetiferous and hornblendic rocks; while the carbonaceous ingredient, losing some of its constituent elements, would separate out as graphite.

M. Renard's testimony to the theory of metamorphism is all the more valuable, as it has been extorted from him by the irresistible logic of facts against his own previous convictions. He has now furnished to this theory fresh evidence in its support, showing how well the observations by which it is established in the field are sustained by minute petrographical analysis. Every one interested in geological research will hope that the paper he has now published is only the first of a series on the same subject with which he will enrich the literature of the science. ARCH. GEIKIE

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[1] "Memoire sur les terrains Ardennais et Rhénan." *Mem Acad Roy de Belgique*, 1848.

[2] "Geognostische Beschreibung des Taunus," &c. *Zeitschrift der Deutsch. Geo Gesell.*, 1867, p 509.

HUMAN MORPHOLOGY

Human Morphology; a Treatise on Practical and Applied Anatomy. By Henry Albert Reeves. Vol. I. The Limbs and Perinæum. (London: Smith, Elder, and Co., 1882.)

THE author of this work is evidently very ambitious. In his preface he tells us that his primary wish was to produce a treatise in which he would deal thoroughly with the anatomy of man, and then compare his structure with that of other vertebrates, giving directions as to the dissection of the type-forms chosen in illustration. Further, being dissatisfied with anatomical nomenclature and classification generally, more especially with the terms at present in use in myology, he attempted a revision in this department.

As he proceeded with his task, however, he found that the labour, time, and knowledge necessary for carrying out so extensive a piece of work was too great, and that he had better relinquish his original idea and leave it for execution to more competent labourers.

But even after departing so far, and wisely as we think, from his first conception of what a student's text-book should be, he has found it necessary still further to withdraw from his original plan, and to excise much that he had written on anomalies of arrangement, various paragraphs on dissections which are out of the student's usual course to perform, and to reduce in quantity the sections on the practical applications of anatomy.

Had the author carried out his original idea of what a handbook for students and practitioners should be, he would have produced an encyclopædia of anatomy, and not a text-book for daily use.

But after all this renunciation of so much of the author's primary conception of what is required in a practical work on anatomy, sufficient is left to form a most voluminous treatise.

The volume before us extends to 719 large octavo pages. It comprises only the anatomy of the limbs and perinæum, and we are promised two additional volumes, each of between six and seven hundred pages, in order to complete the work.

It seems to us that the author even yet has not attained a proper idea of what the contents of a book should be, which, to use his own words, "is to be chiefly used *while the student is dissecting*." He has not sufficiently discriminated between the material that should find a place in a text-book of systematic anatomy and that which properly belongs to a practical treatise. We are quite in unison with him in the propriety of omitting all illustrations and detailed description of minute or microscopic anatomy. But we should have gone still further and cut out the historical sketch, the bibliography, the chapter on anatomical technics, which together would have subtracted between 60 and 70 pages from the volume. Also we should have condensed the descriptions and reduced in amount the sections on variations in the arrangements of the bones, muscles, and other soft parts.

A sketch of the rise and progress of anatomy, and a copious bibliography are not required by the student at the dissecting table. On the other hand they are both interesting and useful in a systematic treatise. The variations in arrangement, more especially in the muscular and vascular systems, which have been observed and recorded, are so multitudinous, that they would require a special treatise for their description. What the student has to deal with in his ordinary work, are the commoner departures from the usually described arrangements, such as a third head to the biceps muscle, the high division of the brachial artery, the variations in the place of origin of the obturator, the profunda, the circumflex arteries, and so on. These and such like ought to find a place in all works on practical anatomy, but the more unusual forms are best reserved for such special treatises as Macalister's Catalogue of Muscular Anomalies, or Quain's description of the Arterial System, to which the student, who is desirous of obtaining a more intimate acquaintance with variations in structure, ought to be referred.

A knowledge of anatomical technics also is undoubtedly of primary importance to professed anatomists. But is one student in five hundred ever called upon to inject a body, either with a preservative fluid, or with a coloured arterial or venous injection? This work is done for him either by the demonstrator, or by the practical assistant in charge of the rooms. To introduce therefore into a work intended for medical students generally an account of methods, which they are never required to carry out, seems to us to be uncalled for.

The author directs especial attention to the number and quality of the illustrations. As regards their quality, with a few exceptions they are artistically rendered. But we think they are far too numerous, and by their number, and the size of many of the cuts, they have largely contributed to the unwieldy bulk of this treatise. Too many illustrations in a book to be used at the dissecting table are apt to draw the student's attention away from his part, and to make him rely upon the pictorial representation rather than on his own efforts to display the organ or region in the subject itself.

In our judgment a handbook of practical anatomy ought to be of such a size, that the student can without inconvenience carry it to and from his work. The instructions for the order of the dissections should be clear and concise. The descriptions of the parts should not be too elaborate. The illustrations should be well selected, with a view to guide the student in the method of his work, and to show him what he has to look for, and where it has to be seen. This treatise fails to comply in many respects with these conditions, and much as we may commend the author for his industry and good intentions, we are afraid that he has produced a work which will have only a restricted field of usefulness.

OUR BOOK SHELF

Common British Insects. Selected from the Typical Beetles, Moths, and Butterflies of Great Britain. By the Rev. J. G. Wood, M.A., &c. Pp. i.-284. 8vo. (London: Longmans, Green, and Co., 1882.)

AFTER glancing through this book the question uppermost in our mind is: Why does it exist? The highly-ornamented cover, and the repeated title thereon, lead one to expect a popular treatise on all orders of insects, an idea at once dissipated by the title-page. There are other books covering the same ground that would answer the young student's purpose as well as this. Judging it in comparison with the multitudinous other compilations from the same pen, we have no very particular fault to find. It is sketchy, but in some respects it compares favourably, especially in some of the explanations concerning the *Coleoptera*. Some of the illustrations are good, others wretchedly bad, and unrecognisable without the explanations. When comparing the "nervures" in the wings of a butterfly with the "rays" in the fins of a fish (p. 178), the writer should have explained the minute structure of both.

The real point at issue in connection with books of this nature is their effect. They are eminently rudimentary, and not elevating. Let us take instances from the book now under review. At p. 14, after an explanation of the terminology of the external skeleton of a beetle, we read:—"At first some of these terms may appear to be harsh, repulsive, and difficult to master. In reality they are not so, and a knowledge of them is absolutely necessary to any one who wishes to understand the description of an insect." This is a very sensible remark. Yet throughout the book the utmost favour is bestowed upon absurd meaningless "English" names. The culminating point of absurdity is reached at p. 276. Amongst the small moths the author "figures" one (under a misspelt generic name), and because it (out of several hundred other fortunate little moths) has received no "popular" name, he terms it the "*Brown Dolly*"!

Anthropo-Geographie oder Grundzüge der Anwendung der Erdkunde auf die Geschichte. Von Dr. Friedrich Ratzel. (Stuttgart: Englehorn, 1882.)

WE have had occasion to speak of the wide extension which geographical science has taken in Germany, and of the broad and intricate field which it covers. The work before us is a good example of this. It is the first of a series of geographical handbooks, which is to include "General Geology," by Prof. von Fritsch; "Oceanography," by Dr. von Boguslawski; "Geographical Distribution of Animals," by Prof. L. von Graff; "Climatology," by Dr. Hann; "Glaciers," by Prof. Heim; "Volcanoes and Earthquakes," by Prof. von Fritsch; and "Botanical Geography," by Dr. Oskar Drude. Dr. Ratzel's volume must not be mistaken for a treatise on Anthropology. That subject it only incidentally includes, its main purpose being to point out in detail the light which geography sheds upon history and the development of social and political economy. The author discusses the various conceptions of geography, its place among the sciences, the human element in geography, and the relations between geography and history. After a brief introduction on these points, the author proceeds to consider, in the second part, natural conditions, and their influence on mankind. Under the head of position and aspect of the dwelling-places of man, pointing out the parts which continents, islands, and peninsulas have played in the distribution of the human species and in history, he devotes a chapter to states and the various conditions which determine their boundaries, and in another discusses the distribution of centres of population. In a chapter on conditions of space he discusses the subject of great and small states, and the connection between the extent and power of states, and has some specially interesting remarks on what he calls the continental type of history. In a section on surface-forms, the author treats of such subjects as the inequalities of the earth's surface and of the contrast, ethnologically and historically, between mountainous and flat countries—of plains, steppes, and deserts. To the important subject of coast-lines, and the dependence of a country's development on their form, a chapter is devoted, and two to the historical importance of water, in its various forms of sea, lakes, rivers, and marshes. Considerable space is, of course, given to climate and to the animal and plant world. One of the most interesting chapters is that on "Natur und Geist," in which Dr. Ratzel attempts to show the great influence of a people's surroundings on their mental and moral development. In two concluding chapters the author gathers up the lines of discussion, referring especially to the subject of human migration, its influence on history, and its effect on the mixture of races; and finally points out the practical bearings of his subject. Thus it will be seen that, whether the subject comes legitimately under the conception of geography or not, Dr. Ratzel has written a work of great interest and of much utility to the historian who wishes to treat history in a scientific spirit. It is both instructive and attractive reading.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Mimicry in Moths

I OBSERVED here, a few days ago, a case of mimicry which interested me much, and may deserve mention. The weather has been such as is usual on this part of the Riviera at this season. There has been a very hot sun, with sometimes a very cold "mistral" wind. Insect life is abundant, and not a few of our summer *Sylviadæ* seem to secure a very good living. Flies are a plague. Mosquitos are not wanting. Bees are busy, and large dragonflies hunt continually. But there is one order of insects "conspicuous by its absence," and that is the *Lepidoptera*. Neither the diurnal nor the nocturnal species have been visible.

I was much surprised, therefore, one day last week to see a large insect of this order come from above the olive trees overhead, with the wild dashing flight of the larger moths. Attracted apparently by the sheltered and sunny recess in which I was sitting and by the scarlet geraniums and bignonias which were in full flower in it, the moth darted downwards, and after a little hovering, settled suddenly on the bare ground underneath a geranium plant. I then saw that it was a very handsome species, with an elaborate pattern of light and dark chocolate browns. But the margins of the wings, which were deeply waved or dentated, had a lustrous yellow colour, like a brilliant gleam of light. In this position the moth was a conspicuous object. After resting for a few seconds apparently enjoying the sun, it seemed to notice some movement which gave it alarm. It then turned slightly round, gave a violent jerk to its wings, and instantly became invisible. If it had subsided into a hole in the ground, it could not have more completely disappeared. As, however, my eyes were fixed upon the spot, I soon came to observe that all the interstices among the little clods around it were full of withered and crumpled leaves of a deep blackish brown. I then further noticed that the spot where the moth had sat was apparently occupied by one of these, and it flashed upon me in a moment that I had before me one of the great wonders, and one of the great mysteries of nature. There are some forms of mimicry which are wholly independent of the animals themselves. They are made of the colour and of the shape which are like those of the surrounding objects of their habitat. They have nothing to do except to sit still, or perhaps to crouch. But there are some other forms of mimicry in which the completeness of the deception depends on some co-operation of the animal's own will. This was one of these. The splendid margins of the fore-wings, with the peculiar shape and their shining colour had to be concealed; and so, by an effort which evidently required the exertion of special muscles, these margins were folded down—covered up—and hidden out of sight. The remainder of the wings were so crumpled up that they imitated exactly the dried and withered leaves around.

Knowing the implicit confidence in the effectiveness of this kind of concealment, which is instructive in all creatures furnished with the necessary apparatus, I proceeded to try and test this very curious psychological accompaniment of the physical machinery. I advanced in the full sunlight close up to the moth—so close that I could see the prominent "beaded eyes" with the watchful look—and the roughened outlines of the thorax, which served to complete the illusion. So perfect was the deception that I really could not feel confident that the black spot I was examining was what I believed it to be. Only one little circumstance reassured me. There was some hole or interstice in the outer covering, through which one spot of the inner brilliant margin could be seen shining like a star. Certain now as to the identity of the moth, I advanced still nearer, and finally I found that it was not till the point of a stick was used to move and shake the earth on which it lay, that the creature could believe that it was in danger. Then, in an instant the crumpled leaf became a living moth with powers of flight, which would have defied capture.

I recollect that many years ago Mr. Wallace kindly showed me a butterfly from the Eastern Archipelago whose upper wings were of a brilliant colour, but which, by the simple act of alighting on a branch, and of folding or closing its wings, became transformed into the perfect likeness of a growing leaf—a likeness so perfect that even the closest inspection only discovered new items of resemblance—inasmuch as the leaf-stalk, as well as the venation of the leaf, were all perfectly represented both in the structure and in the colouring of the under-surface of the wings.

I confess that the number and intricacy of the correlated growth and instincts which are involved in these phenomena strike me more and more as wholly outside the sphere of mere physical causation—by which I do not mean that physical causation has not had its own share of instrumentality in the matter, but that it affords no satisfying explanation of all the elements involved. The ordinary phrases of the Natural Selection Theory appear in the light of such facts to be little better than lean and empty formulæ.

ARGYLL

Cannes, November 29

Double Flowers

I AM indebted to Baron von Mueller for the communication of double flowers of *Tetratheca ciliata*, which possess interest on several grounds, although the changed appearances they present are not infrequent. It may be well to premise (1) that the plant, like all its fellows of the same order (Tremandraceæ), is native to extra-tropical Australia; (2) that, under ordinary circumstances, it has 4 free sepals, 4 free petals, 8 free stamens in a single row, and a two-celled ovary; (3) that "doubling," in a strict sense, is brought about by the multiplication of petals, or by the more or less complete substitution of petals for stamens, or pistils, or both.

The Australian origin of the plant in question is so far of interest, in this connection, that it affords one more illustration of the occurrence, under natural conditions, of double flowers in a division of the globe where, according to the late Dr. Seemann, such forms are rare. The rarity, however, I believe, is not so much in the existence of such flowers, as in the number of observers, at any rate we now know of several cases of the kind.

Some of the flowers sent by Baron von Mueller were double by multiplication of petals, *i.e.* there was a second row of petals inside the first, others were double not only by multiplication of petals, but also by the partial substitution of petals for stamens; thus in one of these last-mentioned flowers, there were four sepals, three rows of petals, one of the innermost row being partly staminoid, and eight stamens in a single row. Of these eight stamens, six were perfect and the remaining two partially petaloid, one lobe of the ordinarily 4-celled anther being destitute of pollen, but enlarged into a relatively large petal-like lobe with inflected margins. So that according to the old notion, this flower affords an instance both of progressive and of retrogressive metamorphosis, of enhanced and of arrested development associated with compensatory changes. On the hypothesis revived by Mr. Grant Allen—for it is no new notion—the two outer rows of petals would be stamens flattened out of all knowledge, while the inner row and the staminal whorl would, I presume, also afford him evidence of the truth of his opinion. For my own part I prefer to adhere to the established order of things, in which the horse precedes, rather than follows the cart, and I do so because to do otherwise would be to run counter to what we know of the homologies of the foliar and floral organs, of leaf-buds and flower-buds, and to ignore or rather to reverse what we know of the mode and order of development of flowers in general.

Not being aware of the precise order of evolution in the flower in question, I can only reason from analogy when I express my opinion that the changes it presents and the order of arrangements of its parts from the leaves on the flower-stems up to the pistil are more consistent with the generally adopted views of morphology than they are with Mr. Grant Allen's. According to his views, so far as I understand them, I can see no reason why the sepals as well as the petals should not be flattened stamens, and if the sepals why not the bracts? if the bracts why not the leaves? The theory would thus do away with the possibility of indigestion in plants, or at least the primordial plant, could not have been troubled in this way, for it would have had no digestive organs.

I have only to add that the flowers in question offered no explanation of the great peculiarity presented by the existence of a single row of stamens in number double that of the petals. Possibly this may be the result of bifurcation at a very early stage of development. It was hardly to be expected that they would throw any light on the equally curious "obdiplostemonous" arrangement in the nearly-allied genus *Platytheca*, in which there are two rows of stamens, the outermost being superposed or opposite to the petals, instead of being alternate with them, as is usually the case in stamens so placed. A possible explanation of this in a sense partly consistent with Mr. Allen's views would be to consider the petal as in this case an outgrowth from the stamen, and not a separate organ, a view that has been propounded in the case of Primulaceæ and some Malvaceæ.

MAXWELL T. MASTERS

Fruit of Opuntia

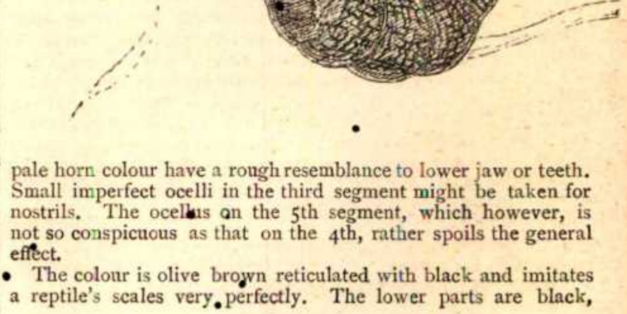
DR. ERNST'S abnormal fruit of Opuntia, as figured at p. 77, appears to be similar to one described and illustrated by Zuccarini (*Abhandl. d. math. phys. Class.*, B. iv., Abth. 1., tab. ii.) in the case of *Cereus serpentinus*, but as Dr. Ernst gives no details as to the arrangement of the vascular bundles, it is impossible to say that the two cases are exactly parallel. The resemblance to certain gourds (Cucurbits), wherein the upper part of the fruit protrudes beyond the dilated end of the flower-stalk, may also be pointed out.

MAXWELL T. MASTERS

Hawk Moth Larva

I FORWARD a sketch of the larva of a hawk moth found in the Khasi Hills, Assam, in the position it assumes when disturbed. Its resemblance to a snake will be at once evident.

The head (just visible in the sketch) and two first segments of the body are retracted, and the third pair of legs of pale horn colour have a rough resemblance to lower jaw or teeth. Small imperfect ocelli in the third segment might be taken for nostrils. The ocellus on the 5th segment, which however, is not so conspicuous as that on the 4th, rather spoils the general effect.



The colour is olive brown reticulated with black and imitates a reptile's scales very perfectly. The lower parts are black, and a portion of the anterior segments dirty yellowish white. I do not yet know the perfect insect. The larva feeds on the wild balsam. The general colour of this larva at once reminded me of two abnormally coloured larvæ of the common death's-head moth that I had brought to me from a potato field in Jersey some years ago, together with others of the ordinary colour.

One was full grown and another half grown. The general colour of these was brown with fine black markings and without a trace of green. The anterior segments were a pale dirty cream colour. There were no ocelli or diagonal stripes on the sides.

I have not seen recorded any similar case of abnormal colouring in the larva of the death's-head moth, but the fact is interesting as indicating a common ancestry in two moths which are probably now classed in different genera.

E. R. JOHNSON

Surgeon Major, Bengal Medical Department

Shillong, October 16

[The form of death's-head larva alluded to is not uncommon; it is a dimorphic condition and finds its parallel in many larvæ of Sphingidæ. ED.]

The Fertilisation of the Common Speedwell

ALTHOUGH it is the wrong time of the year for observing flowers, it will perhaps not seem out of place to draw the attention of your readers to the fertilisation of the common Speedwell (*Veronica officinalis*). The flowers in the plant hang downwards, so as to bring the nearly flat corolla a little under the perpendicular. The two stamens project outwards and downwards on each side of the pistil, which also hangs down, but not so much as the stamens. These latter are very much narrowed at the base. The flower is in this species, proterandrous, and the corolla, as soon as the stamens have shed their pollen, becomes slightly loose.

It at first sight seems quite impossible for either cross or self-fertilisation to take place, as the stamens are quite away from the pistil, and, owing to the position of the flower, insects are compelled to alight in front.

One morning last summer, however, in considering the structure of the flower, &c., I was led to conclude that the explanation must lie in the insect's mode of settling upon it, and accordingly watched two or three plants. In about half an hour's time I had the pleasure of seeing a large fly in the act of fertilisation. As the corolla was flat, and the flower hung down, there was no foothold there, so the insect clasped each of the stamens with its forefeet. Being thin at the base, they were drawn together, and the anthers meeting just below the pistil, dusted the front of its head with the pollen.

On comparing a large number of flowers, I found that when just open, the pistil stood up above the point at which the two anthers would meet, but that in older flowers, especially after the anthers had shed their pollen, it was inclined downwards, If this observation is verified, it will show a most striking adaptation for preventing self-fertilisation.

I may add that in one of the smaller flowered species, *V. hederæfolia*, the stamens and pistil are quite close to each other, so that self-fertilisation must here be the rule. The corolla is also not so easily detached.

A. MACKENZIE STAPLEY

The Owens College, Manchester, November 20

Wartmann's Rheolyzer

YOU gave in NATURE a report on "Wartmann's Rheolyzer." I beg to say that I invented and constructed the same apparatus long ago, and described it in the "Sitzungsberichte d. Wiener kast Akademie d. Wissenschaften," July, 1877, under the name of "Rheonom." Some months after that a fair report of my paper appeared in "Beiblätter zu Wiedemann's Annalen." My instrument was for some years in the hands of several physiologists. Prof. Yeo was present when I made experiments with it in Prof. Ludwig's laboratory in Leipzig in the year 1878, and Prof. E. du Bois-Reymond has it also in his collection of physiological and physical instruments for more than five years. There is no doubt that Prof. Wartmann was not acquainted with my apparatus when he described his, but I cannot be expected to see my invention ascribed to another and keep silent. So you will oblige me very much in correcting the above-mentioned mistake in your paper.

ERNST VON FLEISCHE

Vienna, Währingerstrasse 11, November 30

Pollution of the Atmosphere

THERE was a letter in NATURE some time since, calling attention to the pollution of the atmosphere by the burning of coal; and it was calculated that in the year 1900, all animal life would cease, from the amount of carbonic dioxide; but the author had overlooked the fact that the rain is continually cleansing the atmosphere of this, and the fall of this rain on the ground, and the combination of this with various salts; besides, the oceans alone would absorb their own bulk at normal pressure, but at an increased pressure of, say half a mile deep, would dissolve more than we are likely to need for hundreds of years.

But there are other products of combustion, or rather of incomplete combustion, that are not brought down in this manner by rain, as hydrogen and the hydrocarbons, chiefly marsh-gas and ethylene. The latter has, I believe, been observed by the spectroscope on the Alps, and was supposed to have come from space.

Since the year 1854 (as near as I can estimate) there has been burnt 10,000 million tons of coal; and if we say (in its consumption by household grates, leakage by gas-pipes, &c.) 1-100th escapes, then 100 million tons of hydrogen and hydrocarbons are floating in the atmosphere, or 1-10,000,000th part in bulk; if we say the average proportion of hydrogen to be ·45, and of marsh gas ·35, and of ethylene ·4, we have ·84 per cent. of gases that are lighter than air, and it is more than probable that the law of diffusion of gases, as demonstrated with jars, does not apply to the atmosphere. The cases are not parallel: in the air we have unconfined space, pressure, and temperature diminishing infinitely, conditions favourable to the lighter and the gas with the greater amount of specific heat rising and maintaining its elevation, especially as we know that in *large* halls carbonic dioxide is found in larger quantities on the floor. According to Prof. Tyndall's researches, hydrogen, marsh gas, and ethylene have the property in a very high degree of absorbing and radiating heat, and so much so that a very small proportion, of only say one thousandth part, had very great effect. From this we may conclude that the increasing pollution of the atmosphere will have a marked influence on the climate of the world. The mountainous regions will be colder, the Arctic regions will be colder, the tropics will be warmer, and throughout the world the nights will be colder, and the days warmer. In the Temperate Zone winter will be colder, and generally differences will be greater, winds, storms, rainfall greater.

H. A. PHILLIPS

Tanton House, Stokesley, November 23

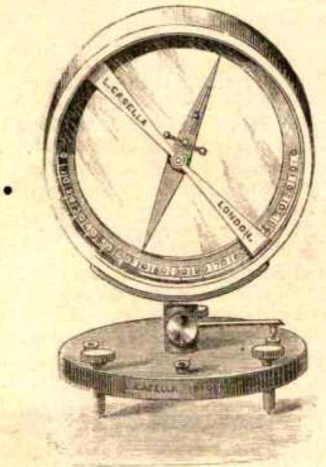
A Modern Rip Van Winkle

WHEN Mr. Evans asks whether it is impossible for "the so-called flint implements and flint flakes to have been formed by natural causes" he surely must have had a scientific nap of forty or fifty years. He can answer his question by going to any good museum and inspecting the beautifully and clearly manufactured implements which the Curator will show him.

November 28 SALTBURN

GOOLDEN'S SIMPLE DIP-CIRCLE

A DIPPING-NEEDLE suitable for the requirements of schools and science classes has long been a desideratum, there having been no instrument obtainable hitherto which would at a moderate cost afford results of sufficient accuracy. Between the mere needle suspended in a simple stirrup of brass, and the delicate and complicated dip circles of standard pattern there has been no intermediate form of instrument. This deficiency, has, however, been remedied by Mr. Walter Goolden, M.A., Science Master in Tonbridge School, who, in conjunction with Mr. C. Casella, has designed the form of portable dip-circle depicted in the figure, which possesses several novel points. The needle, which is 3½ inches in length, is poised upon an accurate axis working in sapphire centres, and magnetised once for all. In order to ensure the coincidence of the centre of gravity with the centre of suspension, two very light adjustible counterpoises are fixed to the needle, one of them being capable of being moved parallel to the length of the needle, the other lying at right angles to the first, and adjustible in a direction to the right or to the left. The metallic circle within which the needle revolves is graduated on both faces, and is inclosed within an air-right case. The instrument turns upon a vertical support above a solid metal plate standing on three levelling-screws. A small loose level, which can be placed upon this levelling-plate, accompanies the instrument. The main novelty in Mr. Goolden's instrument, consists, however, in the arrangements by which the angle of dip may be determined without having either a horizontal graduation or a horizontal compass needle upon the apparatus. It will be seen by reference to the figure that the vertical axis of the instrument is furnished with a spring-arm, which can be clamped to it by turning a screw, and that there are four metal studs affixed to the stand at equal distances apart, into any one of which the pin at the end of the spring arm can be pressed down. These arrangements serve to facilitate the following adjustments. Having levelled the instrument the spring-arm must be unclamped and the pin at the end of it pressed down into the conical hole in one of the studs. While this is so held with one hand the vertical circle is turned upon its axis with the other hand *until the needle points directly vertically downwards to* 90°. In this position, which is of course exactly magnetically East-and-West, the vertical circle is clamped by a turn of the screw. The position is verified by turning the whole circle and spring arm together upon the axis until the pin meets the opposite stud, when the needle will again point vertically downwards. The East-and-West position being thus verified, it is clear that the magnetic meridian will lie in a plane at right angles to this. Hence the next process is to turn the circle round and press the pin into one of the two studs which lie at right angles to the pair already employed. The position of the needle in the circle is then read off. The circle is once more turned through a complete semicircle, the pin pressed into the opposite stud, and another reading is taken: the mean of these two being accepted as the true angle of dip. It will be seen that the usual elaborate processes of eliminating possible errors by reversing the needle-axis upon its bearings and reversing the magnetism of the needle itself are not attempted. Everything will therefore depend upon the accuracy of the adjustments of the instrument before it leaves the maker's hands. As it is, it is claimed that the readings are correct to within 10 minutes of arc.



Goolden's simple dip-circle.

THE COMET

MR. CHANDLER has made another approximation to the orbit of this comet, and now finds the following ellipse (*Science Observer*):—

Perihelion Passage September 17·2304 Greenwich M.T.

Longitude of perihelion	276° 28′ 26″·8	M. Eq. 1882·0.
,, ascending node	345 50 34·0	
Inclination	38 5 3·8	
Log. perihelion distance	7·8835636	
Eccentricity	0·9999700	

Retrograde.

The period of revolution corresponding to this ellipse is about 4070 years; in the middle of November there was a decided difference between the calculated and observed positions, part of which may be due to a cause to which Mr. Chandler has already drawn attention, viz. that the same point in the head of the comet may not have been always observed. We may now say pretty confidently that a short period of revolution is inconsistent with the motion of this comet, and consequently that it is not identical either with the great comet of 1880 or with that of 1843. Nevertheless we must repeat that there are indications of sensible perturbation during the flight through the coronal regions of the sun.

Mr. Gill sends us some particulars relating to the early Cape observations of this comet. It was first detected by Mr. Finlay at five o'clock on the morning of September 8, as he was returning to his house from the observatory. He went back and compared the nucleus with a small star in its immediate neighbourhood. On the following morning the comet was observed again, and the same day Mr. Gill sent the following telegram to Sir James Anderson, Chairman of the Eastern Telegraph Company:—"Kindly tell Astronomer Royal, Greenwich, that bright comet was observed here yesterday morning by Finlay. Right Ascension this morning nine hours forty minutes, increasing daily nine minutes, Declination one degree south, increasing half degree south daily." Mr. Gill acknowledges his indebtedness to the courtesy and liberality of Sir James Anderson for the free transmission of many previous messages. Unfortunately this one notifying the discovery of the comet in some way miscarried, and did not reach Mr. Christie's hands, so that the first intimation of the visibility of the comet came from Mr. Cruls at Rio de Janeiro, who, however, so far from being a discoverer, has informed the Academy of Sciences of Paris, through M. Faye, that he received notice of the comet's presence from another quarter on September 10; it was not seen at the observatory of Rio till 5h. 15m. a.m. on September 12.

Cloudy weather prevailed at the Cape between September 10 and 17, and very few observations could be procured, and those had to be made by measuring the difference of altitude and azimuth from bright stars. On Sunday, September 17, the comet was easily visible with the telescope in full sunshine, and in close proximity to the sun. It was followed during the day by Mr. Finlay and Dr. Elkin, and towards afternoon was found to be rapidly approaching the sun. As the distance diminished "all appearance of tail was obliterated, only a round disc about 4″ in diameter remained visible, but this was intrinsically as brilliant as the surface of the sun, if not more so. Still closer this disc approached to the sun's edge, and its disappearance there was observed just like that of a star when it was occulted by the bright limb of the moon." Both Mr. Finlay and Dr. Elkin observed the disappearance, but though the former was using much the more powerful telescope, he only saw the nucleus five seconds longer than Dr. Elkin; the comet had passed on to the sun's disc (not behind it, as Major Herschel erroneously assumes in NATURE last week), but no appearance whatever of its presence there could be perceived. Mr. Gill himself was not able to arrive at this unique observation, having proceeded to Simon's Bay to meet Capt. Morris, R.E., on his way in the *Liguria* to Brisbane to observe the transit of Venus, who returns to South Africa as chief Executive Officer of the Geodetic Survey of the Cape Colony and Natal; but on the morning of the following day he observed the comet rise just before the sun at Simon's Bay, and says he will never forget the beauty of the scene. Many drawings of the comet were made at the Cape Observatory, and some photographic pictures were obtained with the assistance of Mr. Allis, of Mowbray. To obtain a perfect picture of the more delicate details of the comet, an exposure of not less than half an hour was found to be necessary.

The following places are abbreviated from an ephemeris calculated by Mr. Chandler from his last elliptical elements :—

At Greenwich mean noon.

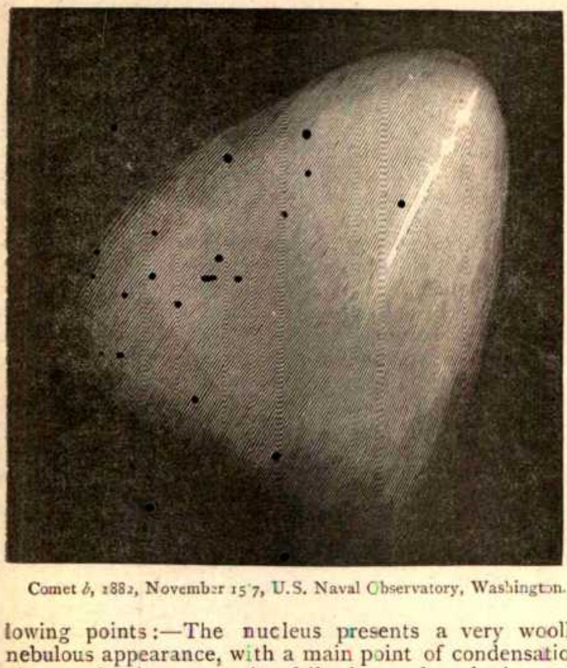
	Right Ascension. h. m. s.	Declination. ° ′	Log. distance from Earth.	Log. distance from Sun.
December 7	8 31 41	−29 42·7	0·1868	0·3110
9	8 25 28	29 57·6		
11	8 19 10	30 9·8	0·1917	0·3250
13	8 12 48	30 19·1		
15	8 6 24	30 25·5	0·1978	0·3384
17	7 59 58	30 28·9		
19	7 53 33	30 29·3	0·2051	0·3512
21	7 47 9	30 26·8		
23	7 40 49	−30 21·4	0·2137	0·3635

Up to Nov. 6 the comet discovered by Mr. Barnard had been sought for unsuccessfully at the Cape Observatory.

We have received the following communications on the comet :—

WITH the permission of Vice Admiral Stephen C. Rowan, U.S.N., Superintendent of the Observatory, I send you a sketch made at 17h. Washington Mean Time, November 15, with the 26-inch Washington equatorial. At the time of observation the head of the comet was about 45 minutes east of the meridian.

As it is extremely difficult to represent such an object faithfully in a woodcut, I will call attention to the following points:—The nucleus presents a very woolly, nebulous appearance, with a main point of condensation, almost circular; near its following end, and about 18″ from this towards the tail, a second point of condensation, prolonged about 54″ in the direction of the tail in a narrow ridge of light. This ridge which has heretofore appeared broken up into four or five beads, is now a continuous line of light with, perhaps, in one or two places, faint indications of condensation. The nucleus is decidedly eccentric with regard to the general direction of the head, and the head is flattened on the *north-following* side.



Comet *b*, 1882, November 15·7, U.S. Naval Observatory, Washington.

The position-angle of the major axis of the nucleus was 309°·4. The distance between the centre of the two main points of condensation, from a series of measures with the filar micrometer was 18″. A magnifying power of about 200 diameters was used. On November 17·7 the extreme length of the nucleus was found by Commander Sampson to be 74″.

The following meridian observation for position was obtained on November 15·7 with the transit circle ;—

1882 November 15·74 (Washington M.T.)

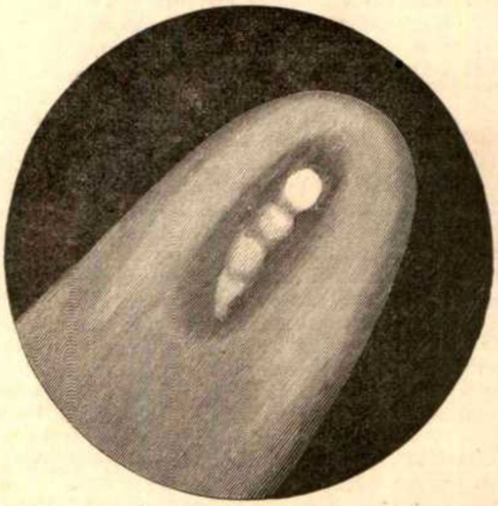
R.A. ... ... 9h. 27m. 50s.·72

N.P.D. ... ... 114° 49′ 18″·9

The part observed was the main point of condensation near the following end of the nucleus. The observation is corrected for refraction, but not for parallax.

WILLIAM CRAWFORD WINLOCK,
Assistant Astronomer, U.S. Naval Observatory

THE drawing represents the appearance of the great comet at 5 a.m. on the morning of October 12 this year. I delayed the publication of my observations on this morning in the hope of securing some more views, but the bad weather prevented any further observations of this object here. The drawing shows distinctly four condensations in the nucleus, whose angle of position on the 12th was about 102°. Its length was 40″·3, as measured with the filar micrometer on the great refractor. The visible length of the tail was estimated at 21°. No doubt it was really much greater. Its southern side was well defined. As seen with the naked eye the nucleus shone as brightly as a star of between the first and the second magnitude. On the morning of the 6th I had seen the end of the tail, which was then apparently 15° long, present a feature very like that indicated in Major Herschel's drawing (NATURE, vol. xxvi. p. 622), but I am not sure of this, as the sky was partly covered with cirro-cumulus clouds.



The Great Comet seen in the Markree Refractor, October 12, 1882, 5 a.m., by W. Doberck.

On October 28, at 5h. 45m. a.m., the angle of position of the nucleus was about 113°, and its length amounted then to 67″. The tail was less curved than on the 12th.

Markree Observatory, December 2 W. DOBERCK

FUNCTION OF THE MEMBRANA FLACCIDA OF THE TYMPANIC MEMBRANE

WHY should a smart blow, as, for instance, with the palm of the hand on the side of the head, or on the wing of the ear, cause rupture of the membrana tympani?

It was in endeavouring to trace the connection between these events, of no very uncommon occurrence, that I was led to the discovery of a most important factor in the physiology of the ear, and one which gives a new and more rational significance to the mechanism of the ossicles and membrane. In the shape of anatomical details I have nothing new to adduce, but in exhibiting the relationship of a series of minute particulars hitherto enigmatical and glanced at separately and only casually by anatomy, I have obtained a valuable result for otology. I must here present those details in the order most convenient for a brief demonstration, giving only the main features.

The membrana tympani, though but a single membrane, consists of two portions. The lower is firm and transparent, and of conical shape, being attached along its centre to the handle of the malleus, and fixed round its whole circumference to the sulcus tympanicus. The upper is comparatively loose, and much less transparent, and being in reality mainly fastened to the skin of the upper wall and only slightly to the bone, there being here no sulcus, but only a smooth margin (margo tympanicus), is easily displaced with a little gentle pressure outwards or inwards. Between the two there is a line of dense fibres forming a ligament, called by Helmholtz the anterior ligament of the membrane, and towards the anterior border of which the short process of the malleus is inserted. With this marked limiting line there is thus a striking difference in the character and mode of attachment of the two portions of membrane, and this reaches to the very foundations of the structures, and is the most remarkable feature in their development. It is to be remembered that the superior arch of bone, forming at its inner end the tympanic margin alluded to, is part of the squamous bone, which is characterised by the general smoothness of its surface—a character it preserves along the whole upper wall of the osseus meatus, not excepting its termination at the porus acousticus externus, where it presents a smooth bevelled edge. But the os tympanicum which forms the inferior arch of bone is contradistinguished by the general unevenness or asperity of its surface, not only being hollowed out by the sulcus at its inner end, but along the whole floor, maintaining a roughness which culminates in its rugged edge at the porus externus. Nature, in constructing the meatus, selects one bone for its smoothness, another for its roughness, and the evident intention is, that what is laid on the one surface shall adhere, what is laid on the other shall glide over it. While, therefore, the lower portion of the drum of this ear is fixed by its connection with the os tympanicum, the upper portion is loosely connected with the os squamosum, which affords it a movable surface. Helmholtz believes that the lower firm portion is alone concerned with sound-waves, the upper lying above the handle of the malleus, and having therefore no direct connection with the chain of ossicles. On this ground, in his treatise on the mechanism of the membrane and ossicles, he leaves the membrana flaccida out of consideration altogether, and no physiologist, as far as I am aware, has ever hinted at its function. Having from the foregoing description obtained an insight into its relation with the bone, it must now be viewed in connection with the skin lining the upper wall of the passage, which is quite distinct in character from that covering the rest of the osseous passage, and next needs to be specially noticed.

Prof. Henle says: "The skin which covers the external meatus has originally the appearance and structure of the cutis, and retains this character along *the upper wall* beyond the *rounded rim* of the squamous bone which helps to complete the porus acousticus externus up to the site of the membrane, whereas in the rest of the circumference the skin, in passing from the cartilaginous to the osseous meatus, abruptly changes its character, decreasing in thickness and assuming the peculiar silvery glance of a fibrous skin.'"[1] Thus along the whole passage the skin on the upper wall retains its ordinary character, being elastic and movable, and having, as noticed by Von Fröltsch, the same kind of loose connective tissue glands and hair cysts as any other part, whereas the movability of the remaining portion ceases with the cartilaginous meatus, as beyond that it ceases to be true skin.[2] Add to this that the one lies on a roughened, the other on a smooth surface, and this singular deviation in apparently so simple a matter and in so minute a particular, must strike the examiner as significant of purpose. If we next turn to the arrangements at the porus acousticus externus, it becomes manifest.

What is noticeable in regard to the rim of bone constituting the porus is simply corroborative of what has already been said. Thus, whereas the under semicircle is comparatively rough and uneven, and projects slightly beyond the upper semicircle, the latter has a smooth-rounded edge bevelled in the manner of bone over whose margin a tendon plays. It is to the curved uneven lamella of the under circumference known as the auditory process that the cartilaginous meatus is principally attached. This is effected by means of strong, slightly movable ligamentous tissue, or rather, as Henle puts it, "by means of a compact cartilaginous substance richly interspersed with elastic ligamentous tissue, which fills up the rough interspaces of the lamella and extends the lower portion of the osseous canal about two millimeters."[3] The upper semicircle, on the contrary, is closed simply by a dense fibrous membrane, there being here a large deficiency of cartilage (Quain). The difference is that while below the osseous canal blends insensibly into the cartilaginous with only dawning facility for movement, above it terminates abruptly, admitting there and then a large measure of movement.

Thus then it appears that from the membrana flaccida of the membrane, which is easily movable at its margin, we have a piece of movable skin running over a smooth polished surface along the whole upper meatus of the bone, which is here bevelled off, and is immediately continuous with the movable membranous roof of the cartilaginous portion of the external passage. The movable piece of skin serves, after its manner, the purpose of a tendon, and the muscle which mainly plays upon it is attached to this upper membranous wall at its point of junction with the osseous meatus.

Of this muscle Henle gives the following account:—"Of the lateral portion of the musculus epicranius (occipito-frontalis), the musculus epicranius temporalis is a very thin bundle of fibres, and is anterior to, and smaller than the attollens auriculam, which forms the remainder of the lateral portion. It has its tendinous origin below the root of the zigoma, near the rim of the osseous canal, to the capsule of the inter-articular cartilage (operculum cartilagineum), and to a tendinous arch through which the vasa temporalia pass into the deep structures. Its muscular fibres spread out in parallel lines forwards and upwards, some of them stretching to the border of the frontalis, and of the orbicularis oculi, and so partly curving upwards around the lateral border of the frontalis, and intermixing with the upper fibres of the orbicularis, they are finally inserted into the glabella."[4]

It will thus be observed that, when the muscle contracts, it raises the membranous roof of the canal upwards and slightly forwards, making the movable patch of skin glide outwards, and so telling upon the membrana flaccida, which is, even in the adult, almost in a line with the upper wall, and is therefore so much the more easily influenced by such a movement. When the delicacy of the parts concerned are borne in mind, it will be obvious that no extensive movement is thus indicated, and in a more complete demonstration a good deal further illustrating the actual movement, has to be said on that head. Here we have space only for a general outline.

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[1] "Anatomie des Menschen," Z. B. s. 732.

[2] "Diseases of the Ear," Roosa's Translation, p. 53.

[3] *Loc. cit.* p. 722.

[4] *Loc. cit.*, s. 136.

The muscle, of course, has no isolated voluntary action, but its effect is brought into play when the eyebrows are forcibly raised by the contraction of the occipito-frontalis. Indeed, although itself really a muscle as described, much of its effect is derived after the fashion of an elastic tendon connected with the great epicranial muscle. It is further assisted by the consentaneous action of certain small muscles of the auricle, notably the attollens auriculam. Its movement is quite perceptible to the finger placed in the sulcus, between the pinna and side of the head, and to an experienced eye its effect on the membrane is distinctly visible through the speculum when the occipito-frontalis is made to contract.

It would be beyond the scope of a single paper to enter into a demonstration of the effect of this movement of the membrana flaccida on the membrane and ossicles—but it can be shown that, in opposition to the so-called tensor tympani muscle, it helps to bring the umbo or deepest part of the membrane outwards, thus tending to reverse the cone, and bring the membrane generally into a more vertical position, relatively to the lower wall of the meatus. This is beyond all question its position for acutest hearing, and it is thus important to observe that by the single contraction of the occipito frontalis muscle, both eyes and ears are brought simultaneously into the attitudes of strained attention. Hence, in endeavouring to hear as well as to see intently, we involuntarily raise the eyebrows in order to tell upon the drum of the ear.

A smart blow administered on the side of the head, as is too often thoughtlessly done by schoolmasters and parents in correcting children, may cause sudden spasmodic action of the muscle, and thus, through the action of the mechanism described, serious injury or even rupture of the drum.

JOHN M. CROMBIE

WEIGHTS AND MEASURES

THE Board of Trade lay before Parliament an Annual Report of their proceedings and business under the Weights and Measures Acts, &c., and their Report for the current year has just been issued.

It is required by law that the three Parliamentary copies of the Imperial Standards of measure and weight, which are deposited at the Royal Mint with the Royal Society, and in the Royal Observatory, respectively, should be compared with each other once in every ten years. The period for such decennial comparisons having recently arrived the Board took the necessary steps for the removal of these Standards to their office. The methods of comparison adopted and the actual differences between the Standards are shown in a memorandum by Mr. H. J. Chaney, which is attached to the Report. It appears that the comparing apparatus in use at the Standards Office is found to require alteration, and that in considering the changes necessary to be made the Board have had the valuable assistance of a Committee of the Royal Society, composed of Sir G. Airy, Major-Gen. A. R. Clarke, and Prof. Stokes. It is really important that a department which is charged with the care and use of our national standards, should have the best apparatus, and we trust, therefore, that the Report of the Committee may be speedily and fully carried out.

Reference is also made to the papers issued by the Comité International des Poids et Measures, Paris, and the Report acknowledges the assistance the Standards Department has received from these papers, particularly with reference to the measurement of heat and the determination of volume and weight. This country is the only civilised country which has not joined the Comité International, and taken part officially in their proceedings, although it would appear that it has not failed to avail itself of their labours.

The two ancient standards of the metric system, the Toise du Perou and the Toise du Nord, are stated to be still at the Paris Observatory, in a good state of preservation, as also are the measures used by Borda, Brisson, and Lavoisier. By a decree of the Sultan, the metric system came into force in Turkey on March 1st last, and the equivalents of the old and new Turkish weights and measures are stated in this Report.

The Board have had their attention directed to the question of a uniform system of screw threads, as well as to that of a standard wire gauge. Reference is made to the want of uniformity in the system of screw threads used in the construction of scientific and optical instruments. It is hoped that the attention which is now being given to this question may result in the adoption of a standard system of screw threads. Any step which tends to lessen the high cost of construction and of repair of scientific apparatus is to be welcomed.

From time to time, as science advances and commerce extends, it is found that new kinds of standards are needed, and the attention of the Department has therefore been this year called to the expediency of adopting new photometric tests for gas, and also as to possible means of measuring electrical energy. In the proposed Bill for amending the enactments for regulating the sale of gas, and of dealing with the mode of testing the illuminating power of gas, we trust that Mr. Vernon Harcourt's new air gas-flame test, on which Dr. Williamson and Dr. Odling have reported, may receive favourable consideration.

Under the Petroleum Acts rules are laid down for determining the "flashing-point" of oils, or the temperature at which they begin to give off inflammable vapours, but it appears by the Report that Dr. Foerster has lately called attention to the omission in these rules of any allowance for variations of atmospheric pressure. The rules in this respect evidently, therefore, require some amendment.

The Report also contains much information valuable to local inspectors and others practically interested in weighing and measuring.

ON THE PROPOSED FORTH BRIDGE

IN offering some remarks (which I trust may be final) merely explanatory of preceding notes on this proposed structure, I shall refer generally to my letter of October 19 (NATURE, vol. xxvi. pp. 598-601).

First, I have to modify the force of my expressions relating to the danger arising from the use of certain long struts to support very heavy end-pressures. My remarks were the consequence of error in the engraved longitudinal vertical plan, circulated (I understood) under the authority of the Official Board. In this plan, by the indiscretion of the engraver, the tubular struts of 340 feet length and 240 feet length respectively, are drawn clearly and distinctly as unconnected in their entire length with any other braces. In other parts of the plan, each connection of that class is indicated by a rose; but there is no such mark upon these rods. A person scrutinising the plan might well feel alarm at the prospect of unbraced rods 340 feet long, intended to support end-pressures exceeding 600 tons. But Mr. Fowler has kindly informed me that the plan is erroneous, and that there is connection at each place where the strut crosses a brace, and that the flexible length of the strut is thus reduced to 170 feet. This diminishes the danger of buckling in a vertical plane so greatly that I imagine it may be passed without further notice. Still I remark that the danger of buckling in a horizontal direction, with a length of 340 feet, remains undiminished, unless it is counteracted by bracing not known to me.

In regard to some effects of the wind, the following comparison between the proposed Forth Bridge and the late Tay Bridge may be interesting. I suppose that equal trains are upon the two bridges; and I assume that the force of the wind on the Tay Bridge train tore one pier from its foundation-attachment. (I imagine that the ruin of the bridge commenced thus). The height of the centre of the Tay Bridge train was about 92 feet, and the momentum of the wind was, therefore, wind × train × 92 feet. (The reader will easily interpret my brief notation). To resist this there were three pairs of attachments to the foundation, with lever-widths of 10 feet, 22 feet, 10 feet, respectively. So that, supposing the holding powers of each attachment the same, we must have had for momentum of resistance, one Tay-attachment $\times (10 + 22 + 10)$ feet. At the instant of breakage, this was equal to the momentum of the wind, or to wind × train × 92 feet. So that one Tay-attachment $= \frac{92}{42} \times \text{wind} \times \text{train} = 2{\cdot}19 \times \text{wind} \times \text{train}$. If we treat the proposed Forth Bridge in the same manner, we must use, length of lever about 660 feet, and two pairs of attachments of the cantilever to the pier (if I read the plan correctly), at distances of 30 and 120 feet. And thus we shall have the equation at a moment of breakage.

One Forth-attachment $\times (30 + 120) = \text{wind} \times \text{train} \times 660$; or one Forth-attachment must $= 4{\cdot}4 \times \text{wind} \times \text{train}$, or double that required for the Tay Bridge.

A numerical value (possibly subject to modification) may be given thus:—Suppose the surface of a train to = 3000 square feet. With the Government scale of 56 lbs. for high wind, the lateral pressure = 75 tons; and, using leverage numbers as above, one Forth-attachment = 330 tons. And this is the strain which each attachment must be able to sustain in respect of resistance to the effect of wind upon a train. I imagine that this has been provided, at least in great measure; but I think it desirable that attention should be called to the magnitude of the forces here concerned.

The able and experienced engineer who has undertaken the prosecution of this great work, will, I am confident, recognise the possibility of serious inconvenience (yet unforeseen) arising from the points to which I have alluded in NATURE, vol. xxvi. p. 599—the novelty of plan, at least in this country—the magnitude of plan—the want of experience in a rising scale of magnitude. Should the bridge be erected successfully, I can imagine that many difficulties on small points might arise. For instance:—all matter yields to force; the brackets of furlong-length, could not strictly preserve their form under the passage of a train; the connection of the end of one bracket with the beginning of the next is not very perfect, and I can hardly imagine that trains could be run through at speed (which, as I understood, is one of the conditions to be secured).

I still prefer the principle of suspension. I would propose for further consideration the modifications which I have suggested in NATURE, vol. xxvi. p. 600, for giving enlarged width with diminished height to the top of the piers, and for use of wire in forming the suspension-chains.

G. B. AIRY

The White House, Greenwich, December 4

NOTES

MONDAY'S sitting of the Paris Academy of Sciences was one of unusual interest. M. Jamin, who was in the chair, delivered an eloquent address on the services rendered to science and to the Academy by M. Dumas, and presented to the illustrious Perpetual Secretary the medal subscribed for by his admirers as a testimonial on the occasion of the fiftieth year of his nomination as an academician. The medal is accompanied by silver and bronze replicas. The whole of the audience, which was very numerous, broke into enthusiastic plaudits. When the enthusiasm subsided, M. Dumas returned thanks, which he did with masterly eloquence.

WE regret to announce the death of the Rev. James Challis, M.A., F.R.S., Plumian Professor of Astronomy and Fellow of Trinity College, which took place on Sunday morning at his residence in Cambridge, after a long illness. The late Professor was born in 1803, and educated at Trinity College, where he graduated B.A. in 1825 as Senior Wrangler and first Smith's prizeman. In 1836 he was elected Plumian Professor of Astronomy in succession to Mr. (now Sir) G. B. Airy, and also held the important post of Director of the Cambridge Observatory. The latter post he resigned in 1861, and was succeeded by Prof. Adams. He was at the time of his death the Senior of the Professors at Cambridge, and until about two years ago personally discharged the duties of his professorship, when increasing age and infirmities compelled him to appoint a deputy. Prof. Challis has published a considerable number of scientific works, including twelve volumes of astronomical observations.

THE death is announced of Dr. Gustave Svanberg, formerly Professor of Astronomy and Director of the Observatory of Upsala University. He died on November 21, in his eighty-first year.

NEWS from Aden reports the death of Marchese Orazio Antinori, the well-known zoologist and African traveller, who had recently started on a new expedition to the Upper Nile. He was seventy-one years of age.

ELABORATE preparations were made in various parts of America to observe the transit of Venus yesterday. The Western Union Telegraph, to facilitate observations, arranged to transmit Washington time wherever desired, in order to secure accuracy in recording results. Some enthusiastic astronomers had proposed general prayer in the churches on Sunday last for clear weather.

M. W. DE FONVIELLE has published the first number of a new astronomical journal, called "Les Passages de Venus," which explains the great astronomical event, and is being sold in the streets of Paris at 1 sou, with illustrations indicating the phases, and giving instructions for their observation in France. The editor states that he trusts that the second number will appear at the right date, June 8, 2004, and the third in June, 2012, and so on, as long as there will be on the earth rational beings intelligent enough to take an interest in the transit of Venus. He congratulates himself on having established a "periodical" which will be perhaps the most durable foundation of his age.

A SWISS Geological Society has lately been formed. It is an offshoot from the Helvetic Society of Natural Sciences. While a permanent section of this, it will have its own life, its committee, its funds, its distinct *séances*, and its publications if thought desirable. It will have members who do not belong to the mother society; will send a delegate to the preparatory assembly of the latter, and will have the right of presentation of members. The number of adherents of the new society is already over sixty. It has absorbed the *Congress der Feld Geologen* and the *Comité d' Unification géologique*. Among other things it will encourage excursions along with discussion on the ground, and will represent Switzerland in the International Geological Congresses. The Society has testified its respect for MM. Studen, Heer, and Merian, by (exceptionally) giving them the title of Honorary Members.

THE Council of the British Association, acting under the powers conferred upon them by the General Committee, in accordance with their Report, have appointed the following to be a Committee, "to draw up suggestions upon methods of more systematic observations, and plans of operation for local societies, together with a more uniform mode of publication of the results of their work," and to "draw up a list of local societies which publish their proceedings." Mr. H. G. Fordham (Secretary), Rev. Dr. Crosskey, Mr. C. E. De Rance, Sir Walter Elliot, Mr. Francis Galton, Mr. John Hopkinson, Mr. R. Meldola, Mr. A. Ramsay, Prof. W. J. Sollas, Mr. G. J. Symons, Mr. W. Whitaker.

COLONEL PREJEVALSKY, the distinguished traveller, intends to resume his explorations in Central Asia in the spring, and to make another attempt to penetrate to the capital of Thibet. He is now suffering slightly from weakness of sight.

PROFESSORS have been appointed to give courses of lectures at the Louvre upon its collections, and the school opens this week. Gaulish antiquities will be expounded by M. Bertrand, curator of St. Germain Museum; Egyptian remains by MM. Pierret and Revillout; Semitic epigraphy and archæology by M. Ledrain; and ancient art by M. Ravaisson.

A "PROJET de Mer Intérieure dans le sud de l'Algérie et de la Tunisse" (occupying the space usually known as "The Schots" or "Les Chotts," which is lower by several feet than the Mediterranean Sea), suggested by M. le Commandant Roudaire, was communicated some time since to the French Government, and was in May last laid by M. de Freycinet before a "Commission Supérieure." This Commission has examined the question under every point of view, antiquarian, political, practical, and commercial, and their labours are recorded in a quarto volume of 546 pages, illustrated by a map. On July 7, 1882, the Commission made the following Report:—

> "La Commission,
> Considérant que les dépenses de l'établissement de la mer intérieure seraient hors de proportion avec les résultats qu'on peut en espérer,
> Est d'avis qu'il n'y a pas lieu pour le Gouvernement Français d'encourager cette enterprise."

IN the course of the coming winter Prof. Emil Selenka hopes to publish a Monograph of the Sipunculacea, in which he will be assisted by Doctors J. G. de Man and C. Bulow. The volume will contain the descriptions of 81 distinct species placed in 10 genera. Some of the species are new. The Monograph will form vol. iv. of Semper's "Reisen im Archipel der Philippinen," and will contain the forms collected by Semper; but in order to make it a more or less complete revision of the group, Dr. Selenka also describes in it the species collected at the Mauritius by Dr. Mobius, those in the Berlin Museum through the goodness of Prof. Peters (this collection contains the types of Grube), those from Stuttgart containing Dr. Klunzinger's Red Sea collection (through Dr. Krauss), those from the British Museum (through Dr. Gunther), and those from Gottingen, the types of Keferstein (through Dr. Ehlers). In addition. Dr. Selenka has been indebted for specimens to the liberality of Dr. von Martens, Dr. Hilgendorff, Dr. Krapelin, and Dr. Lang. Dr. Grœffe was able to forward living examples of *Aspidosiphon mülleri*. Besides a general introduction and description of the genera and species, there will be dissertations on the tentacular and blood systems, while special care has been taken about the subjects of the geographical distribution, anatomical relations, and synonymy of the species. The volume will be accompanied by 15 plates with more than 200 partly coloured drawings.

SINCE the commencement of the present Session the Society of Arts meeting room has been lighted by means of electricity. A Siemens dynamo is employed driven by an 8 horse-power Crossley gas engine. Nearly the whole cost of these was defrayed by subscriptions from a few past and present members of the Society's Council. The lamps used are those of Edison, and there are at present fifty of them in the room. The chandeliers now in use have been lent by Messrs. Verity, who are constructing chandeliers to be permanently fitted, now that the number of lights to be used has been decided upon. Temporary fittings have been put up in the council room, and the result having been proved satisfactory, it is in contemplation to arrange for the lighting by electricity of this and other parts of the building.

AN unusually large number of seals have made their appearance in the Baltic, a few miles north of the Samland coast. Should these animals make that spot their permanent residence, the salmon fisheries would be in a sad plight. On the Pomeranian coast the damage to salmon fisheries done by seals is very considerable.

NO less than thirty-four communes in the district of Chambery (Savoie) are now infected by Phylloxera.

A COMMISSION has been appointed by the Prefect of the Seine to reconsider the disposal of the Paris sewage. A deputation will be sent, at the expense of the Municipal Council, to Brussels, Antwerp, Amsterdam, Berlin, and London to report on the matter.

A BRUSSELS paper, *L'Athenæum Belge*, reports some interesting observations made by M. W. Spring regarding the seat and origin of thunderstorms. During the summer 1881 M. Spring ascended the Schnæhorn in the Bernese Oberland during a thunderstorm. He then noticed that for a considerable time no rain fell, but that a vivid formation of hail took place. From time to time the hail fell very much thicker, and in such moments came a bright flash of lightning followed by a tremendous clap of thunder. After a pause rain-drops mixed with the hail. The same observations were made on the summit of S. Giacomo, where he again observed a thunderstorm. He concludes from his observations that the actual seat of thunderstorms, *i.e.* of the aërial electricity is not in moist regions of the atmosphere but in the dry and cold region of hail.

PROF. HEULE, the eminent anatomist, has been elected, in the place of the late Prof. Wohler, as permanent secretary of the Royal Academy of Sciences at Göttingen.

NEWS from Champagne states that a new enemy to the vine has made his appearance in the shape of a minute fungus, a kind of *Peronosporæ*, the dangers of which are said to be far more serious even than those of Phylloxera.

DR. C. W. SIEMENS, F.R.S., has consented to distribute the prizes and certificates gained by the successful candidates of the metropolitan centres at the recent technological examinations, as well as by the students of the City and Guilds of London Technical College, Finsbury, and of the City and Guilds of London Technical Art School, Kennington. The distribution will take place on Thursday evening, December 14, at 7 o'clock, at Goldsmiths' Hall, Foster Lane, E.C.

TELEGRAMS from General Nansouty to Admiral Mouchez announce that an avalanche of fresh fallen snow had swept away five labourers who were trying to carry victuals to the Pic du Midi for MM. Henry, who are at that place to observe the transit of Venus. Two of these poor people lost their lives.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. W. Nash; a Capybara (*Hydrochærus capybara*) from Venezuela, presented by Mrs. R. H. Fitz-Simons; a European Scops Owl (*Scops giu*), European, deposited.

THE ROYAL SOCIETY[1]

OUR anniversary is in one sense the opening of a new year, in another it is the close of an old one. With one hand we weclome the coming, with the other we bid farewell to the departing guest. In the later parts of my present address I shall have to speak, as on former occassions, of our prospects and hopes for the future. At our more festive gathering in the evening we shall recount some of the victories which have been won over difficulties in the extension of knowledge, and shall rejoice at the gathering of old comrades and friends after our usual period of dispersion. But at the moment of taking my place in the chair to which you have now for the fourth time elected me, I must confess that the sadder side of the picture is the most prominent. We seem almost for the moment to enter the Valley of the Shadow of Death, or, like Dante, to descend to the place of Departed Spirits, and to commune with them once more after they have vanished from the upper world. Each year during my own term of office the numbers lost to us have been greater than the numbers gained; but this year, although the list of deaths is long and comprises not a few distinguished Fellows, they all seem overshadowed by two prominent figures. One of these died in the fulness of years, of honours, and of world-wide reputation; the other in the strength and bouyancy of youth, a buoyancy which appears to have even contributed to his end.

Of Darwin and his works it is not for me to speak. Others, with wider knowledge, after longer intercourse, and with greater authority, have said what was possible at the moment, and the full story of his life is now being written by faithful hands. But I consider it no common piece of fortune to have lived within easy distance of his house: to have been able by a short pilgrimage to enjoy his bright welcome, and his genial conversation, and to revive from time to time a mental picture of that my ideal of the philosophic life.

Of Balfour I knew far less, and his works are beyond my range of knowledge. But such was the fascination of his speech and his demeanour that to have seen him was to desire to know him better. To have been selected at his age as one of the Secretaries of the British Association, a post usually reserved for men of more advanced years and of longer experience, to have been appointed to a professorship founded almost on the basis of his own work, and thereby to have become the coadjutor of his own great master in the Physiological School at Cambridge and all this without one word of cavil or of criticism, was a high testimony to his scientific eminence. But far wider afield, it will be remembered of him, not so much that he was brilliant in intellect, or keen of insight, or varied in his attainments, but that he always found himself among friends, whether in college or in the laboratory, in his own home over the northern border, or on the wild mountain side where he breathed his last.

The list of deceased Fellows comprises other eminent names, many of whom will receive mention in our obituary notices. The list, moreover, serves again to exemplify the variety of qualifications which have opened our doors to election. In Decimus Burton we find an architect of refined taste and cultivated mind; in Stanley Jevons and William Newmarch statisticians of weight, and the former already an authority on political and other philosophy; in Sir Woodbine Parish a geographer, and more than a geographer, a man who by service as well as by study in foreign lands had acquired an unusual amount of first hand and accurate information; in Scott Russell an engineer whose brilliant early strokes of work will be remembered when the difficulties which entangled his later efforts have been long forgotten; in Dr. Robinson a veteran and mentor in science, whose work and whose judgment were alike sound. Of Sir Wyville Thomson mention will be made elsewhere.

To this list of names there was well nigh added yet another, namely, my own. An accident, under circumstances which the issue of events and more mature reflection have shown that I was hardly justified in incurring, has for some time past interfered materially with my usual avocations in life, and thereby, as I fear, with my usefulness to the Society. But the ready and efficient assistance of the other officers has, I doubt not, gone far to supply the deficiency. For myself, I am consoled by the kind expression of sympathy from many, some even unknown, friends; and by the consideration, ever present to my mind, that, except through a combination of circumstances over which I had certainly no conscious control, the result to myself might have been far more serious.

The total number of Fellows lost to our ranks during the past year is twenty-two on the home list (one of whom has withdrawn on account of growing infirmities), and four on the foreign list; a result, on the whole, not very different from that of last year.

Of these two fell young, and by accident. Of the remainder, two died between the ages of 50 and 60, four between those of 60 and 70, six between those of 70 and 80; and the remaining five attained ages between four score and 90.

In Liouville we have again lost a veteran mathematician; in Wöhler, a chemist whose years, numbered from the beginning of the present century, reached to a period almost prehistoric in the records of his science.

I am happy to report that the sale of the Acton estate has been completed; and that of the proceeds, amounting to 32,250*l.*, 17,000*l.* has been invested in preference or guaranteed railway stock; and the remainder will be expended in the purchase of ground rents, partly in the City of London, and partly in the western suburbs. The income from the latter source, already representing a very fair interest on the outlay, may be expected materially to increase at the expiration of the existing building leases. Some additional expense was incurred this year in painting a portion of the Society's apartments. A considerable portion still remains to be painted, either next year, or at some not very distant period.

While on the subject of property, I should mention that Her Majesty has sanctioned "the continuance of the occupation of the Royal Observatory at Kew by the Royal Society," upon certain conditions, which have been accepted. The building will be devoted, as heretofore, to the use of the Kew Committee, whose work, it must be remembered, is provided for in the main by the Gassiot Fund.

Last year the Society accepted a portrait of Sir J. D. Hooker, painted by Mr. John Collier, at the request and at the expense of a considerable number of Fellows. I trust that the Society will approve the action of myself and a few others, in this year offering for our collection a portrait, by the same artist, of Mr. Joule.

Mr. A. Le Gros has presented to the Society a bronze medallion head, executed by himself, of the late Mr. Darwin.

The Library has received many valuable contributions both from our Fellows and from others. Among the latter I may mention the completion of "The Lepidoptera of Ceylon," from the Government of Ceylon; G. Retzius' "Gehororgan der Wirbelthiere," from the author; a new edition of Abel's works, from the Norwegian Government; and facsimile lithographs of some of the late Prof. Clifford's mathematical fragments, and the catalogue in two handsome volnmes from the Public Library of Victoria.

The printing of the general part of our library catalogue is in progress; and although, owing to unforeseen difficulties the hope expressed last year, that it would have been now finished, has not been fulfilled, yet there seems little doubt that early next year it may be in the hands of the Fellows.

On the completion of this work the Library Committee contemplate resuming another decade, 1874-83, of the great Catalogue of Scientific papers; and the President and Council trust that the success which has attended the publication of the eight volumes already in existence will justify the Treasury in undertaking the printing of the second supplement when the MS. has been prepared.

In the staff of the Society I have happily no change to report. Of the existing members my own feelings would impel me to say much more; but, while they would probably wish me to be silent, I trust they will pardon me in this one remark: that while recent changes make me less apprehensive of any future alterations, they at the same time make me hope that any alteration may be long postponed.

Although the number of papers presented to the Society during the past year, apart from their contents, does not convey any very important information, yet in continuation of past practice I may perhaps carry on the ten years' table. It is as follows, showing a slight diminution in the past year:—

1873	92	papers received.
1874	98	„ „
1875	88	„ „
1876	113	„ „
1877	97	„ „
1878	110	„ „
1879	118	„ „

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[1] Address of the President, William Spottiswoode, D.C.L., LL.D., delivered at the Anniversary Meeting, November 30, 1882.

1880 ....123 papers received.
1881........................127 „ „
1882........................109 „ „

Among the papers of this year, I may notice the elaborate research by Dr. Debus on "The Chemical Theory of Gunpowder," forming the Bakerian lecture; the careful and long-continued investigations by Professors Liveing and Dewar on the spectra of water, and of carbon, and of mixed vapours.

Nor must I omit mention of Dr. C. W. Siemens' bold and original theory of the conservation of the solar energy, which has already given rise to so much discussion. It will be sufficient for me here to say that upon the questions therein raised the last word has been by no means said; and that, whether the theory be ultimately established, or whether, like a phœnix, it shall hereafter give rise to some other outcome from its own ashes, it will ever be remembered as having set many active minds at work, and will always have a place in the history of Solar Physics.

In Mathematics, definite integrals, and elliptic and the higher transcendents continue to occupy much attention, and in particular our "Transactions" contain an excellent contribution to the theta-functions of two variables, by Mr. Forsyth, of Liverpool. To the theory of invariants, Prof. Malet, of Cork, has given a happy extension in the direction of linear differential equations; but it is unnecessary to speak in detail of papers which either already are, or will shortly be, in the hands of the Fellows. I will only add that the "Philosophical Transactions" for 1882 will probably exceed in bulk, and not yield in interest to, those of any former year.

Looking outside the circle of our own publications, there has been one step gained during the past year, which, although in some sense a matter of detail, is really of great importance and interest. I allude to the paper by Lindemann, "Ueber die Zahl π" ("Mathematische Annalen," Band xx, p. 213). It had long since been shown that both the numbers π and π^2 are irrational; but hitherto no proof existed of the impossibility of effecting the quadrature of the circle by means of the straight line and circle, and ruler and compasses. Regarded from an algebraical point of view, every such construction must depend upon the solution of a quadratic equation, or rather of a series of quadratics whereof the first has for its coefficients rational numbers, and the succeeding members of the series only such irrational numbers as occur in the solution of their predecessors. This being so, the final equation can always be transformed, by transposition of terms and squaring, into an equation of an even degree with rational coefficients. And, consequently, if it can be proved that π cannot be the root of any algebraic equation whatever with rational coefficients, the impossibility of the quadrature of the circle will be thereby also proved. Starting from Hermite's researches ("Comptes Rendus," 1873), in which he established the transcendental nature of the number *e*, Lindemann has supplied the proof required with reference to the number π. It must be admitted that the proof is neither very simple nor very easy to follow; and it remains only to be hoped that it may some day assume such a form as may influence the minds which still exercise themselves upon the hopeless problem of squaring the circle.

A most important change in the relations between the Society and the Government in respect of State aid to science has been made this year. It will be in the recollection of the Fellows that an experiment was made for a period of five years, during which the sum of 4,000*l*. was annually voted to the Science and Art Department, to be distributed at the recommendation of the Government Fund Commitee of the Royal Society. That experimental period terminated, as then mentioned in my address, last year. The grant to the Science and Art Department has been discontinued, and in the place of it an addition of 3,000*l*. per annum has been made to the Government grant, making 4,000*l*. in all. In concluding this arrangement the following stipulations were agreed to. The increased grant is to be administered by a Committee identical with the late Government Fund Committee; a portion may be devoted to personal grants, subject, however, to special recommendations to the Treasury; and, lastly, unexpended balances may be carried forward from year to year, as has hitherto been the case with the old government grant only. To the stipulation that the increased fund should be administered by the more extended committee the Society felt that no reasonable objection could be offered, because upon it the President and Council are represented in full, and the *ex officio* members are in the majority of cases Fellows of the Society. The object of the second stipulation was, so far as the Society is concerned, to secure at the outset for the personal grants the consent and support of the Treasury, and thereby to preclude the chance of objection being subsequently taken to any of our proposals under this head. The President and Council, however, recognising the importance of great caution in respect of personal grants, have of their own motion appointed a special sub-committee (in addition to the three previously existing), to which all personal applications recommended by any of the other sub-committees are specially referred, and without whose recommendation none can come before the General Committee. To the third mentioned point, viz., the power of retaining unexpended balances, the President and Council attach great value, because that power may enable the Committee to devote more of its funds than heretofore to some of the larger undertakings in scientific inquiry, leaving more of the smaller grants to the special funds already in existence in the hands of the Royal and other societies. The meetings of this Committee will probably take place twice a year, in May and November. In the present year it will not be possible to hold the second meeting before December, but there will be advantages in holding it hereafter in November, as the entire annual grants will then be made by the same Committee, and under the sanction of the same President and Council. In concluding these few remarks on the new arrangements, I cannot refrain from expressing my sense of the obligation under which the Society and Science at large are laid by the sympathetic and intelligent attention bestowed upon the subject by the then Financial Secretary of the Treasury, the late Lord Frederick Cavendish.

Among other subjects referred to the Royal Society by Public Departments I may mention a request from the Board of Trade for advice upon the question of improving the existing means at the Standard Office for the purpose of comparisons. At the request of the President and Council, Sir George Airy, Colonel A. Ross Clarke, and Prof. Stokes acted as a Committee, and drew up a very careful report, the value of which was fully recognised by the Board of Trade. The report suggested certain improvements in the present arrangements; but, having reference to the duties of the Standard Office as defined by Act of Parliament, it was not considered necessary to insist upon extreme scientific accuracy, such, *e.g.*, as that attained by Colonel Clarke himself in his "Comparison of Standards" made at the Ordnance Survey Office at Southampton in 1866.

The arrangements for the observation of the Transit of Venus have been steadily progressing. The parties have now all started for their stations, after their period of training under the superintendence of Mr. Stone at Oxford. An adequate supply of instruments has been secured at moderate cost, and all the accessory parts have been procured and applied by the indefatigable care and forethought of our directing Astronomer.

The English Expeditions for the observation of the approaching Transit of Venus are organized as follows:—

Accelerated Ingress.

Madagascar Observers.—Rev. S. J. Perry. Rev. W. Sidgreaves Mr. Carlisle.

Cape Observatory Observers.—Mr. Gill and Staff.

Aberdeen Road Observers.—Mr. Finlay, First Assistant of the Cape Observatory. Mr. Pett, Third Assistant of the Cape Observatory.

Montagu Road Observers.—Mr. A. Marth. Mr. C. M. Stevens.

Retarded Ingress.

Bermuda Observers.—Mr. J. Plummer. Lieut. Neate, R.N. Capt. Washington, R.E.

Jamaica Observers.—Dr. Copeland. Capt. Mackinlay, R.A. Mr. Maxwell Hall.

Barbadoes Observers.—Mr. C. G. Talmage. Lieut. Thomson, R.A.

Besides the observers at these stations, the Canadian Government has arranged to place three 6-inch and some smaller telescopes in the field. Lieut. Gordon of Toronto was sent by the Canadian Government to England to make himself master of the proposed arrangements, and to secure the necessary instrumental equipment.

Accelerated Egress.

The stations for Retarded Ingress are also available for Accelerated Egress.

RETARDED EGRESS.

Brisbane Observers.—Capt. W. G. Morris, R.E. Lieut. H. Darwin, R.E. Mr. Peek.

New Zealand Observers.—Lieut.-Col. Tupman, R.M.A. Lieut. Coke, R.N.

Besides these observers sent specially from England, the Observatories at Melbourne and Sydney are most favourably situated for observing the Egress. The Directors of these Observatories, Mr. Ellery and Mr. Russell, have promised their co-operation, and their Governments have placed funds at their disposal to cover any necessary expenses.

Unless unfavourable weather should prevent the transit being seen at some of the stations, we may expect some nine or ten pairs of corresponding observations, both at Ingress and Egress, from the British expeditions alone. These observations are certain to be largely supplemented by those made by the observers of other nations; and it is hoped, from close agreement between the instructions issued to the different observers, that the whole may ultimately be available for combination in one general discussion.

The American astronomers, encouraged by the partial success which attended the plan they adopted in 1874, are relying chiefly upon the photographic method; they have sent expeditions to South America and the Cape of Good Hope.

Austria does not take any active part in observing the Transit.

France sends out eight well equipped expeditions, full particulars of which have been published in the "Comptes Rendus" for October 2.

From Holland no special expedition will be sent out, but Lieutenent Heyming, of the Dutch Navy, will observe the transit in the West Indies, probably at Curaçoa.

Italy will confine its operations to observatories in that country.

Russia, also, has decided to send out no expeditions of its own, but it has aided the efforts of other countries by lending a 6·5-inch reflector to the Danish Government, and has placed two excellent 4·3-inch heliometers in the hands of the French astronomers, MM. Tisserand and Perrotin. The considerations which led the Russian Government to this conclusion have been explained in the following paragraphs of a letter from Mr. Struve to myself:

"Experience since 1874 has sufficiently proved that there is no prospect whatever, even with combined international efforts, of obtaining by the present transit a geometrical determination of the parallax of the sun, which would not soon be surpassed in accuracy by other recent methods (for example, that suggested by Mr. Gill), methods which are capable of being repeatedly employed, and that without any costly expeditions.

"Further, although it must be admitted that so rare an opportunity of studying the atmosphere of the planet ought not to be neglected, yet it seems certain that so many and such excellent data will be obtained through the agency of the United States, as well as by other countries having well provided observatories in the southern hemisphere, as well as by other seafaring nations." Under these circumstances Russia has not considered it incumbent on itself to organise any observing parties.

Spain has sent two parties of naval officers, well equipped with 6-inch equatoreals and other instruments, to the Havana and Porto Rico.

Last year I expressed a hope that the difference of longitude between Singapore and Port Darwin in Australia would be determined by Commander Green of the United States' Navy in concert with Mr. Todd. This operation, however, in consequence of some incorrect information furnished to Commander Green as to the intentions of our home authorities in the matter, was not carried out. After various proposals, extending over a period of not less than two years, I am happy to say that it now appears likely that the work will be performed. Through the liberality of the Secretary of State for War an extension of leave has been granted to Lieutenant Darwin, who accompanies Captain Morris to Brisbane to observe the transit of Venus, enabling him to undertake the work. He has received instructions to arrange with Mr. Todd all details of the operation. The publication of the results obtained by Oudemans and Pogson for the difference of longitude between Madras and Singapore has now left only one link wanting, namely, that between Batavia and Port Darwin, to connect Australia with English longitudes. Lieutenant Darwin is eminently qualified for the work; and it seems a happy coincidence that it should fall to his lot to connect astronomically the distant point named after his father with the furtherest ascertained point in that direction. I should not omit to add that Mr. Todd has placed all the telegraphic appliances under his command at the disposal of this service, and it is to be hoped that the determination will prove as useful to the Australian colonies as it will be valuable for the purposes of the transit. The best thanks of the Committee have already been given, but I am glad here publicly to recognise the valuable assistance rendered to the Committee in these long negotiations by the Great Eastern Telegraph Company.

In the course of last year the Treasury made known to the Society that in consequence of Sir Wyville Thomson's ill-health, their Lordships proposed that his chief assistant, Mr. Murray, should undertake the general editorship of the Reports of the *Challenger* Expedition; so that Sir Wyville might devote himself more exclusively to the personal narrative. At the request of their Lordships a small Committee, with whom Mr. Murray might consult from time to time, was appointed, consisting of the President and Officers, Sir Joseph Hooker and Prof. Huxley; but before the Committee could meet the lamentable death of Sir Wyville Thomson occurred. They met, however, shortly afterwards, and having added Prof. Mosely to their number, they received from Mr. Murray, who attended, a detailed statement of the existing condition of the whole arrangements conected with the Report. From this statement it appeared that, in addition to the original estimate of 20,000*l.* given by Sir Wyville Thomson, the work actually in progress and entrusted to the several authors required a further sum of about 20,000*l.*, and that if the series should be completed, by describing on the same scale groups as yet unallotted, an additional expense of about 6,000*l.* would be entailed. In forwarding this statement to the Treasury, the Committee stated that, in their opinion, Mr. Murray's estimates were drawn up with great care and judgment, and that in view of the remaining Reports being carried out on the same scale as those already published, they were reasonable and sound. As to the cause of the great discrepancy the Committee felt themselves unable to offer any explanation; the conduct of the whole business having been left in Sir Wyville's hands, without reference to the Society. They further were of opinion that Mr. Murray might safely be entrusted, under the control and supervision of the Committee, with the entire future management of the undertaking.

After some further correspondence it was suggested that Mr. Murray should furnish the Committee with a statement of the existing condition of the Reports and their management, which should form a starting point for the responsibility of the Committee; and that he should keep the Committee well informed from time to time of the progress of the undertaking. These suggestions were cordially accepted bp their Lordships, and with the general statement which Mr. Murray submitted in October, the special duties and responsibilities of the Committee have begun.

Since last year, three more volumes of the Report have been published, making six in all. The new volumes form volumes iv. and v. of the Zoology, and volume ii. of the Narrative. The latter volume comprises the magnetic results, the meteorological observations, the report on the pressure errors of the thermometers, and the petrology of St. Paul's rocks. Vol. i. of this section, containing the narrative proper, is partly in type; and will, it is hoped, be issued during the summer of 1883. Other volumes will also appear from time to time.

In connection with this subject, I may mention that the collection of specimens from the *Challenger* Expedition are being received at the British Museum, as the particular portions are released by the progress of the publication of the Report. Those derived from the *Alert* Expedition to the South Pacific Ocean, have been deposited in the Museum by the Admiralty, and are now being arranged and described. Dr. Gunther hopes to be able to produce a printed descriptive catalogue of the collection before the expiration of the present year. And I desire here to acknowledge the service rendered to science by the Admiralty in commissioning Dr. Coppinger to accompany that expedition for scientific purposes.

I am indebted to Mr. Murray for the following interesting account of a cruise made last summer to complete some part of the *Challenger* work.

H.M.S. *Triton* was engaged, from the 4th of August to the 4th of September, in a re-examination of the physical and biological conditions of the Faroe Channel.

The chief objects of the cruise were to ascertain by actual soundings, the character of a ridge running from the north of Scotland to the Faroe fishing banks, and separating, at depths exceeding 300 fathoms, the cold Arctic water with a temperature about 32° from the so-called Gulf Stream water on the Atlantic side with a temperature of 47° F. This ridge was traced in considerable detail by means of cross soundings directly across the channel, and the top was found to be on an average about 260 fathoms, beneath the surface. In the northern half of the ridge, however, a small saddle-back was found with a depth of a little over 300 fathoms, through which some of the Arctic water seemed to flow and to spread itself over the bottom on the Atlantic side of the ridge. The top of the ridge is entirely composed of gravel and stones, but mud and clay are found on either side at depths exceeding 300 fathoms. Many of the stones are rounded, and some of them have distinct glacial markings. They are fragments of sandstone, diorite, mica-schist, gneiss, amphibolite, chloritic rock, micaceous sandstone, limestone, and other minerals. The ocean currents here appear to be strong enough, at a depth of between 250 and 300 fathoms, to prevent any fine deposit, such as mud or clay, being formed on the top of the ridge. All the indications obtained of the nature of this ridge, seem to imply that it may be a huge (terminal?) moraine.

It is worthy of notice that the "Wyville Thomson Ridge' is only a little to the east of the position marked out by Croll from the observations of Geikie, Peach, and others, as the probable limit of the perpendicular ice cliff formed in North Western Europe during the period of maximum glaciation.

The dredging captures show the same marked difference as had previously been pointed out in the fauna of the two areas; those in the cold area being of a distinctly Arctic character, and those in the warm area resembling the universally distributed deep-sea fauna of the great oceans. A fair proportion of new species were also found.

The last trip of the *Triton* took place from Oban, on the 11th September, to the deep water in the Atlantic westward of Ireland. The object of this trip was to get *directly* a determination of the pressure unit of the guages employed in testing the *Challenger* thermometers. The original determinations were made *indirectly* by the help of Amagat's results as to compression of air. The observations taken are not yet reduced, but several successful trials were made at depths of 500, 800, and 1,400 fathoms.

(*To be continued.*)

M. MIKLUKHO-MACLAY ON NEW GUINEA

ON October 11 M. Miklukho-Maclay gave, at the Russian Geographical Society, the first of a series of lectures on his sojourn in New Guinea. These lectures have attracted great audiences. His remarkable collections of household articles and implements of Papuans and of various tribes of the Malacca Peninsula, and the many drawings reproducing scenes of the life, dwellings, graves, anthropological types, &c., of the natives, are exhibited in the rooms of the Geographical Society, and attract many visitors.

M. Miklukho-Maclay left St. Petersburg in 1872, and went on board a Russian ship to New Guinea. He expressed the wish to be left there for at least a year, and it was fifteen months after his being landed that he was taken up by a ship which brought him to Batavia. His stay in New Guinea was beset with difficulties. He lived in a small hut, was short of provisions, which he had to supply by hunting, and his health was quite broken down. But he entered into very close relations with the natives. In Batavia he stayed for several years, and published (in German) the results of his anthropological and ethnological observations among the Papuans, on the Brachycephaly of the same, and on the climate of the "Maclay-coast" in the Batavian scientific journal, *Natuurkundig Tijdschrift voor Nederlandsch Indie*. A paper (in French) on the Vestiges of Art among the Papuans appeared in the *Bulletin de la Société d'Anthropologie de Paris* for 1878. In 1876 he undertook a new journey on board the English schooner *Sea Bird*, and visited the Yap, Pelau, Admiralty, and Ninigo Islands, and went again to the coast of New Guinea, to which his name is now attached. An account of this journey has appeared in the *Izvestia* of the Russian Geographical Society and in *Petermann's Mittheilungen* for 1879. During this second sojourn in New Guinea M. Miklukho-Maclay was lodged more comfortably, and was enabled to pursue scientific investigations (anthropological measurements and anatomical researches) with less difficulty. He also explored in a canoe, with natives, the coast of New Guinea between Cape Croaz and Cape Teliata. Having undertaken his adventurous journey on his own account with but a little occasional support from the Geographical Society, M. Miklukho-Maclay was often in difficult circumstances, but a few years ago a public subscription was opened by the Russian papers, and the Russian Society immediately came to his aid, thus enabling him to continue his researches.

When in search of a place at which to study the customs and life of the primitive people at the lowest stage of culture, M. Maclay chose the north-western coast of New Guinea, close by Astrolabe Bay, which was never visited before by Europeans. Neither Dampier nor Dumont D'Urville, who both passed close by, had landed there. He built his hut between two Papuan villages, on a promontory that was occupied by nobody. At the beginning the Papuans wished him to go back whence he came, and obstinately showed him the sea; sometimes they launched their arrows close by him, but without wounding. By great endurance however, by his good nature, and especially by a continuous self-control and severe watching over his own actions, M. Maclay soon won the confidence of the natives. He always strictly kept his word, even in the most insignificant circumstances, and therefore had afterwards the satisfaction of hearing the natives saying "*Balan Maclay hoodi*" ("The word of Maclay is one"). The natives used to call him *Kaaram-tamo*, "The Moonman," partly on account of the supernatural capacities they ascribed to him, and partly on account of his having once, when searching for something about his hut in the night, lighted a white signal-fire that was left from the ship which brought him. The first visits of M. Maclay to the Papuan villages were a source of great trouble among the natives; the women were concealed and the men seized their arms. M. Maclay used then to announce beforehand his arrival by loud whistling, and the natives concluded he did not wish to do them harm. By and by he won the confidence of the natives to such an extent that an attack of a hostile tribe having been expected, his neighbours brought their women and children to his hut, to be under his protection. The war was thus prevented, and the authority of the "Moon-man" was sufficient to prevent further wars.

The natives of this coast are at the lowest stage of culture. Before M. Maclay's arrival they did not know the use of metals, all their implements being made of stone, bones, and wood. They did not even know how to make fire. If the fire were extinguished in a hut, it was taken from another; it would be taken from a neighbouring village if extinguished in all the huts of the village at once. Their grandfathers told them of a time when they had no fire; then they ate their food quite raw, and a disease of the gums spread among them. They do not bury their dead. The dead are put in a sitting position, the corpse is covered with leaves of the cocoa-palm, and the wife must keep a fire close by him for two or three weeks, until the corpse is dried. Corpses are buried only if there is nobody to keep the fire.

M. Maclay left the Papuans with regret, when a passing schooner took him, in 1878, to Singapore. He expects for his friends the fate of the inhabitants of the Melanesian Archipelago, where the population rapidly diminishes on account of the "kidnapping" of men and women to sell them into slavery, which is practised to a great extent by crews of ships of all nationalities of the civilised world.

In his second lecture, M. Miklukho Maclay gave further information with regard to the Papuans of New Guinea. Previous anthropologists had admitted the existence of at least two different races in New Guinea, and had made a distinction between the Papuans inhabiting the coast and those of the interior. After several visits to New Guinea, as well to the coast, as to the interior, M. Maclay came to the conclusion that this supposition is not correct. The Papuans of the interior belong to the same race as those of the coast, and there is throughout New Guinea but one single Papuan race. Virchow found it also necessary, on the ground of craniological measurements, to distinguish the Papuans from the Negritos of the Philippine Islands, and to admit that the former are dolichocephalic, and the second brachycephalic. Hundreds of measurements made by M. Maclay brought him to the conclusion that both types have their representatives even among the purest Papuans of the Maclay coast, and that the transversal diameter of the skulls of Papuans varies everywhere within so wide limits (62 to 86 per cent. of the length of the skull), that no classification can rest on this feature. It was stated also that a special feature of the Papuans which distinguishes them from other curly-haired races, is that their hairs grow in clusters, separated from one another by sinuous spaces devoid of hair. Extensive researches proved, however, that this cluster-like disposition of hairs does not exist among Papuans, not even among children. Finally, several anthropologists considered the diameter of the curls of the hairs as a feature that may help to establish a distinction between the Papuans and the Negritos; these last have been supposed to have smaller curls than the former, that is, no more than one or two millimetres wide. M. Maclay found, however, that the diameter of the curls of the Papuan also does not exceed one and a half millimetre, and that it varies very much in different parts of the head, so that this feature cannot be taken as a basis for anthropological classification.

After having taken some rest at Buitenzorg, M. Maclay left Batavia in January, 1873, for a third visit to New Guinea. The Malayans of Celebes have carried on an intercourse with New Guinea for more than three or four hundred years; they go there, as well as the inhabitants of the islands Lant, Seram, and Key, for the purchase of slaves, turtles, trepang, and pearl shells. To establish closer relations with the natives, the Malayans of Celebes bring with them Malayan girls, give them as wives to the Papuans, and export in exchange Papuan girls who are married in Celebes. (These relations were described by P. A. Leupe in the "Bijdragen tot de Taal-Land en Volkenkunde van Nederlandsch-Indie" for 1865.) Therefore it is impossible to find pure Papuans on the Papua-Onim and Papua-Notan coasts, and M. Maclay took the resolution to go to the Papua-Koviay coast. The inhabitants of this coast have a very bad reputation as robbers and anthropophagi; but still, M. Maclay hired a Malayan "praw," or "urumbay," that is, a boat thirty feet long, and, with a crew of two Christians from Amboyna, and fourteen Malayans and Papuans, he left the islands Seram-Lamut, and reached the Koviay coast. Triton Bay (where the Dutch had formerly a military settlement) proved to be a beautiful strait, to which M. Maclay gave the name of the Russian Grand Duchess Helena Pavlovna. He discovered also another bay that separates the island Namatote from the mainland of New Guinea. He stopped at Aiva, between these two straits, and his men immediately erected a hut from the "ataps" (a kind of mat made from leaves of the tapioca palm) that were brought in the boat. The inhabitants of this coast proved to belong to the same race as those of the Maclay coast; however, it was easy to perceive, especially among children, unmistakable traces of mixture of Malayan blood The size of the men on the Maclay coast varies from 1·74 metres to 1·42; the size of full-grown women was 1·32. On the Papua-Koviay coast the size of the men was from 1·75 to 1·48 metres, and the size of the women 1·31. On the Maclay coast the length of the transversal diameter of the skull was from 64·0 to 86·4 per cent. of the longitudinal diameter, and from 62 to 80 per cent. on the Koviay coast.

Leaving ten men at Aiva, M. Maclay went with the remainder of his crew to explore the interior of the mainland. He landed opposite Coira Island, and, crossing a range of mountains 1200 feet high, reached Lake Kamaka-Vallar. He found there a tribe which calls itself Vaasirau, but does not differ from the inhabitants of the coast. The water of the lake was very warm (31° Celsius), and contained an interesting new kind of sponge, belonging to the *Hallichondriæ*. The rains in this part of New Guinea are so copious that Triton Bay is sometimes covered with a sheet of sweet water that can be taken in vessels and used for drinking. As the lake has no outlet, its water rises many years, sometimes fifteen and twenty feet, and covers the trees that grow on its shores; but after a period of rising, the rocks at its bottom give way, and the water is discharged through a temporary outlet, which is soon checked by stones and mud. Returning to the shore, M. Maclay made excursions to the neighbouring islands (discovering coal on Lakahia Island), as well as several other excursions to the highlands of New Guinea. In Telok Bay the boat of M. Maclay was attacked by a number of pirogues of Papuans, but made his escape by rowing all night. But his men at Aiva were not so fortunate. They were attacked by 200 Papuans, who destroyed the hut and killed an old man who was interpreter, as well as his wife and child. A further stay at Aiva was impossible, as the Papuans had poisoned the springs; and so the party went to stay on Aidum Island, where M. Maclay's hunter brought him every day plenty of interesting birds and other animals. The New Guinea kangaroo, *Dendrologus ursinus*, is worthy of mention, as it has to adapt itself to local conditions, strong nails, and lost at the same time the strength of the muscles of the tail; it has become thus a climbing animal and lives mostly in trees. After having taken prisoner the chief of the Papuans who had robbed his hut, (M Maclay went one day with a few men to their camp, and simply ordered them to tie the chief; the Papuans, terrified by the sudden appearance of a white, opposed no resistance), the party returned to the Seram-Lamut Islands, where M. Maclay studied the mixed race from the crossing of Malayans with Papuans. The anthroplogical results of these studies have appeared in the above-mentioned periodical as an appendix to the paper entitled "Meine zweite Excursion nach Neue Guinea," 1874.

The Papuans of the Koviay coast are a very interesting race of aquatic nomads. They were centuries since in relations with Malayans, who came to New Guinea especially to purchase slaves, exported to a great extent to the Malayan Islands. The slaves were formerly purchased among the inhabitants of the sea-coast; but to have more slaves these last have begun to make raids on the highlanders, who took revenge by raids themselves, so that the inhabitants of the coast were compelled to abandon all their villages. They are living now in covered boats, and continually cruise in them along the shore in search of food, landing only during storms, for in the night, at a few well-known places, where they are safe from attacks by the highlanders. The Malayans have introduced among them the use of gold, opium, and fire-arms, and they are very miserable.

From the Koviag coast, M. Maclay returned to Java, but soon undertook a fourth journey to New Guinea, to the southern coast, in order to ascertain the existence of a yellow Malayan race, which was mentioned several times by missionaries and travellers. After an eleven months' cruise on board a schooner, during which he visited the Solomon and Luisiada Islands, M. Maclay stopped on Teste Island, and thence proceeded on board a schooner to Port Maresby (Anapuata), on the southern coast of New Guinea. During his visits to the neighbouring villages, he perceived, indeed, a mixture of Polynesian blood among the Papuans. These metiss have a lighter skin and uncurled hair. They have also taken from the Polynesians the use of tattooing; all women tattoo themselves as long as they have children, and M. Maclay remarks that not only himself, but also many Europeans, find that the tattooed Papuan women are really better looking than the un-tattooed. They cover themselves with tattooing from the forehead to the feet, and often shave the head to tattoo it. The men are tattooed only to exhibit some of their exploits; by simply looking at a tattooed man you can say how many foes he has killed. The south coast is inhabited by the same Papuans as the other parts of New Guinea. Here also brachiocephalic skulls are not uncommon; but the skulls are also distorted, as the women used to bear loads on their backs, in bags that are attached by a rope to the head. The transversal depression of the bones at the *Satura sagitalis*, which results from this custom, is met with very often, and must be transmitted by heredity.

M. Maclay made a fifth visit to New Guinea on board an English man-of-war, to exercise his conciliating influence on the commander, who was going to burn a whole village and destroy the 2000 inhabitants, in order to punish them for killing four missionaries. The visit was very short.

M. Maclay concluded his lecture with a few remarks on the influence of the whites on the inhabitants of the south coast of New Guinea. Whilst rendering justice to the efforts of the London Missionary Society, who spread, by means of their black staff, the Christian religion, and teach the natives to read and write, M. Maclay pointed out that traders follow immediately the missionaries, and spread among the natives diseases, drunkenness, and the use of fire arms, which completely counterbalance the good influence of the very small amount of knowledge that might be spread by missionaries. The London Missionary Society does not allow its members to be at the same time the bearers of religion and of the above-said "benefits of civilisation"; but several missionaries of other societies appear in both these qualities. M. Maclay hopes, however, that the climate of New Guinea will be a good ally of the natives in their struggle against the white.

THE AURORA

WE have received the following further communications relating to the electric storm and auroral display of November 17:—

HAVING read in the English journals how very extensively and simultaneously the remarkable display of aurora borealis was observed in Europe and the United States, I beg to forward the inclosed report from Prof. Tacchini (see below), taken from a newspaper in Rome, describing that splendid phenomenon as it appeared in this country on the evening of the 17th inst., which probably may interest some of your readers. I would merely add that on the evening in question I was travelling between Spezzia and this city, when my observation was absorbed by the brilliancy of the beautiful phenomenon as seen from a railway carriage, and which accords very closely with the appearance of it in Rome. Soon after sunset the north-western sky was diffused with richly-coloured roseate tints blending into crimson at the horizon, which continued up to 7 p.m.; the transparency of this apparently roseate cloud was also a very remarkable feature, for the stars of the Great Bear were seen through it with little diminution of lustre; the sunset was very noticeable, which I remarked before branching from the coast where I had the sea horizon, and I never saw a more distinct and clear disappearance of the sun at sea below the horizon, even to the clearness of the atmosphere. Aurora borealis is so seldom seen in this country that its appearance caused much public curiosity. ERASMUS OMMANNEY

Florence, 12, Lungarno, November 30

THE following account of aurora borealis, seen on the 17th ult., at the Observatory of the Roman College, was sent by Prof. Tacchini to the Roman journals:—

"Yesterday evening (the 17th), a few hours after sunset, a fine aurora borealis appeared on our horizon. Besides the magnificent rosy arch melting away above, I saw, below, the so-called dark segment, which had a most lovely azure-greenish colour.

"At 5h. 50m. the red ribband rose more than 30° above the horizon, but at 5h. 55m. clouds suddenly covered almost the whole of that part of the sky occupied by the aurora, and a storm, with lightning, arose in the north. At 6h. 18m. there was a slight clearance, and through the aurora, which had already paled, shone some of the stars of Ursa Major. The highest point of the dark segment was precisely between the stars α and ζ of that constellation, being about 14° above the horizon, and 17° from the north towards west, therefore nearly in the direction of the magnetic meridian, and with an amplitude of about 45°. The weather continued bad, and at intervals rainy, and at 6h. 32m. were seen the last traces of the phenomenon. From the auroral light only a very faint continuous spectrum could be obtained, but I could not make such observations at the most opportune moment.

"Several falling stars were observed through the aurora. A magnetic perturbation occurred yesterday, and in the night, and continued also to-day; and, moreover, there is on the sun a large spot, easily visible on using merely a piece of smoked glass.

"The large diameter of this spot is slightly less than the thirteenth part of the apparent diameter of the solar disc. The spot appeared on November 12, at the eastern limb in the sun's boreal hemisphere, and on the 12th and 13th magnetic perturbations occurred. Yesterday I could not observe it well, because of the bad weather; but the day before, clouds of hydrogen were seen on its nuclei, and this morning still the phenomenon is most brilliant, demonstrating the greater intensity of solar phenomena over the spots in the atmosphere of the sun, which may thus be called solar auroras.

"Again, the magnetic perturbation of yesterday and last night is connected with that vast storm depression, which embraced a great part of Central Europe and especially Italy.

"We will further record here, that in the beginning of last October another aurora borealis was observed, and that then also there were strong magnetic perturbations in the earth, and large spots on the sun, seen on the limb on September 25.

"The Director of the Telegraphs has announced that very great perturbations occurred yesterday on all the lines, and from Belluno, Milan, Turin, Moncalieri, Venice, Porto Maurizio, Parma, Modena, Genoa, Luveno, and Viesti have come telegrams, showing that in the north the phenomena must have been very splendid. From Venice the Director of the Observatory states that yesterday morning at 4 o'clock, gleams of auroral light were observed. "P. TACCHINI

"Observatory of the Roman College, November 18"

I AM afraid you must have been overburdened with auroral communications, but perhaps you will kindly allow me on this occasion a little more space. Mr. E. Dowlen witnessed at Medway, Poynton, Cheshire, but little of that of the 17th; but on the 13th saw an auroral haze with shafts of white light, at 6 p.m. in the north and north-west. This had been preceded by a rose-red sunset, unlike an ordinary one, and accompanied by magnetic clouds. He also noticed an auroral glow on several subsequent nights.

On Friday last (the 24th) the Rev. W. Pearce saw a fine aurora at West Horsley, about six miles east from here. It commenced about 9h. 15m. by a yellow glow in the north-north-east and north-north-west, which increased in brightness and rose upwards, until at 9h. 30m. the Great Bear was hidden by it. It then changed to a rose tint, and spread laterally; was at its greatest brilliancy at 9h. 50m., and disappeared at 10h. 15m. Mr. Prince, of Crowborough (who, from the movements of certain insects and the magnetic disturbances, anticipates a severe winter), remarks that the "bright beam" must have been like a row of patches of light he saw on last October 3, southward and nearly parallel with the auroral arch northward. As some of your correspondents seem to ascribe a meteoric character to this beam, I may add I examined it carefully with a large Browning direct-vision spectroscope designed for auroral observations, and found only the well-known citron line, and none other. Also a faint greenish-white continuous spectrum extending a short way from that line towards the violet. This might have been auroral or from moon reflection. I had just previously examined the sky in that direction, and found no auroral line.

Mr. Saxby's letter is interesting in fixing approximately the position and height of the beam, especially when read in connection with Messrs. De la Rue and Müller's vacuum experiments and their table of heights assigned to auroræ, and it is still paradoxical that if such electric displays be within the limits of our atmosphere the air-spectrum is conspicuous for its absence, while it is replaced by one the principal line of which is not found in any other form of matter in the sky or on the earth.

On the other hand this point would not be inexplicable if the aurora be considered a something *per se*, as, for example, phosphorescence (strongly marked in the recent auroræ), excited by the electric discharge. That in such case it might wholly or in great part appropriate the spectrum to itself is shown by the instances of indium, thallium, and some other volatile metals which, when used as electrodes for the condensed spark, give spectra in which the air lines are either absent or faint, and when burnt in the arc have a similar effect on the carbon lines. I have elsewhere pointed out the probability of the aurora being referable to a form of phosphorescence.

The moonlight was unfortunate as regards the masking the fainter lines of the spectrum. I see one record of a faint red line, but except this of no other lines. If any of your readers have fixed the other lines, you will no doubt find space for so important an observation, for it is curious how little we know of the exact positions of these. I believe the measurements of a full set of the auroral lines made by my friend Prof. Vogel of Potsdam, in April, 1872, still remain the only standard, and as we now seem at an auroral period I would earnestly urge upon spectroscopists their special attention to these fainter lines, with a view to fixing their positions. This, too, is important, as there is a suspicion they are not always the same in different displays. The mode of doing this is not, however, very easy. If the spectroscope is of very small dispersion, the lines will be too close for useful measurement. With one of larger dispersion the introduction of a comparison spectrum or an illuminated micrometer scale will swamp the lines. A single illuminated point or line working across the field, the eclipsing the lines successively behind a diaphragm of tinfoil (as suggested by Mr. Lockyer), and a scale photographed on thin glass, through which the lines are seen, and which is itself illuminated by the spectrum, are severally better methods, and might perhaps yield some available results.

Guildown, Guildford, December 1 J. RAND CAPRON

THE unique nature of this meteor must be the excuse for adding another letter on the subject. Your correspondents, Mr. Taylor of Heworth Green, York, and Mr. Elger of Kempton, have kindly answered queries of mine as to the exact place of the passage; these stations being the most important, after the transit stations of Woodbridge and Old Windsor. After Mr. Saxby's letter of November 30, any farther notice may seem superfluous, were it not that the elements he assigns cannot explain the observations. At York the meteor could not have appeared at only 8° altitude, and it is described as 6° under the moon, or 19° alt.; and the passage from Woodbridge to Bristol could not occupy over two hours (at a mile a minute), as the whole difference of time is certainly only a minute or two. We must then seek for more consistent elements.

From the York, Bedford, and Old Windsor observations, the meteor was at about 170 miles elevation, allowing the first station half the weight of the second. Or, combining York and Bristol, which were more nearly simultaneous, it was at over 300 miles elevation. Its visible passage of about 200 miles in length did not occupy two minutes, and was so brief as to be masked by the watch errors of observers; it therefore moved more than 100 miles a minute. Again, it was two minutes in view, by Greenwich; and it passed the meridian with at least twice its mean apparent velocity (as most observers mention its lingering in both east and west); this, with the least height of 170 miles, gives a minimum of fifteen miles per second for its velocity. Another proof of its height is, that though seen in Sweden, yet it appeared to form and pause at 10° alt., as seen at Bristol and Heworth, and did not come up from the horizon.

Can it be supposed that an auroral ray would sweep over 1000 miles from Sweden to Sidmouth, with a velocity of over fifteen miles a second? This is, however, just the velocity of planetary matter; and apparently the most probable explanation of it is that it was a cloud of meteorites ("quite unlike an auroral ray," says Mr. Capron) which just escaped grazing the earth's surface. In this case their velocity would be at least over twenty miles a second, moving in about the plane of the earth's orbit, and crossing the earth's path at least at 45°, or more radially. Perhaps some computer will work out the path approximately, as other meteors have been so discussed.

Such a cloud of meteorites must have been at least 130 × 20 miles and 20 miles deep if cylindrical, and was apparently accompanied by a smaller cloud, as seen at Clevedon. As it was seen brightly in the moonlight, and yet scarcely dulled the moon in crossing it, the visual area of the solid mass might be about a tenth of the whole area of the cloud; so that if the particles were as dark as the moon, the cloud would reflect one-tenth as much sunlight in an equal visual area. If then the mean diameter of the meteors was but 1 inch, their volume would equal a sphere of 800 feet diameter, and would have thrown down a rain of meteors, averaging ninety one-inch balls to the square foot, over a district about twenty miles across.

Falling meteors lose practically all their velocity by friction in the atmosphere, before they strike the earth; since travelling at even 15 miles a second, they would be heated to over 1,000,000° F. by arrest, and yet they do not show in themselves or by their effects, a sign of a thousandth of this heat. All this heat then is produced in the air; and if a meteor strike the earth obliquely, it will be checked and fall within a very few miles. All the heating of the air must thus take place within a small area, in whatever way the meteor may strike. The result then of such a meteor cloud as has been just seen, hitting the atmosphere (as it only escaped doing by a quarter of a minute) would be to heat the air for some twenty miles in each direction to about 10,000° F., or still more if the arrest occurs entirely in the upper regions. This hot air would quickly rise, and spread out above the cooler atmosphere, causing a great in-suck along the earth from surrounding parts. On the upper surface it would quickly cool by radiation into space; and the effects of such a shower to terrestrials would be a terrible gale, blowing towards a centre and upwards, with considerable heat radiating from above.

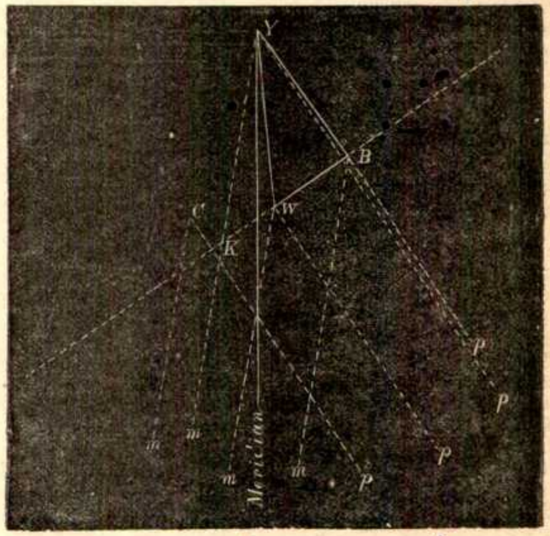
W. M. FLINDERS PETRIE

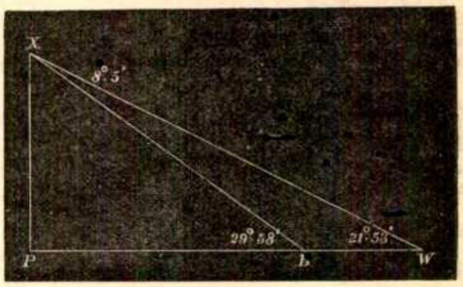
Bromley, Kent, December 2

CONCERNING the apparition during the aurora of the 17th, I ought to have stated the apparent altitude angle between it and the moon when at nearest approach, but as the angle was larger here than anywhere south, it was more difficult to estimate; but I think it was about 12 moon-breadths, at the very most or 6° (centre to centre). Also I foolishly forgot to note the *exact* time, but it was 4 or 5 minutes past 6 p.m. I saw no repetition of the phenomenon for 5 minutes after, and I then went indoors. It is evident that if it was the same object that was seen to transit the moon's disc both at Woodbridge (near Ipswich) and Windsor, that it must have followed a path from north-east by east to south-west by west (astronomical) since the intersection of its plane of motion with the plane containing York, Woodbridge, and Windsor, lies in that direction. Most of the observers state that it seemed to appear about east, and disappear south of west. Let Y, W, and B represent York, Windsor, and Woodbridge respectively in their relative positions. C represents Clifton.

We have YB = 162 miles.
YW = 172 ,,
BW = 94 ,,
CK = 40 ,,

If it be supposed that the object pursued a *nearly straight path*, keeping at a nearly constant height (and this is not inconsistent with the observations), then it ought to have reached its greatest angle of elevation along lines drawn perpendicular to line BW from each place of observation (it seemed to do so at Greenwich, Bedford, and Cambridge, though all the observers do not state whether they reckoned by magnetic or astronomical bearings). The moon was about 8° past meridian, and at York the altitude was about 24°, and at Woodbridge 26° (there being 2° of latitude between). If we consider the angles with respect to the *plane* YBW, which simplifies matters, then elevation at York = 25°, and at Woodbridge the same; and as





angle between directions Ym and YP = 36°. Then tan of angle of culmination at York will be equal to

$$\frac{\tan(25^\circ - 7^\circ)}{\cos 36^\circ} = \tan(21^\circ\ 53'),$$

and tan angle of culm. at Woodbridge will be equal to

$$\frac{\tan 25^\circ}{\cos 36^\circ} = \tan(29^\circ\ 58').$$

I am supposing the angle below the moon to be 7° for the sake of not exaggerating the height.

York and Woodbridge are in a line almost at right angles to BW. Then the parallax of the object when seen along this line (being the line of culmination) = 29° 58′ − 21° 53′ = 8° 5 about.

Thus in diagram No. (2) we have—

$$Bx = 160 \text{ miles} \times \frac{\sin(21^\circ\ 53')}{\sin(8^\circ\ 5')}$$

and required height $xP = Bx \sin(29^\circ\ 58')$.

This, when worked out, gives the astonishing height of 212 miles above the *plane YWB*. Nor can I see how this result can be lessened in any way, for I have allowed an exaggerated parallax. Again, if the mysterious object was *not* pursuing a path almost straight and parallel to the plane *YWB*, as I have supposed, for the sake of a rough calculation, it must have travelled in a *crooked* one, for which there will be evidence forthcoming no doubt. Now Clifton is forty miles off the line *B W*, and as Mr. A. M. Worthington carefully estimated the depression below the moon from centre to centre to be scarcely $3\frac{1}{2}$ moon-diameters, or about $1\frac{3}{4}$ degrees, then at York, which is 160 miles off line *WB*, the depression ought to be $1\frac{3}{4}° \times 4$ nearly = nearly 7° (but it was not so much). I and Mr. Worthington would see it beneath the moon nearly at the same time, he a little later than I. If the height should be anything near 212 miles, then we ought to hear of it being seen overhead in the north of Italy and Southern France, and it would be 200 miles or so in length. I hope that more accurate observations will be forthcoming to enable some scientific man to calculate the path of this strange apparition with some accuracy. Of course, if the thing laid straight along its path, it would appear to observers in England to be curved along its trajectory, as it did to me. I ought to say that at its apparent formation it was partly obscured by cloud in the S.E.E. (astronomical).

Heworth, York, November 26 H. DENNIS TAYLOR

P.S.—Might I be allowed a little more space just to state that my estimate of the meteoroid's depression below the moon is considered far too much by my mother, who, happening to look out at the same time from a window, noticed it beneath the moon. She described it exactly as I had seen it, but did not notice its movement, as she only looked for a few seconds. If there had been two similar appearances at the same time, I do not see how I could have failed to notice them. Mr. S. H. Saxby estimates its height to have been 44 miles, but he will see that if that were so, then I ought to have seen it pass 17° below the moon. One cannot reasonably suppose that a different object of the same nature has been seen from the South of England, from the one that I saw. I see that Mr. A. Batson has observed it crossing the moon exactly from Hungerford, which place is in almost a direct line with York and the moon at the time of observation. Our observations would be simultaneous, and they give a height of 192 miles. The course of the meteoroid would be 22° south of west, almost as Mr. S. H. Saxby states. Being very anxious to obtain more exact data from observers in Yorkshire I sent a letter asking for information from any such to the *York Herald*, but it has not been inserted. Is there anything inherently improbable in supposing this phenomenon to have been at a height of 190 miles, for have not rapid shooting stars now and then been seen incandescent at nearly that height, indicating the existence of an attenuated atmosphere.—H. D. T.

December 3

ON Friday November 17 last, as I was walking along the north side of Lincoln's Inn Fields, at about 6 p.m., my attention was attracted to the moon, which was then shining brightly in a cloudless sky. I observed a broad band of light having somewhat the appearance of a light cloud, only much brighter, moving across the face of the moon from east to west, which was the direction of its (the light's) long diameter. It appeared to me to extend above and below the moon to about the distance of the moon's diameter, and to be in length about four times its own width; when it had passed about half its own length from the moon, it seemed to disappear entirely. The time during which it was visible, I should think, was not more than half a minute, probably not more than a quarter, and its movement across the moon as rapid as that of a cloud when a very high wind is blowing. EDWARD POLLOCK

20, York Terrace, Regent's Park, December 1

A GREAT manifestation of aurora was visible here last night. It attracted my notice at 11 p.m. At the time of observation by me the aurora was very active, projecting white streamers from a point in the south-west, and these, crossing the zenith, faded in the south-eastern sky. There was a stiff, cold north-west wind blowing, and the night was frosty. No prismatic colours were noticeable, only the usual green auroral glow in the north-west sky, where it was crossed by the shooting streamers. A grand band of vapour rested on the western, north-western, and northern horizons. In the east and north-east was a soft blue sky. The display seemed to me to last throughout the night, and to continue through the day; as all day long, at intervals, streamers shot up from a bank of clouds in the north-west horizon. At 5.30 p.m. this evening there was a powerful auroral glare in the west and north-west. After that time a cloud canopy formed and hid the sky. The weather here in the afternoon of Monday was stormy, with a rising barometer and a falling thermometer, wind nearly a gale, hail, rain, and snow falling at intervals. X.

Worcester, November 28

In NATURE, vol. xxvii. p. 548-9, and 571, will be found accounts of the aurora borealis, as seen by your correspondents on Monday evening, October 2 last. I wish to draw attention to the fact that a grand Aurora Australis of magnificent appearance was visible in Australia on Monday evening, also on October 2, but of course was seen by our Antipodean friends about twelve hours before the one seen at this end of the globe. The reports that I have of the Aurora Australis are from Adelaide, Melbourne, Sydney, Sandhurst, Ballarat, &c. So brilliant was it that the firemen turned out, imagining that there was some enormous conflagration in their neighbourhood. This concurrence opens up the question, was there any connection between these two displays? J. FRANCIS COLE

Westfield, Sutton, Surrey, November 28

BY kindness of Astronomer Royal, Greenwich, I am able to add the exact position of moon at Ramsbury, November 17, inst., at 6h. 2m.:—

R.A. = 21h. 12m. 56s.
N.P.D. = 100° 35′ 7″.

At this time the hour angle of the moon was 35m. 49s., or 8° 57′ 15″ west of the meridian.

The above is the most accurate observation possible for calculating the real position with regard to the earth.

Ramsbury, Wilts ALFRED BATSON

I DO not know whether you will publish more auroral accounts, but if you do, the inclosed seems very interesting. The phenomena, as seen in the north, differed much from *our* views of them. J. RAND CAPRON

Guildown, December 4

"A singular pinkish light appeared in the western sky between 5 and 6 p.m. At the same time I noticed a light of a peculiar yellowish white rising up from the eastern horizon. The general appearance was that of two conical-shaped lights about 40° to 50° wide at base, east and west horizon, their apexes meeting at or about the zenith, z. The whole of the northern sky was more or less illuminated, but much more marked in the transverse streaks extending east and west, or nearly so in the former case, deepening to a rich crimson pink towards the western horizon, and to the eastern horizon a bright yellowish white. Its southern termination was a well-defined sharp outline forming an arc about 30° to 40° from the south horizon, inside which the sky appeared almost black by contrast, the new moon lending additional interest to this peculiar atmospheric display.

"F. R. CLAPHAM

"Austwick Hall, Clapham, Lancaster, December 1"

WITH reference to J. E. Clark's remarks on p. 85, I would remind your readers that Sophus Tromholt, of Bergen, has organised a system of simultaneous observations on auroras, and that he will supply forms for recording them to any observer who will apply for them. I am not aware whether he has yet arrived at any definite results as regards the height of auroras; nor do I know whether he is making this specially a subject for investigation; nor whether he has enlisted the services of many observers in Britain. Surely J. E. C. is in error in saying that a height of 100 miles is far greater than is now usually supposed. In works on auroras, far greater heights are given, and I am not aware that these have ever been disproved. It is obvious that the curious spindle-shaped beam seen on the 17th must have been at an enormous height.

Sunderland, December 4 THOS. WM. BACKHOUSE

The past week has been one of remarkable electrical disturbances. Auroras were visible Tuesday evening, November 14, all Friday night, Saturday evening, Sunday night, Monday morning, and Monday evening. It was cloudy in this vicinity between the 15th and 16th, and if there were auroræ they were not visible. The aurora of Friday evening, following an intense magnetic storm, was remarkably brilliant, and lasted all night.

During the earlier part of the evening all the visible northern hemisphere was covered by it, but later, about midnight, all the visible heavens, to within 20° of the southern horizon, was covered by straight streamers extending from all points of the horizon to the zenith, where they formed a boreal crown of blood-red colour. The streamers were pulsating towards the zenith, making the sight a peculiarly magnificent one. Early in the evening the arc to the north was about 10° in elevation, and then gradually raised, showing the rich folds bordered by a dark fringe of a magnificent waving curtain, until it reached nearly to the altitude of Polaris. The southern boundary, also bordered on the south by the dark band, seemed to be nearly at right angles to the circle of the northern arc. Monday evening, the 20th, all manifestation was confined to the south. In a point in the south-east, near where Foucalhaut then was, rays shot northward past the zenith, but instead of converging, the rays diverged like the fingers of one's hand. The horizon, too, in the south, seemed much lighter than in any other direction. Though moonlight, the rays could be plainly seen to within 5° of the moon. It may be remarked that of the spots on the sun during this period of disturbance, one has been visible to the naked eye.

L. G. CARPENTER

Agricultural College, Michigan, Lansing, Mich, U.S.A.
November 21

The electrical storm seems to have been as violent in America as it was in Europe, as will be seen from Prof. G. L. Carpenter's letter above. The American papers of November 18 contain long accounts of the phenomenon. The *New York Times* says:—

"Yesterday's storm was accompanied by a more serious electrical disturbance than has been known for years. It very seriously affected the workings of the telegraph lines both on the land and in the sea, and for three hours—from 9 a.m. until noon—telegraph business east of the Mississippi and north of Washington was at a stand-still. An aurora borealis was the first evidence of the overcharging of the atmosphere with electric fluid. This appeared at about five o'clock yesterday morning, and was brilliant in the extreme. At the same hour trouble began to be experienced in the action of the telegraph wires. The circuits were broken, and the usual annoyances accompanying such disturbances were manifested. These increased in intensity until nine o'clock, at which hour it became impossible to transmit messages over the wires having an earth circuit—that is, where the ends of the line were grounded. Such lines as had a metallic circuit worked all right throughout the day, however, and so some little business was transacted over isolated lines. The disturbance continued until 1·50 p.m., when the electric storm seemed to have ceased. During the electric storm Mr. Brown, the chief operator, stated it was impossible to work the cables at all, except by cutting off the ground wires and making a metallic circuit by connecting the land-ends of two cables. This was done, but even then the cables worked in a very unsatisfactory manner From all the central offices complaints came to the general office of the failure of the lines to work. People who attempted to use the telephones heard a buzzing, ringing noise, rather than any well-defined sound while attempting communication, and occasional words only could be distinguished. A singular fact in connection with the storm was that the wires of the Law Telephone Company did not seem to be affected. Engineer Shaw stated that they had had no more trouble during the day than usual, and attributed this to the fact their lines are all short ones, and therefore less liable to be affected than the longer lines. Their wires are ground circuits, and their freedom from annoyance is a mystery that he can solve in no other way than the one suggested.

From Chicago, under date November 17, the following details of the disturbance were sent to the *New York Times*:—"Officers of the Western Union Telegraph Company there say the electrical disturbance was the most pronounced and wide-spread experienced for years, if indeed it has been paralleled at any time. An electric storm of the greatest violence raged in all the territory from New York to points beyond Omaha, and from Kansas City north to the terminus of telegraphic communication, practically putting a stop to the telegraphic service over the entire area. It first began to be felt about 4 o'clock this morning and increased in intensity till 9.45, when communication from every direction was cut off. This electric storm seemed to go in successive negative and positive waves, alternately neutralising the currents on the wires or increasing their intensity to such a degree as to burn everything up. The switch-board here was on fire a dozen times during the forenoon, and half a dozen keys of the instruments were melted by the current which continued to pass through. The screws burned up and the points parted to their furthest limits. The duplex and quadruplex wires were rendered entirely useless, and at noon only a single wire out of fifteen between this city and New york was in operation, and it was frequently interrupted. Word was received from Milwaukee that the atmospheric electricity coming in on one of its wires from the country had such dynamic power as to keep an electric lamp burning."

Somewhat similar observations were made at Washington. On the Chicago and Cincinnati circuits it was found impossible to work the quadruplex instruments, and they were taken out. The chief operator said that the magnetic interference was greatest on the east and west lines. The officer in charge at the office of the Signal Service, said that great trouble had been experienced in collecting the weather reports on account of the general demoralisation of telegraphic circuits.

Similar reports were sent from Cleveland, Indianopolis, Cincinnati, Milwaukee, Nashville, Bangor, Toronto, and other places. At Cleveland the disturbance was first observed at 4 or 5 o'clock in the morning. From Milwaukee it was reported that "Strong currents of electricity pervaded the atmosphere and actually suspended all telegraphic communication from 9 o'clock in the morning until afternoon. An electric lamp attached to a St. Paul wire produced a brilliant illumination without the use of a battery. Business on 'Change was virtually suspended on account of the lack of telegraphic facilities. At 2 p.m. all the telegraph offices resumed work."

The *Detroit Evening News* states that "telephone communication all over the country was greatly improved, the pronunciation being distinct and much louder than usual, which fact may suggest to electricians an improvement in telephonic communication. Another unusual thing was that the electrical storm prevailed during a cloudy sky and murky atmosphere; heretofore such storms have occurred during a clear atmosphere. With the approach of night and the clearing away of the clouds came a most beautiful spectacle of the electrical agitation of the atmosphere. A more magnificent display of aurora borealis was never seen. It became slightly visible just at dusk, and increased in brilliancy and variety of form, movement, and colour, until midnight, when the whole vast heavens was one grand canopy of dancing flames of every conceivable hue and shape moving in all directions."

At Omaha the aurora was very brilliant, the illumination rendering the night almost as bright as day. At St. Paul the sky was of blood red colour, the display being grand and fearful. Cheyenne reports the illumination at that point as bright as day. At Denver the display in the northern heavens was most brilliant and dazzling. In California the aurora was visible from the northern part of the State as far south as San Diego, and was most brilliant. At Olympia, Washington Territory, the aurora was magnificent, the heavens north and east being brilliantly illuminated.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

LONDON.—At a meeting of the Council of University College on Saturday last: 1. Mr. H. F. Morley was authorised to give a course of advanced lectures on Organic Chemistry. 2. It was resolved to invite Mr. T. W. Rhys Davids to accept the Professorship of Pali and Buddhist Literature, once held by the late Prof. R. C. Childers. 3. It was resolved to ask Mr. R. H. Gunion to take the office of Lecturer on Sanskrit. 4. The resignation of the Chair of Physiology by Prof. Burdon Sanderson was accepted.

MANCHESTER.—A public meeting was held last week to inaugurate a movement for the extension of Owens College by the addition of a museum, which is expected to cost between 50,000*l.* and 60,000*l.* It was stated that there were a few thousand pounds in hand available for the purpose, and it was resolved to ask the public for 50,000*l.* to erect and equip the museum. Fourteen subscriptions of 1000*l.* each and a number of others ranging from 100*l.* to 500*l.* each were announced in the room. Lord Derby, the Duke of Devonshire, Mr. Hugh Mason, M.P., and Mr. Grafton, M.P., each offered 1000*l.*

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 12.—On the volume-changes of water containing salts on heating, and the resulting chemical transpositions, by E. Wiedemann.—On the molecular refraction of sulpho-carbonic ether, with some remarks on molecular refraction in general, by the same.—On the dispersion of colourless transparent media, by A. Wullner. —Some remarks on the papers of Herren Hasselberg and Goldstein, by the same.—On galvanic elements supposed to consist only of elements, and the electromotive useful effect of chemical processes, by F. Braun.—The electric conductivity of chloride, bromide, and iodide of silver, by W. Kohlrausch.—On methods of multiplication and rejection, by E. Dorn.—Contributions to a knowledge of the relations between fluidity and galvanic conductivity, by C. Stephan.—On the joint action of traction and torsion in metallic wires, by F. Himstedt.—On the connection between the units of magnetism and of electricity, by R. Clausius.—On the theory of Fresnel's integrals, by A. Lindstedt.—On the theory of elastic reaction, by E. J. Michælis.

Journal de Physique, November.—On the methods to employ for determining the ohm, by L. Lorenz.—On the electro-chemical figuration of equipotential systems, by A. Guébhard.—On the liquefaction of ozone, by P. Hautefeuille and J. Chappuis.—On the absorption spectra of ozone and pernitric acid, by J. Chappuis.—Application of instantaneous photography to the study of animal locomotion, by G. Demeny.

Journal of the Russian Chemical and Physical Society, vol. xiv. fascicule 7.—On the action of the cyanide of ammonium on glyoxal, by M. N. Lubavin.—On the decomposition of the acetate of tertiary amyl by heat, by Prof. Menshutkin.—Analysis of the water which accompanies naphtha in wells, and ejected by mud-volcanoes, by M. A. Potilitzin.—On new beds of mineral manure, by M. P. Grigorieff.—Analysis of naphtha coke, by M. A. Lidoff.—Residual elasticity and analogous physical phenomena, by M. N. Hesehus.—Review of Russian chemical literature for the year 1881, by M. N. Lubavin.

Archives des Sciences Physiques et Naturelles, October 15.—On cometary refraction, by G. Cellerier.—On the duration of excitability of nerves after the separation of their nutritive centres, by O. Gorlinsky.—Researches on lodes, by F. Sandberger.—The grain of the glacier, by E. Hagenbach-Bischoff.

November 15.—Sixty-fifth session of the Helvetic Society of Natural Sciences, held at Linthhal on September 11, 12, 13, 1882.—The prehistoric antiquity of man, by G. de Mortillet.—The origin of cultivated plants, by A. de Caudolle.—New researches on the appearances of Jupiter, by E. W. Hough.

Journal of the Franklin Institute, November.—An improved feed-water heater and purifier, by G. E. Strong.—Economical steam power, by W. B. Le Van.—Note on the pendulum, by J. R. French.—Vision by the light of the electric spark, by W. Le Conte Stevens.—Notes on water analysis, by R. Haines.—Report on European sewerage systems, &c., by R. Hering.—Examination of water and air for sanitary purposes, with remarks on disinfection, by R. Hitchcock.—Report of Committee on the Rappleye rheometric governor burner.—The silver and gas dynanometers, by L. H. Sargent.—The American iron trade in 1881.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 8.—"Note on the Discovery of Bacilli in the Condensed Aqueous Vapour of the Breath of Persons affected with Phthisis." By Arthur Ransome. Communicated by Dr. W. Roberts, F.R.S.

In the year 1869 the author had examined the aqueous vapour of the breath in health and disease. This vapour was condensed in a glass globe surrounded by ice and salt, and, in condensing, it was found to carry down all the organic matter contained in the breath. It appeared probable that the breath of persons in advanced stages of phthisis would contain the bacillus of tubercle, and that this organism could be rendered visible by Dr. Heneage Gibbes' method of staining.

The aqueous vapour of the breath of several advanced cases of phthisis was accordingly condensed by the above-mentioned method, and each specimen was separately examined.

In order to carry down the organic matter, and to afford a basis to attach the material to the microscopic cover glasses, fresh white of egg, or a little mucus, free from bacilli, was added to the fluid.

No attempt was made to sterilise the fluids, as the ordinary bacteria of putrefaction are not stained by the process used.

In the aqueous vapour obtained from two of the cases, specimens of bacillus were found which took the staining in the same manner as the bacillus found in phthisical sputa and in tubercle.

The organism was not found in several other cases, nor yet in the aqueous vapour condensed in the waiting-room of the Manchester Consumption Hospital.

Physical Society, November 25.—Prof. Clifton, president, in the chair.—A paper by Mr. William Ackroyd, on rainbows produced by light reflected before entering the rain-drops, was read by the Secretary. The author investigated mathematically the rare phenomenon of three bows, and inferred that it would generally take place about sunrise or sunset. Mr. Lecky thought the effect had a simple explanation. It might be said to be due to two suns, one (reflected) appearing to be below the horizon.—Mr. Shellford Bidwell gave an account of some experiments he had made to test the theory of Dr. James Moser, that the action of a selenium cell under light was due to the heat rays making a closer microphonic contact between the selenium and the metal electrode, by expanding the material. He submitted selenium cells to dark heat rays, and found their resistance to *rise*. Under light rays, however, their resistance fell. He therefore concluded that Mr. Moser's theory was erroneous, and that the fall in resistance due to the light rays is the differential result of the rise due to heat and the fall due to light. He also explained the "fatigue" of a selenium cell by use, as caused by its increase of temperature. When the cell cooled again the fatigue disappeared. Dr. Moser and Prof. G. C. Foster made remarks on the paper, the former suggesting experiments to test the reversability of the effects observed by Mr. Bidwell, and the latter seeking to reconcile Mr. Moser's theory with the new data.—Dr. James Moser then read a paper on a general method of strengthening telephonic currents. This consists in forming a primary circuit of the telephone transmitter or derived circuit, a set of induction bobbins in derived circuit, and a changed secondary battery, the whole circuit having a very low resistance. Each primary bobbin has a secondary wound over it, and these secondaries are connected in quantity to the telephone line, which has at its remote end a set of telephones in derived circuit to the earth or return wire. In this way one line wire serves to supply a large number of separate telephones, a hundred being employed by Dr. Moser to transmit music from the Hippodrome in Paris to the Place Vendôme. The system is applicable to long lines; and the induction noises are reduced by subdivision among the separate telephones.

Victoria (Philosophical) Institute, December 4.—A paper by Dr. Miller was read on the references to the Antediluvian period in the cuneiform texts.

Institution of Civil Engineers, November 28.—Sir F. J. Bramwell, vice-president, in the chair.—The paper read was on "American Practice in Warming Buildings by Steam," by the late Mr. Robert Briggs, M. Inst. C.E., of Philadelphia, U.S.

CAMBRIDGE

Philosophical Society, November 27.—On complex multiplication of elliptic functions, by Mr. A. G. Greenhill.—On certain points in the function of the cardiac muscle, by Dr. W. H. Gaskell.—On the development of the Pollinium of Asclepias, by Mr. T. H. Corry.—On some micro-organisms and their relations to disease, by Mr. G. F. Dowdeswell.

BERLIN

Physical Society, November 17.—Prof. Helmholtz in the chair.—Herr Hagen has sought to determine the physical properties, and especially the coefficients of expansion, of metallic sodium and potassium, and he reported on the methods and results of this investigation. Both metals, which, in petroleum, in which they are commonly kept, always present a dull surface of hydrated oxide, Herr Hagen succeeded in keeping, with bright metallic surface, without petroleum, any length of time, in evacuated tubes, after the small amount of oxygen in the residual air had been fixed by a part of the metal in an ante-chamber. By melting the metal, drops could be formed, from whose heights the capillary constants of the two metals were found, viz. 34·23 for sodium, and 14·17 for potassium. The two metals, mixed in the ratio of their equivalents, gave an alloy which is liquid at ordinary temperature, and which, on account of its brilliant metallic surface, might be easily taken for mercury; only its greater specific gravity distinguishes the latter at once from the potassium-sodium alloy. This solidifies at about 4°·5 C., and its capillary constant is 17·86. Very careful experiments were made for determination of the coefficients of expansion, and their relation to the temperature, in suitable dilatometers. The linear expansions, deduced from the volume-expansions, were, for sodium, 0·000853, and for potassium, 0·000721; pretty similar values were had in direct measurement of longitudinal expansion in a metal block. This coefficient of linear expansion exceeds that of all other metals, and is about three times the linear expansion of lead.—Prof. Helmholtz then gave a report of this year's International Congress in Paris, from which he had just returned. The Congress having last year come to an understanding on the units occurring in electrical science and "technic," and their designations, the point now was to determine those units exactly, so that practical normal units might be prepared. Attention was first given to the determination of the unit of resistance,—the "ohm" (as most easily practicable); that is, the exact measurement in metres of the column of pure mercury of one square milimetre cross-section at 0° C., the resistance of which is the "ohm." There were already quite a number of measurements by methods which Herr Helmholtz specified in his lecture. The values obtained are: Herr Kohlrausch, 1·0593; Lord Rayleigh, by the British Association method, 1·0624; Lord Rayleigh, by Lorenz's method, 1·0620; Mr. Glazebrook, in Cambridge, 1·0624; Herr H. Weber, in Brunswick, 1·0611; Herren W. Weber and Zollner, 1·0552; Mr. Rowland, in America, 1·0572; Herr Dohrn, 1·0546. Against these pretty concordant values, however, stood the mean value obtained by Herr F. Weber, of Zurich, by reliable methods, and from experiments agreeing well together, viz. 1·0471, which came so near the older ohm of the British Association, that the Congress, on the motion of Sir William Thomson, refrained meanwhile from forming a definite conclusion. It was rather agreed to recommend the experimenters (1) to compare their resistances with the standard of resistance which the French Government will produce; (2) to compare the induction coils by the method adopted by Herr Kohlrausch with the wire-circuit; (3) in their measurements to avail themselves of the modified and still further to be improved method of Lorenz. The respective governments should finally be urged to support, as much as possible, the national experiments for determination of the "ohm."

PARIS

Academy of Sciences, November 27.—M. Jamin in the chair.—The following papers were read:—Observations of small planets with the great meridian instrument of Paris Observatory during the third quarter of 1882, by M. Mouchez.—Note on the verification and the use of the magnetic maps of Col. Al. de Tillo, by M. Lalanne. He compares magnetic observations (of declination) made by him in 1837, in four localities of the region north of the Sea of Azof, with Col. de Tillo's two maps (for Russia), and notes some defects of the latter (the longitudes of the two do not refer to the same meridian, &c.).—Reply to the objections of M. Decharme to my rational conception of the nature of electricity; proofs of the validity of hypotheses serving as the basis of this conception, by M. Ledieu.—General law of congelation of solvents, by M. Raoult. Every substance, dissolved in a definite liquid compound capable of solidifying, lowers its freezing-point. In all liquids, the molecular lowerings of congelation with different compounds, approach two values invariable for each liquid, and one of which is double, the other (the greater being normal). The normal lowering varies with the nature of the solvent. A molecule of any compound, dissolving in 100 mol. of any liquid, of different nature, lowers the freezing-point of the latter a quantity nearly constant, and near 0°·62.—Chemical study on maize at different epochs of its vegetation, by M. Leplay. Sugar is found in the leaves, and accumulates in the stem till the moment of formation of starch in the grains. It then migrates into the spike, first into the support of the grains, then into the grains themselves, where it is replaced by starch. This migration continues to be fed by the leaves till they disappear, then in great part by the stem—diminishing, however, as the starch is developed. The function of the sugar, then, is to furnish to the grain the elements of starch.—On the conservation of solar energy; reply to M. Hirn's note, by Dr. C. W. Siemens. He estimates the temperature of the photosphere as 3000°, not too high to satisfy the conditions of combustion (M. Hirn's estimate is 20,000°). The theory of diminution of light intensity as the square of the distance seems to be not applicable to the whole of the light of stars. Some wave-lengths less favourable to decomposition, may on this account reach further. As to mechanical resistance of gaseous matter to the planets, he shows reason for thinking it much less than hitherto supposed.—On a theorem of M. Tisserand, by M. Stieltjes.—Extension of the problem of Remann to hyper-geometric functions of two variables, by M. Goursat.—On a new integrometer, by M. Abdank-Abkanowicz. Increased accuracy is obtained by means of a disc which rolls on a cylinder without slipping.—On a mode of transformation of figures in space, by M. Vaneeck.—Equilibrium of elasticity of a solid limited by a plane, by M. Boussinesq.—Theoretic interpretation of the calming effect produced by a thin layer of oil spread on the surface of the sea, by M. Van der Mensbrugghe.—On electric motors, by M. Deprez. He describes an experiment proving that the two laws—that of independence of the current's mechanical action, of the state of rest or motion of the ring, and that of proportionality of the electromotive forces to the velocities (supposing, of course, the intensity of the current constant)—hold good within very wide practical limits.—General expressions of the absolute temperature, and of Carnot's function, by M. Lippmann.—Range of sounds in air, by M. Allard. Experiments with different instruments yielded the result that the intensity of sound in air decreases much more rapidly than according to the law of the square of the distance. The second cause of enfeeblement is considered to lie in the non-homogeneous character of the air. A given sound may have, apart from the influence of wind, very different ranges, varying, *e.g.* from two to twenty nautical miles. For small augmentations of range the work required increases very rapidly. The differences of range for different pitches within the octave are little sensible.—On the reform of some processes of analysis used in laboratories of agricultural stations and observatories of chemical meteorology (fourth memoir); volumetric determination of alkalino-earthy carbonates in waters, by M. Houzeau.—Modifications of structure of nerve tubes in passing from the spinal roots into the spinal cord, by M. Ranvier.—On the present flood of the Seine, by MM. Lemoine and de Préaudeau.—Magnetic perturbations from November 11 to 21, 1882, by M. Renou.—A letter from M. Tarry on the aurora showed that while the magnetic currents of earth lines render possible a pre-vision of aurora several hours in advance, those on submarine lines give a pre-vision of *several days* in advance.

CONTENTS

PAGE
RECENT RESEARCHES IN THE METAMORPHISM OF ROCKS. By Dr. ARCH. GEIKIE, F.R.S. 121
HUMAN MORPHOLOGY 124
OUR BOOK SHELF:—
  Wood's "Common British Insects" 124
  Ratzel's "Anthropo-Geographie oder Grundzüge der Anwendung der Erdkunde auf die Geschichte" 125
LETTERS TO THE EDITOR:—
  Mimicry in Moths.—The DUKE OF ARGYLL 125
  Double Flowers.—Dr MAXWELL T MASTERS 126
  Fruit of Opuntia.—Dr MAXWELL T MASTERS 126
  Hawk Moth Larva.—Surgeon-Major E. R. JOHNSON (*With Illustration*) 126
  The Fertilisation of Common Speedwell.—A. MACKENZIE STAPLEY 127
  Wartmann's Rheolyzer.—ERNST VON FLEISCHE 127
  Pollution of the Atmosphere.—H A PHILLIPS 127
  A Modern Rip Van Winkle.—SALTBURN 127
GOOLDEN'S SIMPLE DIP-CIRCLE (*With Illustration*) 128
THE COMET. By WILLIAM CRAWFORD WINLOCK, Assistant Astronomer, U S. Naval Observatory; Dr W. DOBERCK (*With Illustrations*) 128
FUNCTION OF THE MEMBRANA FLACCIDA OF THE TYMPANIC MEMBRANE. By JOHN M. CROMBIE 129
WEIGHTS AND MEASURES 131
ON THE PROPOSED FORTH BRIDGE. By Sir G. B AIRY, F.R.S. . . . 131
NOTES 132
THE ROYAL SOCIETY. Anniversary Address by Dr. WILLIAM SPOTTISWOODE, Pres R S. 134
M. MIKLUKHO-MACLAY ON NEW GUINEA 137
THE AURORA By Admiral ERASMUS OMMANNEY, F R S; Prof. TACCHINI; J RAND CAPRON; W M. FLINDERS PETRIE; H. DENNIS TAYLOR; EDWARD POLLOCK; J FRANCIS COLE, ALFRED BATSON; J. RAND CAPRON: F. R. CLAPHAM, THOS. WM. BACKHOUSE; Prof. L. G. CARPENTER (*With Diagrams*) 138
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 142
SCIENTIFIC SERIALS 143
SOCIETIES AND ACADEMIES 143

THURSDAY, DECEMBER 14, 1882

ANCIENT SCOTTISH LAKE DWELLINGS

Ancient Scottish Lake Dwellings. By Dr. Munro. (Edinburgh: David Douglas, 1882.)

THE first account of the Irish crannoges, by Sir W. R. Wilde, dates back to the year 1839, and the Lake Dwellings of Switzerland, which have shed so much light on prehistoric archæology, were discovered in the year 1853; yet the first scientific account of any similar dwellings in Scotland was in the paper by Mr. Robertson, read before the Society of Antiquaries of Scotland in the year 1857.

Dr. Munro has now collected together in an interesting and well-illustrated volume the substance of what was previously known, and added to it an account of some interesting investigations of his own, in a general work on the ancient Scottish Lake Dwellings or Crannoges. The number of these more or less artificial islands now known is very considerable. In Wigtownshire we are told that all the lakes were once literally studded with these island habitations.

So far as can be judged at present there is no reason to refer the Scottish Crannoges to so early a period as the earlier Swiss Lake villages. The objects of stone are comparatively few, while those of bone, horn, and of wood are very numerous. None of the animal remains belong to any extinct species. The horns are mostly those of the red-deer; in one case indeed, that of the crannoge at Lochlee, some fragments have been found which may possibly have belonged to the reindeer. This however seems very doubtful. The late Dr. Rolleston, by whom the remains were examined, says of the fragmentary pieces of horn: "I incline to set them down as indicating the presence of the former animal" (the reindeer). "It is usually easy," he says, "to separate even the fragments" (of reindeer's horn), "if the fragment is fresh. Of course the surfaces of the horns in these two horns" (*i.e.* of the reindeer and of the red-deer) "are different, but here the two fragmentary horns have no brow antler left and their surfaces have been macerated so long as to be desquamated." It is obvious therefore that Dr. Rolleston felt considerable doubt on the subject.

The majority of the metallic objects are of iron, but some few gold ornaments have also been discovered, and in one case, viz. at Buston, a single coin which, curiously enough, is a forgery. It consists of two thin plates of gold fastened together by some resinous substance. It belongs to a class which has hitherto been found almost exclusively in England, and is probably Saxon, belonging most likely to the sixth or seventh century. A very similar coin has been found near Dover.

We will now leave Dr. Munro to speak for himself:

"The great and primary object," he justly observes, "of the island builder was the protection afforded by the surrounding lake or morass, the securing of which has continued to be a ruling principle in the erection of defensive works down to the Middle Ages, long after the wooden islands ceased to be constructed. The transition from an island fort to the massive mediæval castle, with its moat and drawbridge, is but another step in the progressive march of civilisation" (p. 243).

The objects discovered in the Scottish Crannoges are in the main of a domestic character:

"Indeed," says Dr. Munro, "amongst the relics military remains are only feebly represented by a few iron daggers and spearheads, one or two doubtful arrow-points, and a quantity of so-called pebbles and so-called sling-stones. On the other hand, a very large percentage of the articles consists of querns, hammer-stones, polishers, flint-flakes, and scrapers, stone and clay spindle-whorls, pins, needles, and bodkins, knife-handles of red-deer horn, together with many other implements of the same material, bowls, ladles, and other vessels of wood, some of which were turned on the lathe; knives, axes, saws, hammers, chisels, and gauges of iron; several crucibles, lumps of iron slag, and other remains of metals, &c. From all these, not to mention the great variety of armaments, there can be no ambiguity as to the testimony they afford of the peaceful prosecution of various arts and industries by the lake-dwellers" (p. 282).

As regards the mode of life of the Scotch Lake-dwellers, we can, he continues:

"From the respective reports of Professors Owen, Rolleston, and Cleland, on a selection of osseous remains taken from the lake-dwellings at Dowalton, Lochlee, and Buston (see pp. 50, 139-143, 236-239), we can form a fair idea of the food of the occupiers. The Celtic short-horn (*Bos longifrons*), the so-called goat-horned sheep (*Ovis aries*, var. *brachyura*), and a domestic breed of pigs were largely consumed. The horse was only scantily used. The number of bones and horns of the red-deer and roe-buck showed that venison was by no means a rare addition to the list of their dietary. Among birds only the goose has been identified, but this is no criterion of the extent of their encroachment on the feathered tribe, as only the larger bones were collected and reported upon. To this bill of fare the occupiers of Lochspouts Crannog, being comparatively near the sea, added several kinds of shell-fish. In all the lake dwellings that have come under my own observation, the broken shells of hazel nuts were in profuse abundance'" (p. 283).

It is an interesting fact that the Lake-dwellings as yet discovered are by no means evenly distributed throughout Scotland.

"Though we cannot argue definitely from the present geographical distribution of the Scottish Lake Dwellings, the indications are so clearly suggestive of their having been peculiar to those districts formerly occupied by Celtic races, that the significance of this generalisation cannot be overlooked. Thus, adopting Skene's division of the four kingdoms into which Scotland was ultimately divided by the contending nationalities of Picts, Scots, Angles, and Strathclyde Britons, after the final withdrawal of the Romans, we see that of all the Crannoges proper none have been found within the territories of the Angles; ten and six are respectively within the confines of the Picts and Scots, while no less than twenty-eight are situated in the Scottish portion of the ancient kingdom of Strathclyde. Nor is this generalisation much affected by an extension of the list, so as to include those stony islets so frequently met with in the Highland lakes. On the other hand, that they have not been found in the south-eastern part of Scotland may suggest the theory that these districts had been occupied by the Angles before Celtic civilisation—or rather the warlike necessities of the times—gave birth to the island dwellings. In that case we would suppose that their development dates back to the unsettled events which immediately followed the withdrawal of the Roman soldiers, to whose protection the Romano-British population in the south-west of Scotland had been so long accustomed" (pp. 248 and 249).

In support of this view he also remarks that

"Fragments of Samian ware, bronze dishes (one with Roman letters), harp-shaped fibulæ of peculiar type, together with a large assortment of beads, bronze and bone pins, bone combs, jet ornaments, &c., are so similar to the class of remains found on the excavated sites of Romano-British towns, that there can hardly be any doubt that Roman civilisation had come in contact with the lake-dwellers and partially moulded their habits. The Celtic element is, however, strongly developed, not only in the general character of many of the industrial implements of stone, bone, and iron, but also in the style of art manifested in some of the ornamental objects included in the collection."

We confess that we are disposed to doubt whether the geographical distribution of the Scottish lake dwellings at present known is really connected with that of the ancient Celt, and whether it is not more due to the activity of the Ayrshire and Wigtonshire Archæological Association, of Mr. Cochran Patrick, M.P., and of Dr. Munro himself. Whilst thanking him for what he has already accomplished, we may express a hope that he will continue his researches.

JOHN LUBBOCK

OUR BOOK SHELF

The Sportsman's Handbook to Practical Collecting, Preserving, and Artistic Setting up of Trophies and Specimens. To which is added a Synoptical Guide to the Hunting Grounds of the World. By Rowland Ward, F.Z.S. Second Edition. With numerous additional Illustrations. (London: the Author, and Simpkin, Marshall, and Co., 1882).

THIS very useful little book affords all requisite information for the traveller who wishes to preserve specimens of natural history, more especially large animals. The process of skinning quadrupeds and birds is so well explained, and so copiously illustrated by characteristic woodcuts, that the merest tyro would soon learn the art. The best modes of preserving reptiles, fishes, and insects are also given; and then follow instructions for the setting up of trophies, for mounting birds and fishes, and for dressing skins of large animals. A sketch of the chief hunting-fields of the world concludes the book, and in this part much useful information is given as to the more important animals characteristic of each region.

The book is especially valuable in that it does not confuse the reader by a multiplicity of details, or leave him to choose between a variety of methods. The simplest and most effective appliances are alone recommended, and the great experience of the writer in the preservation and mounting of animals renders his advice on these points of the greatest value. The introductory chapter gives good outlines of the bodies and skeletons of the chief types of large mammalia, with the vital spots marked on each, so as to guide the sportsman in killing his game.

We only notice a single point which appears to call for correction in a future edition. The use of the blow-pipe is recommended for killing small birds, and it is described as a tube of metal or wood about 3 feet long and $\frac{3}{4}$-inch in diameter, through which pellets of clay may be propelled by the breath. Such an instrument would be of very little use, and we doubt whether any ordinary person could propel a ball of clay of this size with sufficient velocity to kill any bird at ten yards off. For using clay pellets, the bore should not exceed $\frac{3}{8}$, or at utmost, $\frac{1}{2}$-inch diameter, and the length had better be 6 or 8 feet than 3. The blow-pipes used in South America are usually 8 or 10 feet long, and under $\frac{1}{2}$-inch bore, and with these, light arrows can be propelled so as to kill birds on lofty trees, while with clay pellets, humming-birds are easily killed at more moderate distances.

The book is strongly and tastefully bound, and should be the companion of every sportsman and naturalist about to visit foreign countries.

A. R. W.

Diagrams of Insects Injurious to Farm Crops; suitable for Elementary Schools. Prepared by Miss E. A. Ormerod, Honorary Consulting Entomologist to the Royal Agricultural Society of England.

A SERIES of six large diagrams as follows, issued by the Society:—Large White Cabbage Butterfly, Turnip Fly, or Flea (the prospectus writes "Flee") Beetle, Beet Fly, Wire Worm and Click Beetle, Hop Aphis or Green Fly, with Ladybird, Daddy Long-legs or Crane-fly. These diagrams seem admirably adopted for the purpose intended, and are accompanied by short explanations, including remedial prescriptions. In her scientific names Miss Ormerod púts the cart before the horse, by reversing the order of things, and making the lesser include the greater. When she indicates *Agriotes* (*Elater*) *lineatus*, *Phyllotreta* (*Haltica*) *nemorum*, and *Phorodon* (*Aphis*) *humuli*, she means that the parenthetical generic term is the larger and older, and that the preceding one is a later creation by those dreadful specialists; but she does not say so.

Manuel d'Électrometrie Industrielle. Par R. V. Picou (Paris: G. Masson, 1882.)

THIS is one of the many books which owe their appearance to the recent rapid growth of the electrical industries; and may not be inappropriately termed a treatise on electric measurements, only a very small section being however devoted to electro-chemical quantities. The work begins with the ordinary laboratory processes of testing resistance, electromotive force, and strength of currents, &c. The latter half of the book deals with the practical application of such tests to the measurement of the electromotive force, resistance, &c., of batteries and dynamo-electric generators. Under the latter head the methods of MM. Pollard and Cabanellas are expounded. The author does not appear to be acquainted with the recent testing instruments invented by Professors Ayrton and Perry, nor those of Sir W. Thomson, which present many advantages over the instruments described by the author. There are several glaring defects in the work of too important a nature to be passed over. The author gives instructions for making up resistance coils without saying a word about the necessity of winding them so as to avoid self-induction, and as he cautions the reader to use a simple key in testing with Wheatstone's bridge he cannot be aware of the substantial reasons which exist for the use of the British Association key with double-successive contact. As the author professes to follow the British Association in its system of units he ought not to write "dyne = gramme-masse," because that is exactly what the dyne is not. He ought also to know that the statement he makes on p. 132 respecting the efficiency of electromotors, that the useful work is a maximum when the back-electromotive force is equal to half the electromotive force of the generator is not true, and does not refer to maximum efficiency but to maximum rate of using up power. Students will find the books on kindred subjects by Kempe and Day of much more use than the manual of M. Picou.

The Falls of Niagara and other Famous Cataracts. By G. W. Holley. Illustrations. (London: Hodder and Stoughton, 1882.)

THE bulk of this volume is devoted to Niagara, concerning which Mr. Holley has brought together a great deal of information on its history, geology, and local history and incidents, two-thirds of the space being occupied with the last section. The information seems to us in the main correct, though much of the miscellaneous matter included under local history and incidents is of trivial importance. The space devoted to the other "Cataracts" of the world is small, though most of the important ones are mentioned. The illustrations are good, and on the whole the book is interesting.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Priestley and Lavoisier

If Mr. Rodwell had anything new to tell us about Lavoisier, there would have been a sufficient motive for his writing; but I do not see what useful purpose is gained by telling us what was already known, namely, that a century ago Lavoisier rendered many important services to science; or, what was not so well known, namely, that chemistry is a French science; or, that Lavoisier was "the most generous of men," "incapable of any meanness." The real question Mr. Rodwell himself asks:—"Upon what authority does Dr. Thomson assert that Dr. Priestley informs us that he prepared the gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774?"

Mr. Rodwell quotes from Thomson's notice of Priestley; had he turned to that of Lavoisier (p. 105, vol. ii. 1831, not 1830), he would have found an answer:—"Dr Priestley discovered oxygen in August, 1774, and he informs us in his life [this ought to be "Life," *i.e.* autobiography] that in the autumn of that year he went to Paris and exhibited to Lavoisier, in his own laboratory, the mode of obtaining oxygen gas by heating red oxide of mercury in a gun-barrel, and the properties by which this gas is distinguished; indeed, the very properties which Lavoisier himself enumerates in his paper. [*Mem. Acad.* 1775, pub. 1778.] There can therefore be no doubt that Lavoisier was acquainted with oxygen gas in 1774, and that he owed his knowledge of it to Dr. Priestley."

Dr. Black complained of the publication of Lavoisier's papers without any allusion whatever to what he himself had previously done on the same subject. Cavendish complained of something more than a similar neglect. The facts, as stated in Dr. George Wilson's "Life of Cavendish," are briefly these:—Blagden went to Paris in June, 1783, and informed Lavoisier of the discovery of the composition of water. Lavoisier was incredulous, expressing his opinion that the union of the two gases (O and H) would produce, not water, but an acid. Nevertheless he repeated Cavendish's experiment on a large scale; and in his account of it to the Academy on June 25, stated that the conclusion as to the compound nature of water was drawn by Laplace and himself. The charge brought against Lavoisier by Cavendish, Blagden, and Watt, was summed up by Watt to this effect, that after Lavoisier had had the theory of the composition of water explained to him, "he invented it himself."

Mr. Rodwell "utterly denies" that the acceptance of Lavoisier's doctrine was mainly due to Cavendish's discovery. A strong objection to the oxygen theory was advanced by Berthollet and others, founded on the observation that in the action of dilute acids on metals inflammable air is produced. [The inflammable air of Cavendish, in 1766, was referred not to water, but to the metals]. Whence came this element? The discovery of the composition of water answered the objection, and converted it, as Dr. Whewell remarks, into an argument in favour of the theory.

My statement that "the compound is always equal to the sum of its elements" was already known, was elicited by a remark of Lavoisier's, quoted by Mr. Rodwell:—"I am obliged in this reasoning to suppose that the weight of the bodies employed was the same after the observation as before." My statement is new to Mr. Rodwell, and he calls for references. Many of the old writers on the idea of substance acknowledged the proposition, and its best application was Wenzel's doctrine of definite and reciprocal proportions, although its full significance did not become apparent until the aeriform elements were also taken into account.

But to return to Priestley, I am bound to admit that 1744 is a mistake into which I was misled by Whewell ("His. Ind. Sci., 1857, iii. 110), who gives that date.

Priestley was presented with the Royal Society's Copley medal, as an honourable testimony to his numerous scientific discoveries, which, considering the crude state of chemistry in his time, must be regarded as admirable. He was afterwards driven from the Royal Society and from his country, his house was pillaged, and his library, manuscripts, and apparatus destroyed, and all this persecution was on account of certain opinions which happily are now widely spread. The statue at Birmingham is a less impressive tribute to his memory than the maintenance of respect for his fame; and it is with no unfriendly feeling towards Mr. Rodwell that I express an opinion that this old quarrel between Lavoisier, Priestley, and Cavendish had better be left to repose in the history of science, where it has been discussed with sufficient fulness and fairness by such writers as Thomson, Brande, Whewell, and George Wilson.

Highgate, N., December 4 C. Tomlinson

The Forth Bridge

In some remarks made in Nature, vol. xxvii. p. 101, by Mr. Charles Shaler Smith, the following passage occurs:—"The tests of the last few years show conclusively, that iron exposed to compression within its buckling limit is compacted in texture and strengthened by such use while, if subjected to continuous tension beyond two-thirds of its elastic limit, it is attenuated and weakened." As I think that the words above quoted may perhaps to a certain extent mislead those who have not themselves made experiments on the elasticity of iron and steel, and on the alteration of density which can be produced by compression or extension, I would observe:—

1. That the increase of *density* which can be produced by compressing within the buckling-limit such rods as may be employed in the construction of bridges, would certainly not account for the strengthening of iron exposed to continuous compression. I have examined carefully the alteration of density which can be effected in iron and steel wires by longitudinal extension, &c., and even in cases where the wire was strained to breakage, and the permanent extension exceeded 20 per cent., there was no diminution of density equal to 1 per cent. Of course the words "compacted texture" may not mean that the density is increased, but the idea seems to be not uncommon among engineers, that increase of strength *necessarily* implies increase of density. Though I cannot at this moment lay hands upon it, I remember reading an account of some theories advanced respecting the hardening of steel, from which it was evident that the author of these theories assumed that the hardening is attended with *increase of density*, whereas the density of steel can be more *diminished* by this process than by any mechanical means with which I am acquainted.

2. It is quite true that iron, if subjected to continuous tension beyond two-thirds of its elastic limit, is attenuated, but whether the attenuation is attended with weakening or not depends largely upon the manner in which the tension is applied. If the latter is increased by small amounts at a time, and each amount allowed to act for a few hours before any increase of stress is made, not only is there comparatively small permanent extension, but there may be an actual increase of strength as far as resistance to extension is concerned. The fact is that whether we subject iron and steel to *long-continued* compression or extension, we increase the resistance to compression and extension respectively, mainly for the same reason, namely, that we *give time* for the molecules of the metal to take up such positions as will enable them to offer the maximum resistance. Thus I have proved that the value of "Young's Modulus" is considerably increased in the case of an iron wire which has suffered permanent extension, by allowing the wire to rest for some hours either loaded or unloaded; this increase of elasticity is not attended with any appreciable increase of density.

As I feel that too much precaution cannot be taken in a question of this kind, where life is at stake, I would venture to make the following suggestion:—That bars or rods of steel and iron which run the *slightest* risk of having at any time to undergo a considerable extending or compressing stress should before use be subjected, if possible, to the same kind of stress gradually-increased in amount with intervals of some hours between each increase until a stress equal to at least three-fourths of the breaking-stress be reached. Three or four days would suffice to bring the metal to its maximum strength, both as regards resistance to permanent and to temporary strain.

Should any one be interested in the subject, I would refer them to the experiments of Mr. J. T. Bottomley (*Proc. R. Soc.*, No. 197, 1879), of Prof. Ewing (*Proc. R. Soc.*, 1880, June 10), and of myself ("Influence of Stress and Strain on the Action of Physical Forces, *Phil. Trans.*, 1882, second volume).

HERBERT TOMLINSON

King's College, Strand, December 4

Intra-Mercurial Planets—Prof. Stewart's 24·011d. Period, Leverrier's and Gaillot's 24·25d., and Leverrier's 33·0225d. Sidereal Periods Considered

As your regular monthly numbers did not reach our Free Library from September, 1881, until comparatively recently, and I was absent from home when they did arrive, it was only quite lately that I had an opportunity of seeing Prof. Balfour Stewart's very interesting paper "On the Possibility of Intra-Mercurial Planets," read at last year's meeting of the British Association, and published at length in your issue of September 15, 1881. "The possibility" has been almost an admitted fact for over a century, but Prof. Stewart's valuable paper discusses the relation of certain sun-spot periods to a probable sidereal period, approximately at least, of an intra-Mercurial planet of 24·011 days.

On looking through your subsequent numbers, I was rather surprised that so suggestive a paper had not elicited quite a discussion, although it is true that Prof. Stewart remarked that "the test was not yet complete," and many may have waited to see the final results, which have not yet appeared, but perhaps will be forthcoming at the next meeting of the British Association. But the first point that struck me, although not referred to by Prof. Stewart, was the near approximation periods of 24·011 days affords to Leverrier's and Gaillot's period of 24·25 days noticed in your columns of August 22, 1878, which M. Gaillot endeavoured to fit to Prof. Watson's observation, in Wyoming State, of a supposed intra-Mercurial planet at 2° 9′ from the Sun, during *the total eclipse*, July 29, 1878. M. Gaillot's difficulty seemed to be to reconcile Leverrier's formula with Prof. Watson's reasonable belief that he saw the planet in the superior part of the orbit, while Gaillot made the formula and interval require it to be in the inferior part of the orbit July 29, 1878. The only interval that Gaillot referred to was from 1750 (January 1, I presume); it might have been obvious, therefore, that quite a small fractional difference in each of so many revolutions would suffice to make the period accord with either condition that Prof. Watson's observation required; namely, *that the planet was seen at 2° 9′ from superior conjunction*, or 2° 9′ *past inferior conjunction*. For instance, I have obtained these two result for the synodical periods. The same interval for both about 46962½ days, requiring $1808\frac{11}{24}$ revolutions of 25·96825104d., each $\frac{11}{24}$ being equal to 11·90211506d.; that accords very closely with Prof. Watson's belief, while $1808\frac{1}{24}$ revolutions of 25·9742355d. each, and 1·08226d. remainder, meet the condition of its being 2° 9′ past the inferior conjunction. Of course, as a matter of opinion, I presume it would be impossible to see the planet so near its inferior conjunction during any total eclipse of the Sun, the planet's crescent being altogether too fine. These results are simply what the conditions require in relation to approximate 26-day apparent-periods, but we must avoid exactly 26 days, or the interval would put the planet at *its elongation*, perhaps apparently 10° from the Sun, and if we tried the Lescarbault interval, from March 26, 1859; 7065½ days, singularly enough it would put the planet *in the other elongation*. Fractional differences are of course very important therefore. And I do not find either that M. Gaillot's figures 24·25d. for the sidereal time, and 14°·8462 for the diurnal motion exactly accord, and neither fills the conditions required by Prof. Watson's observation, if I am approximately correct, which I think I am. For instance, 14°·8462 diurnal motion, gives us 24·24862928d. for the sidereal periods, not 24·25d., and the synodical period would be 25·9729903466d., and the planet's position would be about 46° 12′ in its orbit past inferior conjunction, or apparently about 8°·24′ from the Sun, and 46° 12′ would be about 3⅓ days.

The sidereal period of 24·25 days, makes the diurnal motion 14°·8453608247, and puts the planet at about 6°·8′ past inferior conjunction, or apparently less than 1°. The synodical revolutions would be 25·974562624d. and fractional remainder 0·88945, or 11h. 46m. 43s., which of course would be too close to the sun. But the sidereal period and the diurnal motion should both agree, instead of producing such a difference as I have here indicated, of nearly 40° in the revolutions. But although I believe we cannot accept the exact published figures, 24·25d., or 14°·8462, still I have shown how near we may make the final results conform to them.

Adopting the same number of synodical revolutions, and practically making the best use of the formula, obtaining $1808\frac{11}{24}$ revolutions, and $1808\frac{1}{24}$. The revolution being 25·96825104d., or 25·9742355d., and the remainders 11·90211506d., or 1·08226. Reduced to clock time they stand as follows: $1808\frac{11}{24}$ being equal to 25d. 23h. 14m. 16·9s. each, and 11d. 23h. 39m. 2·7s. remainder, and $1808\frac{1}{24}$ being equal to 25d. 23h. 22m. 54s. each, and 1d. 1h. 58m. 27¼s. remainder. *The latter is almost absolutely identical* with the periods that would fit the Fritsch and Stark interval from October 10, 1802, to October 9, 1819, 6208 days, or 239 periods of 25d. 23h. 23m. 51s. And from Stark to Lescarbault makes 14,413 days, which would require 555 periods of 25d. 23h. 19m. 53½s., *which affords almost exact identity* with the general mean placing Prof. Watson's observation in the superior part of the orbit. Thus, then, we have almost positive assurance that Fritsch, Stark, Lescarbault, and Prof. Watson's planet were identical, and that Prof. Watson was correct about it being 2° 9′ from superior conjunction: these interesting facts, giving a record to Lescarbault's planet of 80 years from Fritsch's observance October 10, 1802, to October next. What other "myths" will stand such satisfactory results? I am afraid that Prof. Proctor and some other astronomers have not given the attention to this question that it deserves. But there are a few exceptions deserving credit: M. de la Baume, in Paris, was engaged last year in a classification of reported observed transits, although he did not then draw any inferences respecting apparent revolutions. He regarded Fritsch, De Cuppe, Lescarbault, and Lummis' transits as the same planets, agreeing relatively with the nodes. While Lichtenburg, November 19, 1762, Hoffman, about May 10, 1764, Scott, June 28, 1847, Ritter and Schmidt, June 11, 1855, and W. G. Wright, of San Bernardino, California, October 24, 1876, whose transit was illustrated in the *Scientific American* of November 18, 1876, he regarded as another larger planet than Lescarbault's.

Adopting the same principle with Prof. Stewart's hypothetical sidereal periods of 24·011d., I first find what results that gives, as applied to the same interval from January 1, 1750, and then take the nearest modification I can to the conditions of Prof. Watson's observation. 360° divided by 24·011d. gives us 14°·99312818 for the planet's diurnal motion, which, multiplied by 46,962½ days, gives us 704114°·78215325, from which, subtracting the earth's motion, 46288°·463941, leaves a residue of 657826°·318213, which, divided by 360° gives us 1827·29533 synodical revolutions; using that to divide the 46962½ days, we obtain the synodical periods of 25·700553d. The fractional revolution ·29533 is equal to 106° 19′ 17″, or 7d. 14h. 10m. Now while that would put the planet in the superior part of the orbit, it would still be nearly 60° from where Prof. Watson observed it. I ought to have explained before that 2° 9′, or 2° 10′ apparently, is about equivalent to 15° from *superior* conjunction, or, 15° past *inferior* conjunction in the planet's orbit; 15° from 180°, therefore, leaves 165° as the required position, instead of 106° 19′ 17″. Perhaps I am only approximately correct, but sufficient for illustration. It is very evident, however, that a very slight modification of Prof. Stewart's inferential sidereal periods, 24·011d., would give us the 60° more required, or exact accord with Prof. Watson's observation, and the evidence would be rather in favour of $1827\frac{11}{24}$ revolutions, obtained from such a solar analogy, and may still have an incidental bearing or relation to the Leverrier-Gaillot formula I have construed to require $1808\frac{11}{24}$ or $1808\frac{1}{24}$ revolutions. $1827\frac{11}{24}$ revolutions would give us 25·698260334253d. for the synodical periods, and a remainder of 11·7783693198 6d. Reduced to clock time, that would give us 25d. 16h. 45m. 29·7s. for each apparent revolution, and 11d. 18h. 40m. 51·11s. for the remainder. It must be understood that these definitions, $1808\frac{11}{24}$ and $1827\frac{11}{24}$, with their results, are intended only to express possible general mean periods of apparent revolutions, and may not exactly apply to any of the intervals between the long list of recorded observations of supposed transits. When Leverrier, October 1876, had strong faith in sidereal periods of 33·0225d., it was probably a general mean from January 1, 1750, to Lescarbault, March 26, 1859, and only approximately fitted Lummis, De Cuppis, Stark, Fritsch, and others, but still in a general sense applied to some of them, while Leverrier was led to predicate a transit, March 22, 1877, which probably did not occur. And yet, viewed in connection with a reported transit, October 24, 1876, seen by Mr. W. G. Wright at San Bernardino, California, and illustrated and fully reported in the *Scientific American* of November 18, 1876, which circumstance Leverrier probably had no knowledge of, was by no means as unsatisfactory as the public imagined; for practically *Wright's transit and Leverrier's hypothetical period mutually confirmed each other.* The *Scientific American* Supplement of August 27, 1881, published some remarks I sent them, which may have reached England. One point I directed attention to was that Leverrier indicated that a conjunction was due September 21, 1876, and I found that there were thirty-three days between that and October 24, 1876, so if Leverrier took 176 synodical periods from Lescarbault to September 21, it was nearly the same thing to take 177 to October 24; but extending the interval to January 1, 1750, there would be much nearer similarity in the synodical periods to accord with Wright's transit, October 24, 1876. I also noticed that the ratio of displacement of the node from Lescarbault and Lummis was 7 days retrograde in 3 years' advance, and on that data, applied to Wright's transit, another transit would be due $11\frac{2}{3}$ days earlier in 1881, while Leverrier, in October, 1876, remarked that *for a transit at this node we must wait till about* 1881." My computation made it fall due, therefore, October 12 or 13, 1881, and I was anxious that it might be looked for. The computation made the Hawaiian Islands the most favourable place; but although I believe it was not seen there, nor was it observed from Sacramento or Salt Lake City, where Mr. W. R. Frink looked for it with a 4-inch aperture achromatic telescope, we have no evidence to show whether it might not have occurred in Europe or elsewhere, and been noticed if it had been looked for.

Sacramento, California A. F. GODDARD

[The subject of this communication is a very interesting one, as relating to the possibility of changes on the sun's surface being due in some way to the positions of the various planets of the system. But before this relation can be considered as established, it will be necessary to increase the accuracy of our solar information by collecting our past observations, as well as by securing a set of daily observations for the future.—ED.]

An Extraordinary Meteor

I BEG to send you the following, in case you consider it worth inserting:—At about 1.10 a.m. on the night between November 18 and 19, whilst going in the s.s. *Bokhara* in the Red Sea, about midway from Aden to Suez, the quarter-master on duty called me, saying he had just seen a new comet, or shooting star, which was still visible many minutes after its first appearance. He said that whilst he was looking out ahead, or in a northerly direction, he suddenly noticed the effect of a bright light shining from astern, and on turning round saw a very bright shooting star still moving from left to right, and slightly downwards, in the south, at an altitude of about 40°. The star speedily disappeared, but left a bright train of light behind it, which continued so long (from five to ten minutes he guessed) that he thought I might like to see it. I came on deck a little before a quarter past one by the ship's clock, and found a streak of light which I estimated as 8° or 10° in length, and rather less than half a degree in width, apparently stationary, midway between Sirius and Canopus, and nearly as bright as *the* comet, the head of which must have risen half an hour or more previously. I watched the streak till half-past one o'clock, when it seemed sensibly fainter, though still a conspicuous object, notwithstanding the presence of the moon, the comet, and a number of bright stars. Whilst watching I noticed two small meteors shooting from left to right across the southern sky, which struck me as probably belonging to the same group as the large one whose train I was watching.

At half-past one o'clock I went below, and did not return on deck till 5 o'clock, when the apparition had disappeared. The quarter-master told me afterwards that it had faded away soon after I left the deck, but he believed that from first to last it had remained conspicuously bright for more than half an hour.

Clewer, December 6 B. R. BRANFILL

British Rainfall

I AM just preparing to issue to all the observers of rainfall known to me, blank forms for the entry of their records for the year shortly about to close. This staff now exceeds 2000, but still as they are not unfrequently rather clustered there are many parts of the country where additional records are needed. I have no doubt that records are already kept in many places unknown to me, and I shall be glad if you will allow me to invite communications from any one who has kept an accurate record, and to supply either those already observing or contemplating doing so with a copy of the rules adopted by British observers, and with all necessary blank forms—all, I may perhaps as well add, free of charge, as our greatest requirements are *ample* and *accurate* records. G. J. SYMONS

62, Camden Square, London, N.W.

Swan Lamp Spectrum and the Aurora

IN NATURE, vol. xxv. p. 347, is a description of the spectrum of carbon as found by Professors Liveing and Dewar in a Swan lamp rendered incandescent in the ordinary way. Finding one of these lamps only feebly lighted by ten pint Grove cells, it occurred to me to test it by the secondary current. The coil was nominally a 6-inch spark one, but little battery power was used, and the spark considerably reduced. One wire was connected with the filament holders, the one made into a little coil and laid on the top of the lamp.

The first effect was a fine silver glow filling the lamp, and showing Plücker tube changes when the circuit was reversed. This gave a carbon spectrum of bright lines. Soon, however, the colour of the discharge changed to pink, and the carbon spectrum gave way to a nitrogen banded one. A yellow spark had been noticed where the wire lay on the top of the lamp, and it was evident air had found its way into it.

At one point perforation had taken place by a single spark, while near this the glass was pounded into a sponge-like mass by a series of these.

The sodium lines due to disintegration of the glass were observed in the spark and glow. I was much struck by the rapidity with which, as what was probably only a small quantity of air found its way into the lamp, the nitrogen-spectrum swept away and took the place of the carbon one, a matter which seems to present another difficulty to the favourite theory which makes the aurora, with its bright, sharp unrecognized lines, an electric discharge in rarified air. J. RAND CAPRON

Guildown, November 30

The Aurora

ALREADY we have for the height of the "auroral beam" the varying estimates of 44, 170, 200, and 212 miles, and assuming the correctness of any one of the three last figures, we seem drifting from the improbable to the impossible, for are we not told by Messrs. De la Rue and Müller (NATURE, vol. xxii. p. 24) that while at 81·47 miles' height, the discharge is "pale and faint, at 124·15" *no discharge could pass?* Lest this addition to the aurora's mysteries be for want of definite particulars in the observations, I add mine as nearly as I can—Time 6h. G.M.T. and a few minutes? Altitude of moon above horizon 28°. Distance from moon's centre to centre of beam as it floated *above* 2°, direction east to west (nearly). Lat. 51° 13′ 46″ N., Long. 0° 28′ 47″ W. (observatory).

Guildown, Guildford, December 11 J. RAND CAPRON

Fertilisation of the Speedwell

IF Mr. Stapley, who wrote on this subject in last week's NATURE, can refer to Dr. H. Müller's treatise on the relations between flowers and insects, in the first volume of Shenk's *Handbuch der Botanik* (now publishing as part of Trewendt's *Encyclopædie der Naturwissenschaften*), he will see that his own observations are very similar to those of Dr. Müller. The latter, however, refers to and figures the Germander, not the Common, Speedwell. Is it possible that Mr. Stapley—who speaks of the *Veronica officinalis* as having larger flowers than the *V. hederæfolia*, whereas they have flowers of about the same size—mistakes the *V. chamædrys* for the *V. officinalis?*

The insects which Dr. Müller found bending down the stamens, as Mr. Stapley describes, were small Diptera chiefly of the genera *Ascia* and *Melanostoma*. He mentions this also in *Kosmos*, iii. p. 497, and a few pages earlier (*ib.* p. 493) he gives a large drawing of *V. urticæfolia*.

I have not Darwin's book at hand while I write, but does he not mention the Germander Speedwell?

Bedford, December 9 ARTHUR RANSOM

Shadows after Sunset

FIVE years ago my attention was attracted to the phenomenon now under discussion. I was then at San Fernando, and could perceive almost every evening the rosy and blue or black and white rays converging to a point apparently below the horizon. I was able to trace the rays from west to east many times, and frequently also to trace the black or blue spaces to visible prominences in the cumuli in the western horizon, to whose shadows there is no doubt the rays were due, as they swept the sky with such a rapidity; and they were so persistently traceable to the bright bordered cumuli, that even though there were any hills in the direction of the setting sun (which there were not), the phenomenon could not be attributed to them. Besides, I have observed it when off the coast of Portugal, which leaves the hill shadows out of the question, as the observations were made in the (two consecutive) evenings. Though the sky is too cloudy in this part of Spain, by looking at the right place at the right time I have been able to see it many times. The mock sun described by Mr. Rand Capron in the last number of NATURE (p. 102) was seen once by me, but the phenomenon was but little conspicuous. The rays are seldom equidistant.

Naval School, Ferrol, Spain, December 5 PROF. DIER

Complementary Colours

IN connection with recent correspondence in NATURE it may be worthy of remark that I have often noticed the appearance of strong complementary colours in water from contrast-effects, in the case of a wave, breaking on the shore. If the water is properly illuminated so as to be of a decidedly green tinge, the crest of the wave often appears of a delicate pink, and this even in strong sunlight. The purplish hue of cloud-shadows on the ocean is also a familiar example of the phenomenon under discussion. CHAS. R. CROSS

Mass. Institute of Technology, Boston, November 23

An Extraordinary Lunar Halo

I PURCHASED a copy of NATURE of November 30 in the hopes of finding some account of a lunar halo observed by myself and several friends on Saturday night, November 18, about 10.30. Instead of which I find that Mr. Barkas has sent you an account of one seen by him on the following Monday. His description tallies almost exactly with the one seen by me, with the exception of date and hour. Can any of your readers give any information respecting them? S. A. GOOD

751, Wandsworth Road, S.W.

"Lepidoptera of Ceylon"

ALLOW us to correct a slight error in the address of the President of the Royal Society, as reported in NATURE last week. He speaks of the "*completion*" of the "Lepidoptera of Ceylon" having been presented to the Library, whereas it is only the first of the three volumes of that work which is as yet complete. Part vi., being the second part of the second volume, will be ready next week, and the succeeding parts will follow in due course. The error is not very important, but might mislead subscribers and others interested in the work.

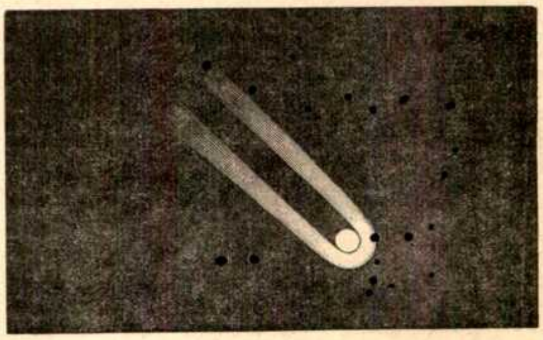
L. REEVE AND CO.

5, Henrietta Street, Covent Garden, London, W.C.,
December 11

THE COMET

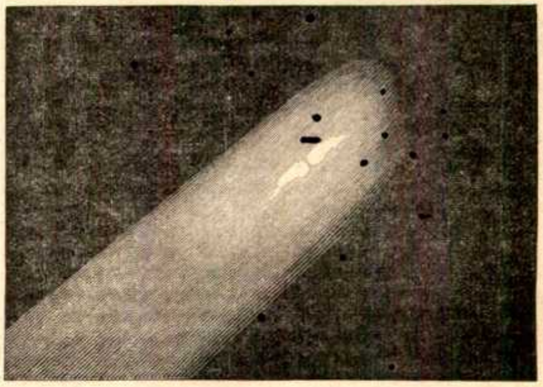
WHEN the comet was first seen on September 16 at 22h. 45m. its appearance was most symmetrical, in colour a most intense white. The sketch shows the appearance on such a scale that the nucleus would have a diameter of about 45", by a comparison made at the time with a sun-spot, the exact size of which has since been kindly furnished by the Astronomer Royal from the Greenwich photographs of the sun. The direction of the comet was to the centre of the sun, as far as could be estimated. On p. 81 of this volume there is a diagram of the sun and the comet; the size of the comet as there given compared with the sun is about as it appeared; and if one imagines the sketch I give, reduced to the length of the sign for the comet on the diagram, and placed some two diameters of the sun to the south-west and *radial*, he will have a good idea of the appearance on the morning of September 17.

The general appearance of the comet has been so fully described that I will confine myself to some points that I have observed with the three-foot reflector, which I did not get to bear, however, till October 29 at 16h. 40m.



September 16, 22h. 45m.

Although the moon was very bright the comet was well seen, the nucleus appearing as an oval bright spot fading into the head gradually (this is called the nucleus in my note-book, but subsequent observations show it ought to be called the bright part of the head). The most noticeable feature of this morning's observation is the peculiar termination to the head; at the n.f. side of head (see sketch for October 30), there was noted an absence of light, while the extension on the south side was particularly noticed, there may have been some extension on the corresponding north side, but I have not recorded it, if so the appearance would then be similar to that in sketch No. 2 (NATURE, vol. xxvii. p. 109). This oblique termination appears in all my sketches made at the telescope. The length of this nucleus or bright part of head was measured as 55". An absence of stripes in the tail was particularly noticed, if there was a difference the south side was a little the brightest.



October 30, 16h. 50m.

On this morning the brightness of the moonlight had a marked effect on the visibility of the broader part of the tail, so much so that it was easier to trace it in the sky with the naked eye, than with either a binocular, a 3-inch achromatic, or a 3-foot reflector.

The following morning, October 30, 16h. 50m., the appearance of the comet was so altered that either a remarkable change had taken place, or the details had not been properly made out on the previous morning—the head had become brighter, narrower, and longer, with a decided nucleus, situated a little less than half way from the following end; further examination showed a break in the line of light forming the head, a comparatively dark space splitting it in two, the nucleus being on the border of this space, while a brightening of the head near the other side of this space gave an appearance of another nucleus. The sketch for this date shows the position of these brighter parts or nuclei, and the space between.

A careful measurement at 17h. 41m. gives the pos. angle of the line of light forming head as 115° 5′. The distance of the nuclei was 11″·5, and the width of the head 10″. On November 1, 17h. 18m., the pos. angle of head was 117° 5′, the breadth of the head 11″, and the length 100″, the general appearance being as that given for October 30, excepting that on this morning a brightening is recorded as observed at the extremity of the *following* part of the head, giving a tri-nuclear appearance. Subsequent observations made at intervals (on November 5, 8, 9, 10, and 17) show little deviation from the last sketch. Particular attention has not since been given to eye-observations, as the 3-foot has been used on these dates for photography (with doubtful advantage). The brightness of this comet is, as far as can be judged from a comparison of similar exposures, about the same as the great comet of 1881. One minute gives an image faint, but certain, about 25 minutes' exposure gives an intense image of the head and a trace of the tail; but the result is not at present worth the great trouble it causes. One of the November 9 plates shows the dark space in the head, and this is all that can be said for it; longer exposures without a proper means of following the motion of the comet give only a trail. This however I propose to get over by having motions adapted to the plate-holder and an eyepiece attached to the holder for the purpose of running a second image of the comet, taken out of the cone of rays from the speculum by another diagonal mirror properly placed for the purpose. A. AINSLIE COMMON

Ealing, December 4

IN a clear sky at 4.50 a.m. November 26, not a vestige of the comet was to be seen by the naked eye, though its position was known exactly. On applying a telescope to the spot the nucleus appeared as a round nebula, with four small stars near it; as for the comet's tail, I presume it was "left behind," for no trace of it could be discerned, except by the eyes of the imagination. This is singularly corroborative of the statement that has appeared in these columns, viz. that the moonlight obscures the comet, although it seems to be doubted.

Oxford, December 5 FRANK STAPLETON

ILLUSTRATIONS OF NEW OR RARE ANIMALS IN THE ZOOLOGICAL SOCIETY'S LIVING COLLECTION[1]

X.

26. THE MALAYAN TAPIR (*Tapirus indicus*).—In the present condition of zoological life on the world's surface there is no better instance of discontinuous distribution than that of the Tapirs. While Tropical America contains several species of *Tapirus*, and may be regarded as the *focus* of the genus, a single well-marked species—not, however, sufficiently distinct, even in the eyes of those most fond of inventing new names, for generic separation—occurs in Tropical Asia. This is the Malayan or Indian Tapir, *Tapirus indicus* (sive *malayanus*) of systematists.

The discovery of this Tapir in Sumatra, where it was first met with, though claimed by Cuvier for French naturalists, is undoubtedly due to those of our own country. Marsden described the animal in his work on Sumatra as long ago as 1785, and Raffles obtained a knowledge of it in 1805. In 1818 a living example, captured near Bencoolen, was sent to the menagerie at Barrackpore, and was the subject of a drawing, forwarded to Cuvier by Diard and Duvaneel, which first made the great French philosopher acquainted with the existence of this animal.

The first example of the Malayan Tapir sent to Europe likewise came to this country. It was received in September, 1820, from Sir Stamford Raffles, and was the subject of an excellent memoir by the great surgeon and anatomist, Sir Everard Home, which was published in the *Philosophical Transactions* for 1821.

The Zoological Society of London acquired their first living specimen of this animal by purchase of Capt. Miland in September, 1840. This example died on April 17 in the following year. Although one or two specimens of the Indian Tapir passed through this country at subsequent intervals, it was not until the present year that the Society succeeded in obtaining possession of a second specimen. This was a young individual of the male sex, from which our illustration (Fig. 26) was taken by Mr. Smit in August last. It will be observed that although the large white area which covers the hinder quarters like a sheet, and renders the Indian Tapir so readily distinguishable from all its American brethren, is easily distinguishable in this drawing, the stripes and spots, which prevail in the younger dress of all the Tapirs, are still quite distinct. These disappear altogether when the animal is quite adult, leaving the entire body, with exception of the white back, of a glossy brownish black. The Indian Tapir is further distinguishable from all the American species by the absence of the mane, and by the minute structure of the teeth.[1] Unfortunately the Zoological Society's second specimen did not live to exhibit its adult characters, but died in October last in consequence of a disease of the rectum, which seems often to afflict these animals in captivity.

Besides Sumatra, where the Dutch naturalist, Salomon Müller, found it on the west coast up to a height of 2000 feet above the sea-level, the Malayan Tapir inhabits the interior of Borneo and the Malay Peninsula. There is also good evidence that a Tapir of some sort is found in the south-western provinces of China, which is probably of the same species.

In its native state the Indian Tapir is exclusively an inhabitant of the forest, keeping principally to the vicinity of the rivers and treading paths by following the same routes during its excursions from the banks in search of food. In captivity it becomes very tame and familiar. Dr. Cantor gives us the following account of a young female specimen which was captured in Keddah in 1845, and lived many months at his station in Malacca:—

"From the first, although fresh from its native wilds, this young Tapir showed a remarkably gentle disposition. The daytime it spent in sleeping in a dark recess of the portico of my house, though it would rouse itself if noticed. Towards sunset it became lively, would bathe, feed, saunter abroad, and with its lengthened nose examine objects in the way. Within a few days after its arrival it commenced to exhibit a marked partiality to the society of man, not indeed to its keeper in particular, whom it scarcely had discrimination enough to distinguish, but to anybody who happened to notice or caress it. Towards sunset it would follow a servant on the green in front of the house, and punctually imitate his movements, whether standing, walking, or running. If the man suddenly hid himself, the Tapir would hasten to the spot where it had lost sight of its keeper, look about in all directions, and if unsuccessful in discovering him, express its disappointment by a peculiar loud whistling. On the reappearance of the man, it expressed its pleasure by rubbing its side against his legs, running between them, occasionally giving out a short singular sound, resembling that produced when the larger woodpeckers tap the trees, but more sonorous. When of an evening it heard the voices of people in the verandah above the portico, it exhibited strong marks of impatience till let

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[1] Continued from vol. xxvi. p. 606.

[1] *Cf.* Tomes in *Proc. Zool. Soc.*, 1851, p. 121.

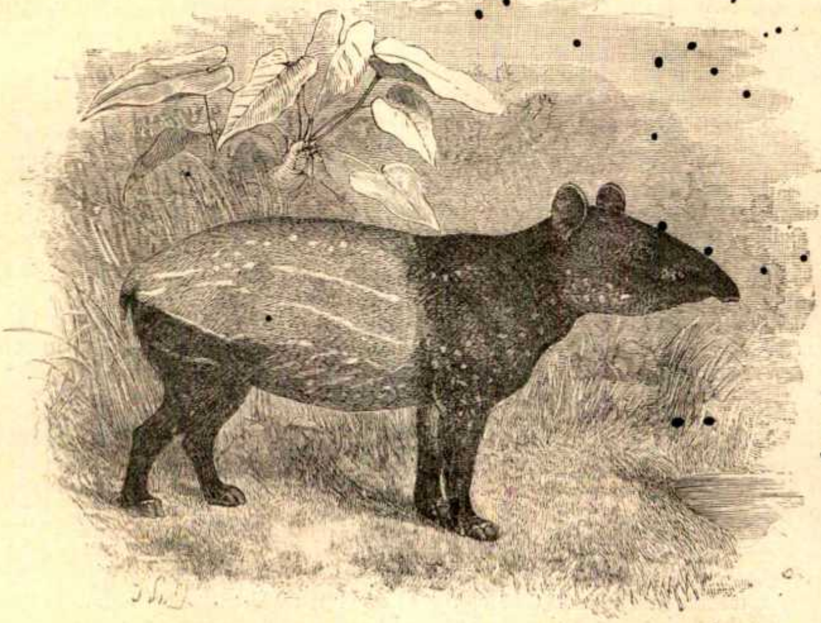

FIG. 26.—The Malayan Tapir.

loose, when of its own accord it would, awkwardly enough, ascend a flight of stairs leading to the verandah. It would then quietly lie down at their feet, and by stretching its limbs and shaking its head, express the

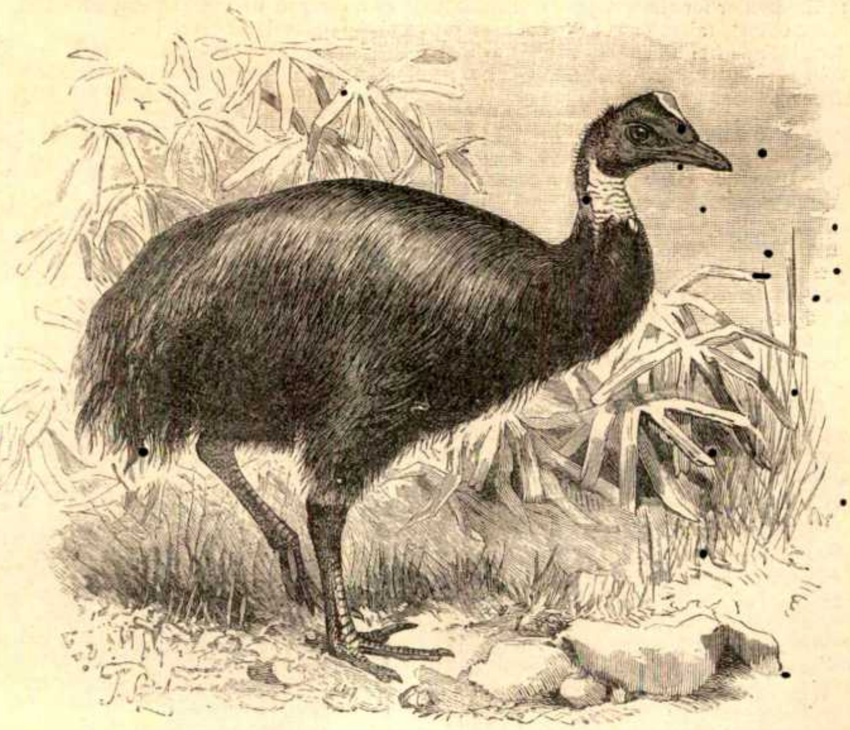

FIG. 27.—The One-wattled Cassowary.

satisfaction it derived from being caressed, and it was only by compulsion that it could be made to leave the company. Its food consisted of plantains, pine-apples mangustins, jambu, leaves of *Ficus pipul*, sugar-cane, and boiled rice, of which latter it was particularly fond, if mixed with a little salt. Its drink was water and also milk and cocoa-nut oil, which latter taste the Tapir possesses in common with the O'rang-'utan. It delighted in bathing, and was otherwise cleanly.

27. THE ONE-WATTLED CASSOWARY (*Casuarius uniappendiculatus*).—The Cassowaries of the Moluccas and Papuan Islands, together with the Emeu of Australia, constitute a very well-marked division of the Struthiones, or ostrich-like order of birds, and occupy a large area of the Australian region. But while the Emeu (*Dromæus*) is spread over the whole of the Australian continent, the Cassowary is only met with in the northern parts of Queensland and in the peninsula of Cape York, and we must cross Torres Straits into New Guinea and its adjoining islets before we arrive at the true metropolis of the Cassowaries. Here we shall find them scattered over the different islands to the number of nine, as indicated in Count Tommaso Salvadori's recent essay on the group,[1] and but one, or at most two species being ever found exactly within the same area.

A characteristic of the Cassowaries is the large horny casque which covers the head, and is devoid of feathers. In one division of the genus this is much elevated and laterally compressed, in the other the casque is pyramidal in shape, and flattened cross-ways behind. The One-wattled Cassowary belongs to the second division, and is further distinguished by having (in common with its near ally *C. occipitalis*) but a single wattle in the middle of its throat.

This Cassowary was first made known to science in 1860 by Blyth, from an example brought alive to Calcutta, of which the exact origin was uncertain. It has, however, since been ascertained that it inhabits the Island of Salawatty, and adjacent western portions of New Guinea, where the naturalists Bernstein, v. Rosenberg, D'Albertis, and Beccari, who have visited these districts, have obtained specimens. Like other members of the group, it is a forest-hunting bird, living principally on various fruits, but also occasionally indulging in such animal food as lizards, fishes, and insects.

Our figure of this Cassowary (Fig. 27) is taken from a nearly adult individual of this fine species, now living in the Zoological Society's Gardens, which was obtained by purchase in July last.

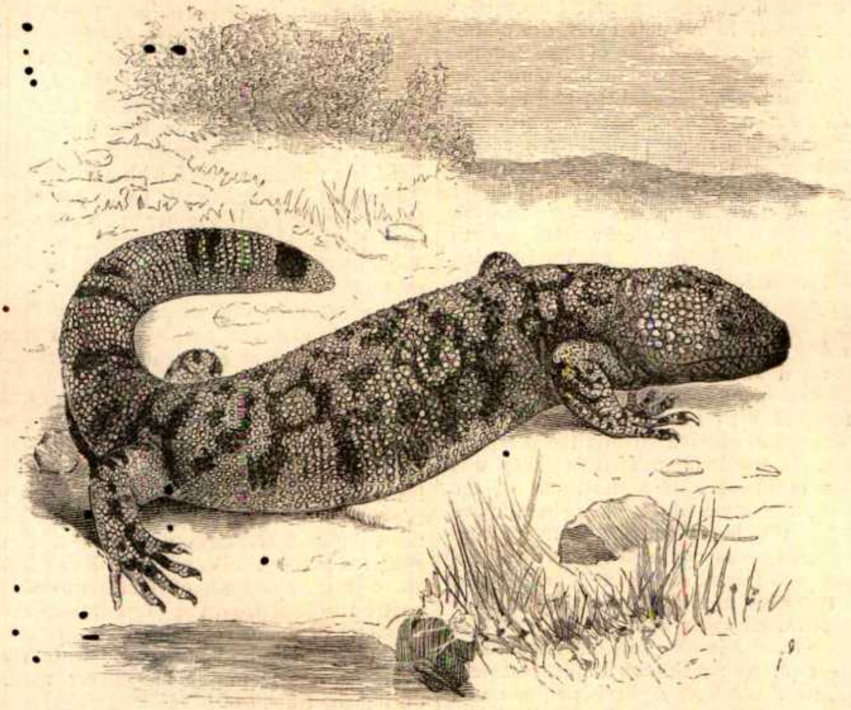

FIG. 28.—The Sonoran Heloderm.

28. THE SONORAN HELODERM (*Heloderma suspectum*).—Lizards, as a general rule, are perfectly harmless animals, whose only object when approached is to get out of sight as fast as possible. In almost every country, it is true, some sorts of lizards have a dreadful reputation amongst the ignorant. The Slowworm, in England, and the Gecko, in India, are alike reputed to be highly venomous, but naturalists well know that there is not the slightest foundation for these fancies, and that both these little creatures are, in fact, quite innocuous. It was, in fact, until within a comparatively recent period, the generally received opinion among the best authorities, that no member of the Lacertilian order was really venomous. It is only within the last few years that the evil reputation which certain lizards of Mexico and the adjoining districts of the United States have long borne among the natives of those countries, has been confirmed by an accurate examination of their teeth, and the conclusion thus forced upon us that at least one form of lizard is endowed with the faculty of producing a poisonous bite. The possesssors of these formidable weapons of defence are the members of the genus *Heloderma* of naturalists, one species of which was long ago described from Mexico under the appropriate name *Heloderma horridum*. It has been shown by MM. Dumeril and Bocourt in France, and Dr. J. G. Fischer in Germany, that this lizard has not only grooved teeth, after the manner of many of the poisonous serpents, but likewise highly developed salivary glands, which issue at the bases of the teeth with the evident purpose of carrying the poisonous saliva into the grooves. It has been likewise shown by the evidence of careful observers that the bite of the *Heloderma horridum* is fatal to small mammals and birds, and highly injurious to man, although not perhaps under ordinary circumstances capable of inflicting a fatal wound.

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[1] "Monographia del gen. Casuarius, Briss. Per Tommaso Salvadori." *Mem. R. Acc. Sc. di Torino*. Ser. ii. tom. xxxiv.

The same seems to be nearly the case with a second species of *Heloderma*, *H. suspectum* of Cope,[1] a portrait of which we give (Fig. 28) from a fine specimen recently added to the Zoological Society's collection. Experiments made with this animal have shown that it is sufficiently venomous to kill a small guinea-pig, and, as hereafter shown, there is no doubt that its bite inflicts serious injury upon any one handling it carelessly.

The Sonoran Heloderm, or "Gila Monster," as the inhabitants of Arizona call this reptile, is one of the largest lizards in North America, and is found all through New Mexico, Arizona, and Texas. It inhabits the sandy deserts of that arid land, and is said to be a wonderfully striking object as it darts about the rocks, and shows its brilliant armour of jet black and orange scales. In a recent number of the *American Naturalist*, Dr. Shufeldt gives the following account of his experiences with one of these "monsters":—

On the 18th inst., in company of Prof. Gill, of the Institution, I examined for the first time Dr. Burr's specimens of the Heloderm, then in a cage in the Herpetological Room. It was in capital health, and at first I handled it with great care, holding it in my left hand, examining special parts with my right. At the close of this examination I was about to return the fellow to his temporary quarters when my left hand slipped slightly, and the now highly indignant *Heloderma* made a dart forward, and seized my right thumb in his mouth, inflicting a severe lacerated wound, sinking the teeth in his upper maxilla to the very bone. He loosed his hold immediately, and I replaced him in his cage with far greater haste perhaps than I removed him from it. By suction with my mouth I drew out a little blood from the wound, but the bleeding soon ceased entirely, to be followed in a few moments by very severe shooting pains up my arm and down the corresponding side. The severity of these pains was so unexpected, that added to the nervous shock already experienced, and to a rapid swelling of the parts that now set in, it caused me to become so faint as to fall, and Dr. Gill's study was reached with no little difficulty. The action of the skin was greatly increased and the perspiration flowed profusely. A small quantity of whiskey was administered. This is about a fair statement of the immediate symptoms: the same night the pain allowed of no rest, although the hand was kept in ice and laudanum, but the swelling was confined to this member alone, not passing beyond the wrist. Next morning this was considerably reduced, and further reduction was assisted by the use of a lead water wash. In a few days the wound healed kindly, and in all probability will leave no scar; all other symptoms subsided without treatment beyond the wearing, for about forty-eight hours, so much of a kid glove as covered the parts involved. After the bite our specimen was dull and sluggish, simulating the torpidity of the venomous serpent after it has inflicted its deadly wound, but it soon resumed its usual action and appearance, crawling in rather an awkward manner about its cage."

The specimen of the Sonoran Heloderm in the Zoological Society's Garden's Reptile House was presented to the collection in July last by Sir John Lubbock, Bart., F.Z.S., by whom it was received from Mr. G. A. Treadwell, of the Central Arizona Mining Company, of Vulture, in Arizona territory. There was much difficulty at first experienced in getting the reptile to take food. After articles of diet of various kinds had been presented to it, and successively refused, it was found that small hen's eggs were sufficiently attractive to induce it to break its fast. Since then the Heloderm has grown less dainty, and has actually condescended to take a small rat, though it prefers eggs to any other kind of food. It may be remarked that it is difficult to conjecture of what use venom can be to an egg-eating lizard.

It may be added in conclusion that Dr. Steindachner, the well-known herpetologist of Vienna, has recently described and figured a new form of lizard from Borneo,[1] under the name *Lanthanotus borneensis*, which is nearly allied to *Heloderma*, and has similarly grooved teeth. It would be of great interest to know whether the Bornean lizard has likewise venomous qualities.

THE TRANSIT OF VENUS

A VERY fair amount of success appears from the telegrams to have attended the British expeditions for the observation of the late transit of Venus. In Jamaica Dr. Copeland and his colleague secured all four contacts; at Barbados Mr. Talmage, though he lost the first external contact, observed the other three; we have no intelligence yet from the station at Bermuda, occupied by Mr. Plummer, nor, of course, from the expedition on the west coast of Madagascar. At the Cape the observers were similarly favoured by the weather, and we hear of very successful observations in New Zealand by Colonel Tupman. The only regrettable failure was at Brisbane, whither Capt. Morris, R.E., had proceeded, with Mr. C. E. Peek. It had been at first the intention of the Committee of the Royal Society to send an expedition to the Falkland Islands, but on learning that other countries intended to occupy stations in that part of the globe, Brisbane was substituted with the view to strengthen the Australian stations, and, so to say, assist in counterbalancing the great number of observations that might be expected in the United States. At the Naval Observatory, Washington, all four contacts were observed with the principal instruments, as also at the Observatory of Haverford, near Philadelphia, and in due course we shall doubtless hear of many more American successes.

At the principal observatories in this country little or nothing was seen of the transit. Dr. Ball, so far as we are aware, was most successful at Dublin; though he did not secure either contact at 2h. 37m. Dublin time he was able to commence a series of measures of distance of the outer and inner limb of Venus from the sun's limb, which he continued to 3h. 3m. He found by calculation from the time observations that at 2h. 43m. 30s. Dublin mean time, the limb of Venus nearest to the sun's centre was 188″ from the sun's nearest limb; and also, that at 3h. 0m. 0s. the limb of Venus furthest from the sun's centre was 162″ from the adjacent limb of the sun. The diameter of Venus resulting from these observations is 64 seconds, corresponding exactly with that deduced by Prof. Auwers from his heliometer measures at Luxor, during the transit of 1874.

On comparing the times calculated from the elements of the transit which have been adopted in NATURE when referring to the phenomenon with those telegraphed to the *Times*, as having been noted by two observers at Washington, and one at Haverford, the following differences between calculation and observation are shown:—

		WASHINGTON.		HAVERFORD.
		Frisby.	Sampson.	
		s.	s.	s.
Contact	I.	−80	+16	−41
,,	II.	+13	+3	+33
,,	III.	+21	−38	−29
,,	IV.	+80	+38	+19

Mr. Neison observed the first external contact at Durban at 3h. 54m. 41s. local mean time; if we assume his longitude to have been 2h. 3m. 30s. east, the difference of the calculated time would be −15s. The view from the observatory there was almost perfect. The conditions were, cloudless sky, but the air was unsteady.

Mr. Marth, writing on November 21, places his station at Montagu Road, Cape Colony, in longitude 20° 3′ E., with 33° 20′ south latitude. At the date of his letter he was fearful that one of the whirlwinds of sand and dust of which he had had much experience might at the last moment spoil all. The heat during the day was overpowering, but at night he was in need of his winter overcoat. The transit observations, however, were very successful.

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[1] Described in. *Proc. Acad Sc. Phil.*, 1869, p 5.

[1] *Denkschr. k. Ak. Wien.*, xxxviii p. 95 (1878).

The following telegram from Mr. Talmage, chief of the British party at Barbados, was forwarded to the *Times*. by Mr. Stone :—" Internal contact at ingress, and internal and external contacts at egress were well observed." The observers were Mr. Talmage and Lieutenant Thomson, R.A. On this the *Times* remarks :—

" The observations at ingress will combine with those made at the Cape and those at egress with the Australian and New Zealand observations. An official telegram received from Captain Morris, R.E., confirms that from Mr. Peek, announcing a complete failure at Brisbane. Lieutenant Darwin, R.E., who accompanied Captain Morris, will, before returning to England, determine the difference of longitude between Point Darwin and Singapore, thus completing a connection between the Australian and English longitudes. Information has now been received from all the British stations with the exception of Madagascar and Bermuda.

" From the Paris telegram it will be seen that no news is expected from the Patagonian stations for about a week, and it is feared that the Chilian mission has failed. The Russian, Austrian, and Italian Governments have sent no parties out for observation of this transit; but the former have lent two heliometers to MM. Perrotin and Tisserand, and an equatorial to Dr. Pechule, who has been sent to St. Thomas by the Danish Government. Spain has sent two parties to the Havannah and Porto Rico; these are provided with instruments of the same class as those of the British parties.

" Many observers have noticed the phenomenon known as the 'black drop,' and in some cases a grayish light, probably similar to that seen by Winthrop in observing the transit of 1769 at Cambridge, U.S., the American observers appear to have specially looked for traces of a satellite of the planet but could see none. A very curious phenomenon was seen by Prof. Langley and others observing at the same station. When half the planet was on the sun's disc a small patch of light appeared near the limb outside the sun; it extended for about 30 deg. along the limb, and was totally distinct from the luminous ring seen surrounding the planet by Prof. Langley and several other observers at different places. Spectroscopic observations were taken at several places in the United States; the spectrum showed some strange lines, and a watery vapour was suspected in the atmosphere of the planet. With regard to the observations made by M. Janssen at Oran, no details have been received, but it would appear that they are likely to prove of considerable value, and will add to our knowledge of the physical condition of the planet.

" The phenomenon of the 'black drop' takes place at the contacts of the limb of Venus when the planet last touches the sun's edge at entry on, and first touches when about to pass off, the disc; it has been noticed by some observers at all preceding transits which have been observed, while others have noticed a brown or greyish ligament joining the limbs of the sun and planet. The atmosphere of Venus was remarked by Hirst, who observed the transit of 1761 at Madras, and subsequently by other observers. When part of the planet has entered on or has moved off the sun, a ring of light has been seen surrounding Venus; this arises from the reflection of the solar light on the atmosphere surrounding the planet. This ring of light was noticed during the transit of 1761, and has been seen at all those of 1769, 1874, and on Wednesday. The phenomenon observed by Prof. Langley has not been observed at previous transits, and is probably due to some local causes. This is the only phenomenon mentioned in the accounts received which has not been previously noticed."

Under date December 12, Mr. Stone sends the following to the *Times*:—I have received the following telegram from Mr. Plummer, at Bermuda :—" Ingress well observed. Egress observed amid clouds." The telegram probably indicates that the observations at egress are not of much value. The egress, however, appears to have been better observed than the ingress at some of the American stations, and there will be plenty of observations of accelerated egress to balance the observations which will be available of retarded egress for New Zealand and Australia. Reports have now been received from all the British stations except Madagascar, which is a most important ingress station, and from Captain Wharton, H.M.S. *Sylvia*. Captain Wharton has two good telescopes, and will have established his party somewhere on the South American coast, where he may, if the weather was favourable, have observed both the ingress and egress, but with small factors of parallax. These observations, if secured, would however be very valuable, as a check upon the results obtained from the discussion of the observations at stations where the time is largely affected by parallax. We are not likely to hear any news of the Madagascar expedition for some weeks. The British expeditions have, on the whole, been most successful, and a valuable result is assured.

Up to the present time the following additional details have appeared in the *Times* as to the observation of the transit at home and abroad. At home the meteorological conditions were generally unfavourable :—

At Greenwich Royal Observatory the Astronomer Royal had made considerable preparations for observation, and shortly before two o'clock the whole of the staff were at their instruments, ready to take advantage of even a break in the clouds; but unfortunately the dense stratum of cloud which lay beyond the occasional rapidly passing patches of scud prevented even the sun's position being discerned. Arrangements were made for taking a number of photographs, a photoheliograph having been specially erected to view the sun until it reached the horizon. At the Radcliffe Observatory, Oxford, Mr. E. J. Stone had made considerable preparations for observation, but the sun was only visible for about five seconds, when the planet was seen on the solar disc, well separated from the limb. At Bath the haze which was prevalent during the morning cleared away, and the transit was visible till sunset. In South Wales the sky was clear until shortly before sunset. At Penzance, Plymouth, and Cork the sky was cloudless. Mr. J. Burns, at the Castle, Wemyss Bay, observed the external contact at 2h. 6m. 38s., and internal contact at 2h. 20m. 32s. (Greenwich mean time). The Rev. W. S. Lach-Szyrma, at Penzance, saw the transit from the time of contact to sunset; the black drop was clearly seen.

The astronomers at Potsdam succeeded in taking good photographs of the transit. In France, no observation could be made. At Paris, Bordeaux, Grenoble, Lyons, and Marseilles it was cloudy. MM. Thollon and Gouz, in Portugal, could also take no observations. M. Dumas received telegrams giving the main results of the observations of the transit in Oran, Martinique, and Mexico. In Martinique, M. Tisserand detected the first contact of the planet and the sun, but unfortunately he had scarcely commenced recording his observations when a cloud came over and concealed the rest of the phenomenon. At Puebla, on the other hand, M. Bouquet de la Grye had an unmixed success. The entire transit was visible, and he was able to take observations for determining the parallax. He will now, of course, work out these calculations, which promise to be amongst the most important of all the observations. At Oran M. Janssen was likewise favoured with sunshine. He was not commissioned by the Academy, but made spectroscopic observations on his own account. In this department he seems to have admirably succeeded, having obtained capital photographs 30 centimetres in size. He telegraphs that he has not only taken excellent photographs, but that he has further been able to observe atmospheric phenomena. As to Col. Perrier, who was posted in Florida, the French Foreign Office have received a telegram reporting full success, but giving no details. No telegrams from the missions in Patagonia, Chili, and Port-au-Prince have as yet been received. In short, of the results thus far known only observations in Mexico and Florida for the calculation of parallax, and those of Oran with the spectroscope seem to have been successful.

The transit was observed at many places in the United States. Special observations were made at the Observatory of the Central High School of Philadelphia by Professors Snyder and Ritter. The weather was favourable, but clouds obscured the sun during part of the time that Venus was crossing the sun's disc. The sky was cloudy all day. All four contacts were observed, and the last two well observed. The weather was not so favourable for the observation of the first two. The planet was seen off the disc at first, and in the fourth with a ring of light frequently visible. While the exact time has not yet been computed, it is known that the first two contacts were in advance of the ephemeris. Prof. Snyder says that just at or before the first contact the planet was projected on the chromosphere. The point where this occurred was verified by a notch of the advancing planet. As Venus approached the second contact a bright luminous horn darted out from the sun round the planet, but not encircling it, being only visible on one side. The same thing was also visible at the third contact. Just before the second contact the edge still off the sun was illuminated by a most beautiful hazy ring of light, seeming to have a sensible breadth. During the second contact the ligament phenomenon was visible, but not so markedly as it was observed in the case of the transit of Mercury in 1878. Just preceding and during the second contact the atmosphere was hazy, but the phenomena were well observed nevertheless. The weather at the third contact was much better, and the ligament phenomenon was not noticed, though a faint obscuration of the luminous line existed just before the geometrical contact, the latter being well observed. After the third contact the horn appearance again came, there being several times noticed evidences of a ring of light. At the last contact, and after the notch had disappeared, the planet seemed to linger off the edge of the sun. The Philadelphia observations of all four contacts are considered to have been successful. A snowstorm prevailed in Canada and the Northern portion of the United States, seriously interfering with the observations elsewhere. But successful views of at least part of the contacts are reported from Ottawa, Albany, Howard University, near Boston; the Naval Observatory, Washington; and the Johns Hopkins University, Baltimore.

Prof. Sharpless, posted at Haverford College Observatory, near Philadelphia, reports the Washington mean time of the contacts as follows:—First contact, 9h. 56m. 5s.; second contact, 9h. 15m. 49s.; third contact, 2h. 39m. 43s.; fourth contact, 2h. 59m. 51s.

At the Washington Naval Observatory the observers slightly differ in the times recorded for the contacts. Prof. Frisby, who had a six-inch equatorial, reports that the first contact occurred at 8h. 56m. 45s.; the second contact at 9h. 16m. 9s.; the third at 2h. 38m. 57s.; the fourth at 2h. 58m. 55s. Prof. Sampson, with a nine-inch equatorial, reports the first contact to have happened at 8h. 55m. 9s.; the second at 9h. 16m. 19s.; the third at 2h. 39m. 56s.; the fourth at 2h. 59m. 37s.; the time of the fourth contact is somewhat uncertain, owing to the prevalence of clouds. Washington mean time was used. Fifty-three photographs were taken during the transit. The professors report that their labours were generally successful, and that if other stations were equally fortunate the result ought to be computed within two years.

Successful observations were made at Yale College, Newhaven, and also by Professor Draper in New York, who got good photographs. The French Astronomical Commission succeeded, at St. Augustine, Florida, in taking 200 photographs. Partially successful results were obtained by the Belgian Commission at San Antonio, in Texas, where 204 photographs were taken of the later phases, the first two contacts being lost. Fully successful observations were made at San Francisco, with 48 photographs; and partly successful ones at Cedar Keys, Florida, where the first contact was lost, while the other three were well observed.

One hundred and eighty photographs were successfully taken by the German Commission at Hartford, Connecticut. They failed to observe the first contact, but afterwards got eight full sets of heliometric observations, which made all they desired, and which they consider very satisfactory. No trace of a satellite was visible. Partly successful results were further obtained by the Germans at Aiken, South Carolina, where they lost the first two contacts, but got three sets of heliometric measurements in the afternoon.

At the Harvard College Observatory spectroscopic observation showed no perceptible absorption of the sun's light by the planet's atmosphere.

Telegrams announce a complete success in New Zealand, Panama, New Mexico, Jamaica, and some parts of Australia. In New Zealand, England was represented by Colonel Tupman and Lieutenant Coke, R.N., both observers in 1874. The observations of the transit are described as very successful, and Colonel Tupman expected that the observations for determination of the longitude would be complete by Sunday last. The United States party, under Mr. Edwin Smith, observed at Wellington, and were successful in taking two hundred and thirty-six photographs. Of the Australian stations perfectly successful observations were secured at Hobart Town (Tasmania), Wentworth (New South Wales), and in South Australia. At Adelaide the transit was slightly obscured by clouds, but no information to hand states whether the contacts were observed or not. In Queensland no success was obtained. Mr. Russell at Sydney arranged to provide about ten observers at lofty stations along the east coast; heavy rain fell in Sydney and Gippsland, but it is probable that observations have been secured by some of the observers, although no success was obtained at the Sydney Observatory. At Melbourne observations were secured, and twenty-three photographs taken. The Government of Victoria provided for two or three stations respecting which no information has been received, but judging from the state of the weather in Melbourne, there is a probability of success.

We have received the following communications on this subject:—

In a communication from the observatory of the Collegio Romano we are informed that the observations of the transit at Rome were quite successful, although the sky a few minutes before contact was cloudy. Signor Tacchini observed the contacts with the grating spectroscope applied to a Merz refractor of 25 c., and M. Milloseisch simply used a Cauchoix refractor of 15 c. in the ordinary way. In the morning, Signor Tacchini, was able to make his usual spectroscopic observations on the limb of the sun, and he saw that on that part of the limb at which the first contact would take place, the chromosphere was regular, but composed of very active flames, and two protuberances bounded the section (*trait*) of the chromosphere, on which the planet might be looked for before the first external contact. In fact, Signor Tacchini, at 2h. 24m. 33·8s. mean Roman time, saw the edge of the planet on the sharp points of the chromospheric planes He continued to see very clearly the planet advance towards the base of the chromosphere, and he observed the first external contact at 2h. 48m. 54·43s. Afterwards he watched the complete reappearance of the chromosphere, and then he noted the first internal contact at 3h. 9m. 34·79s. The image of the chromosphere was always very well preserved, and the size of the planet projected upon it always very clear. Prof. Milloseisch observed the contacts at the following times:—First external contact, 2h. 49m. 48·14s.; first internal contact, 3h. 9m. 29·34s., for the moment of the appearance of the black drop, and 3h. 10m. 10·14s. for the moment of the disappearance of the drop. Between the times noted by the two observers for the first contact, the difference amounts to 94 seconds, which clearly proves the great advantages of making use of the spectroscope. Shortly after the first contact, Prof. Milloseisch perceived for the first time the presence of the planet's atmosphere, verified by Signor Tacchini and his assistant. Signor Tacchini even observed in the spectroscope the phenomenon of absorption by that atmosphere, as in Bengal in 1874, and even at Palermo something of the same kind has been seen. MM. Tacchini and Milloseisch did not see the entire planet before the first contact. The atmosphere of the planet was more active near the edge of the sun. Prof. Milloseisch estimates the height of the atmosphere of Venus at one-fourteenth of the planet's diameter. Signor Tacchini found the diameter of Venus to be equal to 67″·25.

I HAD the advantage of seeing here yesterday the transit of Venus, under exceptionally favourable circumstances, by means of a very simple and ingenious apparatus fitted up by my cousin, Mr. J. Campbell, of Islay. The image of the sun was thrown from a small telescope, properly focussed, upon a large sheet of cardboard paper, in a dark room. The size of this solar image was a little more than two feet in diameter. Upon this image the solar spots and some brilliant "faculæ" were very distinctly visible. As the time approached, Mr. Campbell expected that we might see the planet whilst yet some little distance from the illuminated edge of the sun, owing to the atmosphere of the planet catching and reflecting some solar light before the apparent contact. I believe Mr. Campbell had seen this on the occasion of the transit, in the clearer atmosphere of Japan. Here, none of the party could detect the planet before its disc began to impinge on the edge of the sun. But when the planet's disc had advanced about one quarter of its own diameter upon the solar image, then a faintly luminous ring was distinctly seen defining the rest of the planet's disc, in the darkness out of which it was moving. For some time I was incredulous as to this appearance; but before one half of the planet's disc had crossed the illuminated edge of the sun, the luminosity of the other side of that disc was too distinct to be doubted, and the appearance was very striking and beautiful. I may mention that the size of the planet's disc was, as nearly as possible, seven-tenths of an inch. The time of contact was exactly 2.28 p.m.

Cannes, December 7 ARGYLL

AT 1h. 48m. 28s. Dunsink mean time, I first saw the sun through an opening in the clouds. Venus was at once seen (in the finder of three inches aperture attached to the south equatorial), and was estimated to be about one-third of the way on the sun. But the snow and clouds again intervening, there was nothing more to be seen until 2h. 37m. 19s., when I was enabled to commence a series of micrometric measures with the large instrument. I used a polarising eye-piece and a power of 177 with the Piston and Martin filar micrometer. The limb of the sun was boiling furiously, and Venus was often of any shape but a circular disk. The measures were consequently by no means easy. I set one wire tangentially on the sun's limb, and the other on that of Venus. Altogether, I was able to make sixteen of such sets, nine being made with the limb of Venus nearest the sun's centre and the remainder with the other limb. The following are the results:—

Dunsink mean time.	Far limb of Venus	Near limb of Venus.
h m. s.	″	″
2 37 19	171	—
39 24	177	—
40 42	185	—
42 18	187	—
44 13	191	—
45 20	190	—
46 22	198	—
48 17	196	—
52 16	—	142
54 16	—	149
55 36	215	—
58 17	—	161
59 36	—	164
3 0 46	—	161
1 41	—	164
2 51	—	170

These results have not been corrected for refraction.

I conclude from the mean of the first series that at 2h. 43m. 30s. the far limb of Venus was 188″ from the limb of the sun, while at 3h. 0m 0s. I conclude from the second series that the near limb of Venus was 162″ from the sun. By a projection of the results it is easy to see that the diameter of Venus must have been 64″.

My assistant, Mr. Rambant, was observing with the small equatorial, which is 78 metres from where I was observing. He reports as follows: "At 1h. 45m. the clouds, which up to that time had obscured the sun, cleared away, and I saw the planet with about one-half of its disc projected on that of the sun, but a snow-shower coming on almost immediately, I was unable to perceive any trace of light rouud Venus, or even to follow its outline beyond the limb of the sun. By the time the snow cleared away the internal contact was passed, and Venus appeared at about twice of its own diameter from the sun's limb; the sun's light appeared of great brightness right up to the dark disc of the planet except at the northern limb, where I suspected a dark brown fringe, but the boiling was such that I could not be certain of this.

R. S. BALL,
Astronomer Royal of Ireland

Dunsink Observatory, Dublin

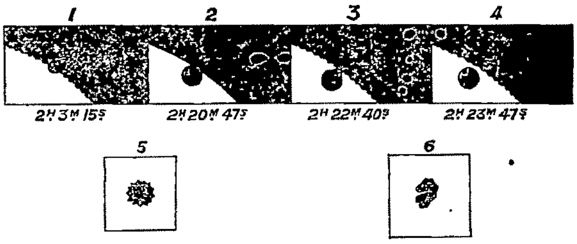
THE transit of Venus was well seen at this observatory owing to the unusually favourable state of the weather. I observed it in the Markree refractor with an aperture reduced to five inches. I did not meet with any difficulties and saw no "black drop," perhaps because I had focussed the eyepiece on double stars the night before. Owing to the boiling of the sun's edge, I did not see Venus till 1h. 27m. 38s., external contact having taken place previous to this. I then measured the distance between the cusps micrometrically, and from a provisional reduction of these observations it appears that Venus was bisected at 1h. 37m. 53s. The internal contact was first noticed at 1h. 46m. 40s., when a fine line of light appeared at the outer edge of Venus. At 1h. 47m. 58s. the cusps met for an instant, but did not unite permanently till 1h. 48m. 28s. (Markree mean time). Venus was visible on the sun till sunset. I measured its semi-diameter =29″·07. W. DOBERCK

Markree Observatory, December 7

THE ingress of Venus was observed at the Armagh Observatory under very favourable circumstances. I employed the 15-inch reflector in the Newtonian form with an unsilvered flat mirror and a negative eyepiece (power 140) and glass wedge. With an aperture of eleven inches the sun's limb was "boiling" considerably, so that I missed the exact moment of external contact; but having reduced the aperture to seven inches, the internal contact was very well seen. At 1h. 49m. 31s. local M.T. the whole circumference of Venus could be seen; at 1h. 54m. 49s. I saw a faint shade-like object between the cusps, which broke at 1h. 55m. 24s, when a very thin bright line separated Venus from the sky outside; 27s. afterwards the interval was very conspicuous. Rev. Ch. Faris, assistant astronomer, observed with the 4-inch finder attached to the same instrument, and every care was taken to make the two observations independent of one another. He observed external contact at 1h. 35m. 34s., and saw the cusps meeting at 1h. 55m. 10s. without observing any disturbance of the limb. Good determinations of time were obtained on the previous and following evenings. J. L. E. DREYER

Armagh Observatory, December 9

THE all-important 6th of December, 1882, will long be remembered by us in Lochaber, who had eagerly looked forward to a sight of the transit of Venus. Our hopes were realised to the full, and considering our somewhat high latitude, we were privileged indeed. An account of the meteorological conditions will be of introductory interest. A barometric depression had two days previously travelled down from the vicinity of the Faroe Islands, bringing overcast weather and rain—very discouraging—but the mercury rapidly rose in its rear on the night of the 4th, light north-easterly winds set in, the sky cleared on the 5th, and free radiation, and a hard frost followed with charming winter weather. At 9 a.m. on the 6th the barometer, corrected and reduced to 32° F. and mean sea-level read 29·677, and was steady, dry bulb 27·3, iced bulb 26·1, giving a vapour tension of ·112, relative humidity 76 per cent., and a dew point of 20·9. Light airs were noted from north-east by east, and some pieces of innocent-looking cumulus clouds were observed. Browning's spectroscope, soon after 9 a.m., showed an entire absence of rain-band to the left of the D line, in the solar spectrum, but a broad telluric band stronger than usual was observed on the right of D; this, however, did not much distress me. The meteorological conditions generally continued much the same, the weather being very fine. When the hour for the beginning of the transit drew nigh, I repaired to a field adjacent, entirely open to south-west, with telescope and sketching equipment. Here an uninterrupted view of the sun could be obtained. The instrument employed has a clear aperture of 2⅜ inches, object-glass of first quality by Dancer of Manchester, power used 70. A few clouds near the sun about 1.53 caused anxiety, but they soon cleared off, and a perfect and continuous view of the sun was obtained. Two good watches and clock were set to Greenwich mean time, obtained with all possible accuracy from post-office signal in the morning. Considering the sun's low altitude, the thick stratum of atmosphere traversed by the oblique rays, tremors of the air, and effects of atmospheric refraction, the phenomenon of the ingress was well observed in the telescope. The external contact, when the dark body of Venus just indented the sun's limb, south-east by south, took place at 2h. 3m. 15s. Greenwich mean time (see sketch No. 1), by 2h. 13m. 0s. the half of the planet was upon the sun; sketch numbered here 2 (several others not now given were taken) was made just before the internal contact; and at 2h. 22m. 40s. I noted the internal contact. At this time (sketch No. 3) I observed the ligament joining the edges of Venus and the sun, like the thread between two drops of water when about to part, and the planet was much in shape as an apple with the stem joining on to the edge of the solar orb. At 2h. 23m. 47s. (sketch No. 4) Venus was as a round black spot upon the sun, and clear of his edge, and a narrow streak of light intervened. By this time friends had gathered around, and as the chief observations had been made they were enabled to take turn at the instrument and watch the progress of the planet in its course across the sun's disc, until a mass of cumulus cloud at 2.45 put a stop to observation. The outline of Venus against the sun was very irregular (sketch No. 5). Mr. Colin Livingston, of the public schools, also observed the ingress independently, and we agree to the very second that the internal contact took place 19m. 25s. after the external contact. The sun's photosphere by the way was almost wholly free from spots. Just a stippling was observed a little west from the centre, and a small disturbance was noted in perspective near the eastern limb—in marked contrast with the great spot regions well observed here on November 16, and now probably existing in the opposite hemisphere. The sun's edge was very uneven, as I have attempted to show in the sketches. The highest temperature during the day, it is worthy of mention, was only 30·9, and pressure remained fairly steady at a mean of 29·670 at sea-level. Views of the transit were again obtained before sunset, but the intensity of refraction near the horizon so distorted both the sun and Venus—the former being like an egg in shape, and the latter at times as shown in sketch No. 6—that I could but wistfully watch them go down together on a gorgeous sky behind the snow-clad heights of Beinn na Cille, envy our cousins in the west their sight of the egress, and wonder under what strange circumstances the next transit will be observed in the year of our Lord, 2004. CLEMENT LINDLEY WRAGGE



Transit between 2.30 and 2.45.

Venus distorted shortly before sunset, photosphere apparently bisecting planet.

The Transit of Venus (Ingress) as observed by Clement L. Wragge.

Fort William, December 10

THIS rare phenomenon was well observed here on the 6th inst. The few clouds which partially hid the sun during the first stages of the transit, only served a useful purpose in moderating the sun's brilliancy. At about 2 p.m. a dark indentation was observed on the south-east margin of the sun's limb, and it was evident the phenomenon had commenced. A few minutes later this indentation had developed into a semicircular notch, and at about 2.21 p.m. the black and now complete circle of Venus had fully entered upon the solar disc. It was very large and conspicuous, and its effect, even as observed in small telescopes, was very striking. The opaque and well-defined globe of the planet was projected with remarkable boldness upon the sun's bright photosphere. Protecting the naked eye with deeply tinted glass, the planet was very plainly seen; indeed, the dark spot was thus clearly distinguishable before it had entered fully upon the sun, and while it had notched the disc. The planet appeared to be surrounded by an annulus of reddish hue, and over the central parts there was diffused a patch of faint light. The region of the disc just within the margin was very black. These effects may, however, have been purely telescopic. I used a reflector of 4-inch aperture, with which the definition was not all that could be desired. There were very few of the ordinary sun-spots visible. A small irregular group lay slightly to the south-west of the centre, and another with much feculæ, near the eastern limit, but they were of very insignificant character, and not at all comparable to some of the fine spots which have been recently visible on the solar surface. To an observer accustomed to the appearance of these objects, the view of Venus now in transit must have been of extreme interest, and he could not fail to be struck with the marked difference between the black circular disc of the planet, and the more irregular and far less intense forms of the ordinary sun spots. As the transit progressed, the sky continued clear, so that it could be watched until near sunset, but the telescopic view became less effective, owing to increasing atmospheric undulation, which, as usual with objects at low altitudes, greatly impaired the definition. W. F. DENNING

Bristol, December 9

SOME parts of the transit were well seen here. I used a 3¼ Merz refractor, power 60. The external contact was excellently seen. I watched for those peculiar phenomena (black drop, &c.) which have created so much interest, but was able to see nothing of them. At the moment of external contact, I had the point of impact in the centre of my field, and the planet indented the edge of the sun with a black and perfectly circular segment, disturbed only by the "boiling" appearance characteristic of the solar edge. I watched the planet advance upon the sun to within, I guess, a few seconds of internal contact, when unfortunately the sun became obscured by a small cloud. At the time of observation, so near was internal contact, that I could every now and then see the boiling appearance of the solar edge peeping out from behind the black edge of the planet; but no other distortion of the planetary or solar edge was observable, except what arose from the "boiling" appearance referred to. D. TRAILL

Raleigh Lodge, Exmouth, December 7

THE transit of Venus was perfectly seen here yesterday. The sky was overcast quarter of an hour before, but the first and second contacts took place on a clear disk, and the first was almost instantly apparent with a hand glass. The sky remained clear till considerably past three. Considering the total failure in London and Paris, one could wish that some trained observers had selected our south coast. HENRY CECIL

Bregner, Bournemouth, December 7

P.S. As a consequence of popular ideas and anticipations as to celestial phenomena having got "a little mixed" lately, my gardener asked me this morning "*if I saw the star fall into the sun yesterday.*"—H. C.

THE transit was seen here to great advantage, the day being exceptionally fine, the sun shining brightly from about 9·30 till sunset, with the exception of a few passing clouds at noon, and one small cloud obscuring the sun from 2.30 to 2.35 p.m. According to the times and positions of the sun and planet, given in the *Nautical Almanack*, I had made a diagram as correct as I could of the sun with the path of Venus across his disc; but if I had relied entirely upon the diagram I should have missed the place of external contact, as I found afterwards I had drawn the position of Venus considerably too high. To make sure of my object I depressed the telescope so as to keep as much of the sun as possible out of the field of view, and only allowing a portion of his limb to appear, and at 2h. 3m. 11s. I picked up Venus depicted against the sky, and just coming in contact with the sun's limb. Her disc appeared a trifle paler than the background, and was surrounded with a very thin circle of light which appeared a little wider on that side furthest from the sun. It was this light which attracted my attention, and enabled me to identify the planet. At 2h. 3m. 20s. the first *touch* of Venus upon the sun's limb took place. I now watched with much interest to see if it was possible to detect signs of an atmosphere to Venus—changing the eye-pieces. Sometimes I thought there were visible signs of it, but I would not say decidedly that it was so. I noted with some surprise that the planet appeared much smaller upon the sun than it did immediately previous to contact. At 2h. 23m. 31s. internal contact took place. The planet appeared to pass clear off and away from the sun's limb, without showing the least sign of a "black drop," or any appearance of a lingering connection between her limb and that of the sun. The telescope used is an equatorial with driving-clock, silvered glass reflector 8½-inch diameter and 7 feet focal length; eye-pieces ranging from 30 to 170 of magnifying power. I took three photographs between 2h. 35m. and 2.45, but the spring of my instantaneous shutter did not act as it should, and therefore the photographs are not so good as I could wish, but Venus can be readily seen upon the sun's image in the negative.

Silverton, Devon, December 9 R. LANGDON

NOTES

THE following are the probable arrangements for the Friday evening meetings before Easter, 1883, at the Royal Institution:—January 19, R. Bosworth Smith, M.A., The Early Life of Lord Lawrence in India; January 26, George J. Romanes, F.R.S., Recent Work on Starfishes; February 2, Sir William Thomson, F.R.S., The Size of Atoms; February 9, Moncure D. Conway, M.A., Emerson and his Views of Nature; February 16, Prof. William C. Williamson, F.R.S., Some of the Anomalous Forms of Primæval Vegetation; February 23, Walter H. Pollock, M.A., Sir Francis Drake; March 2, C. Vernon Boys, A.R.S.M., Meters for Power and Electricity; March 9, Prof. George D. Liveing, F.R.S., The Ultra-Violet Spectra of the Elements; March 16, Prof. Tyndall, F.R.S.

MR. H. O. FORBES, on his return to Amboina from his first visit to Timor-laut, writes as follows:—"Extended movements were impossible, so that my botanical collections are not very extensive, but the ornithological and anthropological parts are very good. I am now engaged in packing them up for despatch, and hope to send them off soon. My intention is to return to Timor-laut in a few days, if my health will permit, by the Government steamer which leaves for the Tenimber Islands. I shall settle in some more quiet spot than Ritabel. A full report on this interesting country shall be sent by next mail. One of the singular facts I observed is the immense herds of wild buffalo existing on the mainland of the island. They must have, of course been introduced, but by whom, and how long ago, is an interesting question. I was unable to get a specimen unfortunately. My wife, who accompanied me, aided me greatly, so that when I was down with fever (and the fever is of extreme severity) the work was still able to go on." Mr. Forbes' collections will be consigned to the Committee of the British Association for the exploration of Timor-laut, as arranged when the expedition was determined on.

THE Accademia delle Scienze dell' Istituto di Bologna has lately announced that a gold medal of the value of 1000 lire (say 40*l.*) will be presented "to the author of that memoir which, proceeding on sure data either of Chemistry or of Physics, or of Applied Mechanics, will indicate new and efficacious practical systems, or new apparatus for the prevention, or extinction of fires." Memoirs must be written in Italian, Latin, or French, and sent in (in MS. or printed form, and in the usual anonymous way) before May 30, 1884.

ANOTHER well-known naturalist has passed away. Prof. Andrea Aradas, of Catania, died on November 1 last, after a long and laborious life, which was devoted to the study of marine zoology and palæontology. His publications were very numerous, and extended over nearly forty years. He was a man of great amiability as well as learning.

THE death is announced of Sir Thomas Watson, M.D., F.R.S., the eminent physician, at the age of ninety years.

ON Monday evening the annual dinner of the Professors and Members of the Royal School of Mines was held in the Victoria Room of the Criterion, Piccadilly. Mr. E. L. J. Ridsdale, late of the Royal Mint, presided. It was announced that Major-General Martin was about to retire, owing to ill-health. Prof. Huxley made a long and interesting speech, in the course of which he recalled the personal characteristics of the professors who filled the chairs at the school. Prof. Judd proposed "The Geological Survey," to which Dr. A. Geikie responded.

PROF. KENNEDY has issued invitations for an inspection of the experimental engine and other apparatus just completed at the Engineering Laboratory, University College, London. Prof. Kennedy draws attention to the fact that the laboratory was the first of its kind established in England, and was at the time of its establishment an entirely new departure in technical education in this country. Since that time (1878), its principle has been more or less formally adopted by all the recently-established technical colleges. A very large number of the leading engineers of the country have also formally expressed their approval of the scheme, which, too, came in considerable detail before the present Royal Commission on Technical Education. The additions now just finished to the Laboratory render it already probably the most complete of its kind in Europe.

A RECENT writer in the *China Review* exemplifies the difficulties surrounding interpretation from Chinese into English, or *vice versâ*, by mentioning that the simple question, *Was he (or she) dead?* which occurs so frequently in inquests and other judical proceedings, admits of a positive or negative reply according to whether the European or the Chinese idea as to when death occurs be followed. We believe that a man is dead when he has ceased to breathe, and when his blood no longer circulates; the Chinese consider him still alive whilst a trace of warmth lingers in the body. The two estimates may thus differ by several hours. Hence it was that in inquests in Hongkong the time of death formed a stumbling-block in almost every Chinese case. The medical evidence would show that the deceased must have been dead when brought to the hospital, while the relatives would swear he was alive at the gate. Subsequent inquiry showed that the general view among the Chinese was that a person is considered to be dead when the body is cold, and not before. It does not speak very well for the Chinese scholarship of the officials of Hongkong that it took about forty years to discover this important distinction.

AN aurora was seen in Belgium on October 2, and one feature of it was (according to M. Montigny) the formation of a broad luminous arc extending across the sky from east to west, and passing a little to the south of the zenith. After a little time it broke up and gradually disappeared. M. Montigny observed the stars during this aurora, and found his former conclusions (as to increased scintillation during auroras greater in winter, and in the northern region, and towards the zenith, &c.) confirmed. He notes, however, an interesting new phenomenon of scintillation. For more than a year, when a magnetic perturbation has been observed to occur at Brussels Observatory, he has very often observed a simultaneous sudden increase in the scintillation. No auroral phenomena were reported at those times, as during aurora the increase is more marked for the north and west, and the circular line in the scintillometer becomes irregular. M. Montigny is prosecuting his study of the phenomena.

IN the same *Bulletin* of the Belgian Academy, with M. Montigny's paper (November 9–10) is a full description, by M. Tarby, of the aurora of October 2, as observed at Louvain. Besides the luminous arc referred to above (which moved towards the south), he notes that the aurora had not the pronounced red tint characteristic of large phenomena of this class; white streamers constantly predominated. The successive displacement of the manifestations was from east to west, by north, a direction presented in certain previous auroras (which he specifies); while the opposite direction was observed in others. M. Tarby tabulates several years' observations of aurora in Belgium, and finds striking confirmation of an observation of M. Quetelet's in 1870, that auroras (through some unknown periodic influence) tend to appear at about monthly intervals.

IN the pile-dwellings near Bobenhausen (Zürich), a hatchet made of pure copper has been discovered. Special importance is attached to this discovery by students of prehistoric archæology.

THE fourth edition of the Micrographic Dictionary is now more than two-thirds completed. The book will always be an indispensable work of reference to the student of the lower forms of animal and vegetable life. Very little attempt appears, however, to have been made by the editors to keep pace with the advance of biological science during the eight years that have elapsed since the publication of the last edition; notwithstanding the number of new forms that have been discovered during that period, the work so far occupies rather less space than before. In order to test the extent to which recent knowledge has been incorporated, we turned to two or three of the cryptogamic articles. Under "Fungi," we find it still stated that "the structure of all fungi exhibits a well-defined separation into two parts, namely: (1) a *mycelium* . . . and (2) the *reproductive structure*, or *fruit*"; and this although Schizomycetes are given as one of the groups of fungi; while the classification of "Fungi" into "I. Schizomycetes; II. Phycomycetes; III. Hypodermiæ; IV. Basidiomycetes; V. Ascomycetes; and VI. Myxomycetes" is stated to be "that of Sachs (!) slightly modified." Under "Lichens," the theory of the symbiosis of algæ and fungi is dismissed in a few sentences, without adducing any of the evidence in its favour, as "one of the modern natural-history romances." A new paragraph appears under the head "Gongrosira," which is described as a genus of Chætophoraceæ, without any reference to its genetic connection with *Vaucheria*. These and similar deficiencies suggest the question how far the text can have been revised by the eminent cryptogamist whose name still appears on the title-page.

THE concluding volume of the new edition of "The Imperial Dictionary," edited by Mr. C. Annandale, has been issued with praiseworthy promptitude by Messrs. Blackie and Son. In a supplement Mr. Annandale has added a considerable number of words omitted from their places in the body of the work, including not a few scientific terms. In the Appendix are copious lists of classical, scriptural, and geographical names, foreign words and phrases. In the preface the editor explains his method, which we think rational and judicious, and which has led to an excellent result. The list of authors consulted for quotations contains about 2000 names.

THE French official paper publishes an *arrêté* from the Minister of Public Works requiring that all trains be furnished with continuous brakes, and if possible automatic.

THE inundation of the Seine, which had reached a level of about 6½ metres above the summer season, and has caused many disasters, has terminated abruptly by the cold weather which has set in with the new moon.

AT the last meeting of the St Petersburg Society of Naturalists, M. Beketoff reported that the expedition for the exploration of the Altai sent out during last summer was very successful. MM. Sokoloff, Polenoff, Nikolskiy, and Krasnoff have returned with very rich botanical, zoological, mineralogical, and geological collections. He added also that the appeal of the Society for botanical collections (addressing them to the St. Petersburg University) had been responded to. No less than eighteen very good collections had been received, among which one by the scholars of all *Realschulen* of Western Siberia merits special attention.

IT is worthy of note that snow fell on Sunday in Madrid to the depth of one foot. It is said that no such weather has been experienced in the Spanish capital for twenty years.

THE diaries, pocket-books, cards, and the other useful and beautiful things issued by Messrs. De La Rue for the coming year are in all respects equal to those of which we were able to speak so highly last year. It would be difficult to imagine anything more beautiful of their kind than the cards, and what with Japanese beauties, flowers, birds, and insects, they might be utilised for giving the young ones a liking for natural history. The astronomical and other useful information contained in the diaries is as full and accurate as ever, and adds greatly to their value in our eyes.

AMONG the articles in the *Companion to the British Almanac* for 1883 are "Halley's Comet," by Mr. W. T. Lynn; "Modern Fish Culture" and "Fishery Exhibitions," by Mr. J. G. Bertram; "Insects Injurious to Agriculture," by Mr. W. E. A. Axon; "Electric Lighting," by Mr. L. T. Thorne; "The British Museum," by Mr. Charles Makeson; and a brief sketch of the Science of the Year, by Mr J. F. Iselin.

HARTLEBEN, of Vienna, has sent us a catalogue of German works, some of which might commend themselves to those who may wish to entice their young friends to the study of German.

A GERMAN translation is announced of Dr. Ingvald Undset's "Study in Comparative Prehistoric Archæology"; Meissner, of Hamburg, is the publisher, and the last number (23) of *Globus* contains an abstract of Dr. Undset's researches into the first appearance of iron in Northern Europe.

THE last number (vol. xvii. part 1) of the *Journal* of the North China Branch of the Royal Asiatic Society contains a short article by Dr. Guppy, R.N., on the Geology of the Neighbourhood of Nagasaki, and a few notes on the South Coast of Saghalin, by Mr. Anderson. The principal paper, occupying 180 pages, is on Annam and its Minor Currency, by M. Toda. Besides the portion devoted to numismatics, the author gives a short historical and geographical account of Annam, which should be valuable at the present time, when public attention is being strongly drawn by political events to these regions. Of the remaining papers, one, by Mr. Giles, discusses Chinese Composition; the other, by Dr. Hirth, describes a manuscript work written at the end of the last century, referring to the manner in which the Customs dues on foreign goods were then levied at Canton. It is called the "Hoppo" book, "Hoppo" being the title popularly given, even now, by foreigners to the principal native chief, or commissioner, of Customs at Canton.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus* ♀) from India, presented by Mr. W. Percy Laing; a Black-headed Lemur (*Lemur brunneus* ♂), a Black Lemur (*Lemur macaco* ♀) from Madagascar, presented by the I Company 3rd Battalion King's Royal Rifles; two Leopards (*Felis pardus* ♂ ♀) from India, presented by Lady Brassey; a North African Jackal (*Canis anthus*) from Tunis, presented by Capt. W. F. Wardroper; two Mexican Sousliks (*Spermophilus mexicanus* ♂ ♀) from Mexico, presented by Mrs. Simmonds; a Great Eagle Owl (*Bubo maximus*), European, presented by Mr. R. Leigh Pemberton; a Martinique Waterhen (*Porphyrio martinicus*) from Venezuela, presented by Mr. F. L. Davis; a Common Squirrel (*Sciurus vulgaris*), British, presented by the Hon. L. W. H. Powys; two Raccoon-like Dogs (*Nycterentes procynides*) from North-Eastern Asia, purchased.

OUR ASTRONOMICAL COLUMN

COMET 1882 *b*.—A number of very beautiful photographs of the great comet have been received from Mr. Gill during the past week. Several of them are remarkable for the amount of delicate detail that is brought out. Mr. Gill writes: "These photographs are interesting, not only as pictures of the comet, but they appear to me to show the possibility of making, with very little labour, a photographic *Durchmusterung* of the heavens." One of them taken on November 8 was exposed two hours, and shows all the 8th magnitude stars and the curious envelope extending 4° or 5° beyond the nucleus. This envelope was barely visible either to the naked eye or in the telescope.

Both Mr. Gill and Dr. Elkin had made a careful search for the cometary body seen within a few degrees from the nucleus of the great comet, by Prof. Julius Schmidt at Athens.

We have more than once pointed out that calculations based upon such observations as were available here at the time of writing, indicated sensible disturbance of the comet's motion at the perihelion passage. It is right, therefore, that we should state at once that this inference is hardly countenanced by calculations made by Mr. Finlay and Dr. Elkin at the Cape, who have had the advantage of more numerous, and probably in general more accurate and uniform series of observations. Mr. Gill writes: "The great comet is a puzzle. The whole question of its orbit now turns on which point of its nucleus should be observed. So long as the nucleus was single, *i.e.* from September 8 to September 28, Dr. Elkin has been able to represent its motion by parabolic elements within 3″ of observation. But after September 28 matters change; the head begins to break up. What we took for the principal nucleus is no longer the centre of gravity. Finlay and Elkin's original elements are now nearly 2′ out. Elkin's subsequent elements founded on observations September 8 to 28, give a place corresponding nearly with the end of the elongated nucleus (about 1½′ long) furthest from the head. Now (November 21) the nucleus is getting very ill-defined. We have done the best we can in the matter, and shall continue the best observations we can, as long as the comet is visible."

COMET 1882 *c* (Barnard, September 10).—From an approximate orbit calculated by Mr. Hind, and communicated to Mr. Gill at the Royal Observatory, Cape of Good Hope, which reached him on November 11, this comet was found the same evening, and was observed on the meridian on several days up to November 19. The first position from a lower transit is as follows:—

		RA		Decl.
Nov. 11	...	12h. 53m. 21s.·74	...	−65° 57′ 28″·3

Mr. Gill's observations will allow of a much better determination of the orbit of this comet, than could have been made from the European observations alone; the comet arrived at perihelion on November 13.

GEOGRAPHICAL NOTES

MR. JOSEPH THOMSON sailed yesterday for Zanzibar as leader of the Geographical Society's Expedition to Mount Kenia and the East Coast of the Victoria Nyanza. Mr. Thomson expects to be away for two years.

THE new number of the *Deutsche Geographische Blätter* continues the interesting account by Dr. Arthur Krause of the researches of himself and his brother in the Chukchi peninsula and Alaska; there is, besides, a separate catalogue of the ethnological collections, and a short paper by Dr. Kuntz of the plants collected. The number contains a useful paper on South New Guinea from the observations of D'Albertis, Moresby, Macfarlane, and others. In the *Zeitschrift* of the Berlin Geographical Society are several papers of interest. Major Lovemann gives the leading results of the new survey of Russia, which is being carried out; Dr. Hann examines the data of Dr. Rholf's for the altitudes in the oasis of Kufra; Herr G. A. Krause gives some account of the Saharan town of Chat, which is followed by an abstract of the census of Bulgaria; and a preliminary account of Prof. Haussnecht's Oriental travels. Dr. W. Gotz contributes a valuable paper on a subject which is taking great prominence in Germany—commercial Geography, while Dr. Reiss contributes an analysis of recent researches in some tributaries of the Amazon. In the December number of the *Deutsche Rundschau* for geography and statistics (Vienna, Hartleben), we have the conclusion of Baron von Lehnest's paper on his Land Formations in the Lunda region, the first of a series of pictures from East Africa, by Karl Berghoff; a short paper on the distribution of islands, and a biography of Mr. A. R. Wallace, with a good portrait. The number contains many other short papers and notes.

THE new quarterly number of the *Bulletin* of the Paris Geographical Society reports at length several important papers: Commander Gallieni gives an account of his mission to the Upper Niger and Segou, with a map and several interesting illustrations, some of which show curious formations, suggesting the buttes of some of the North American rivers. M. A. d'Abbadie has a useful paper on the spelling of foreign words; M. Jules Garnier an account of his excursion to the country of the Don Cossacks; M. M. Biollay, a paper on Finland; M. Dutreuil de Rhins, on Père Creuse's journeys to Southern China; M. Romanet du Caillaud, notes on the Ting-King; and M. Theodore Ber the first part of an elaborate paper on the Titicacon valley of Tiahuanaco.

THE December number of *Petermann's Mittheilungen* contains some supplementary information by Dr. Junker on his Welle explorations, in addition to the letters already referred to. Herr R. A. Hehl contributes a geographico-geological sketch of the Brazilian coast-lands between 20° and 23° S. lat. Along with the chief results of the Hungarian Census is an excellent series of statistical maps showing the various aspects of the figures. Signor P. Gialussi contributes an interesting paper on the changes which have resulted from recent geological action in an Istrian valley, while Herr Hehl gives a detailed account of the German colonies in South Brazil.

THE Carpathian Club, which was formed at Hermannstadt (Transylvania) after the pattern of the Alpine Club in 1880, having for its object the study and minute investigation of the mountains of the country, as well as the endeavour to direct the attention of tourists to that region, already numbers no less than 1200 members. It is divided into nine sections. Quite recently the second year-book of the Club appeared, which contains a number of valuable scientific papers, as well as descriptions of tours in the Carpathian Mountains.

SCHWEIGER-LERCHENFELD'S interesting work "Die Adria,' has just been completed in twenty-five parts, and published by Hertleben of Vienna. The fact that the eastern coasts of the Adriatic are so little known by the general traveller, renders the book valuable. In an appendix the commerce of the Adriatic, as well as the fisheries, are spoken of, and an excellent map is added to the work.

THE ROYAL SOCIETY[1]

II.

THE subject of the Circumpolar Observations mentioned in my address last year, was since that time brought more formally before our Government by that of Russia. At the request of the Treasury, the President and Council, after consultation with the Meteorological Office, advised as follows:—

"The object of the undertaking is to throw light on the influence of the great inaccessible region surrounding the pole on the meteorology and magnetism of the earth. With this view it is proposed to take simultaneous observations at a chain of circumpolar stations for a full year at least.

"A chain of not less than eight stations will be occupied independently of any co-operation by this country. This chain, however, leaves a gap of 90° in longitude in the northern part of America, the centre of which would be advantageously occupied by a station in the Dominion of Canada. The value of the results will be greatly enhanced by the addition of this link to the chain Independently of this, such a station would be of great value as being of a continental character, in contrast with the other stations, which are in close proximity to the coast. By choosing for the station one of the forts of the Hudson's Bay Company, no great outlay need be involved in its occupation."

The point first proposed was Fort Good Hope, near the mouth of the Mackenzie River; but it was found too late to erect the necessary huts and to transport the party and its provisions there during the present season. Fort Simpson, on the same river, was next suggested. Guided by considerations of facilities of access and sustentation, the Committee came to the conclusion that either Fort Rae or Fort Providence, on Great Slave Lake, is to be preferred to Fort Simpson, with which the former forts nearly agree in latitude; and accordingly the President and Council recommended one of these.

"In framing an estimate, it was thought well to assume that the expedition might last a year and eight months, so as to allow a sufficient margin for travelling to and from the station, and for possible detention in waiting for the Hudson's Bay Company's brigade. It is calculated that the cost might be safely estimated at 3,000*l*., which would include salaries of one officer and three men; journey of the party from England and back, including reasonable baggage; rations, allowances, and all other expenses."

To this communication the following reply was received:—

"My Lords have to thank you, and the Committee whom the Council appointed to advise them in the matter, for the valuable information contained in Dr. Michael Foster's letter of the 16th ultimo. Acting upon that information and upon the advice of the Royal Society, Her Majesty's Government have decided that this is an object on which public money may properly be employed and they are prepared to ask Parliament to provide a total sum not exceeding 2,500*l*. for the purpose. My Lords understand that there is good reason to hope that the balance required to make up the total estimated cost of 3,000*l*. will be forthcoming from other sources.

"I am to ask whether the Royal Society would be so good as to take charge of the Expedition under similar conditions to those under which the Transit of Venus Expedition is being conducted; accounts of the expenditure chargeable to the Parliamentary grant being rendered to this Department. The choice of stations, the appointment of observers, and the methods of procedure would be left entirely to the Society, subject to the condition that the total amount chargeable on public funds does not exceed 2,500*l*. My Lords understand that it is expected that not more than 1,500*l*. of this amount would come in course of payment during the present year, and they will present estimates to Parliament for 1,500*l*. and 1,000*l*. at the proper times."

The Canadian Government has since promised a contribution of 4,000 dollars towards the expenses of the expedition.

A committee, consisting of the President, Dr. Rae, Sir George Richards, Mr. R. H. Scott, and Prof. Stokes, was accordingly appointed to superintend the expedition, which, comprising Captain H. P. Dawson, R.A., in command, Sergeants J. English and F. Cookesley as observers, and W. Wedenby, as artificer, left England on May 11, for Quebec, was heard of at Fort Carlton on 27th June, and was about to proceed the next day for Green Lake, on the way to Portage Loche. It was still not quite certain whether it might not be necessary to push on to Fort Simpson, on account of insufficient accommodation, as well as lack of time and materials for building at Fort Rae.

Two parts of *Mittheilungen der Internationalen Polar Commission* have been published, containing full particulars and instructions relating to the whole circumpolar scheme.

The geological, mineralogical, and botanical collections, formerly in the Museum in Bloomsbury, have been properly arranged in the new building in Cromwell Road, and are on exhibition in their respective galleries. A commencement has been made in the transfer of the zoological collections. The osteological specimens, hitherto packed out of sight in an obscure vault in the basement of the old Museum, have been safely removed to the new building, and are now exhibited in a large and well lighted gallery. The collection of shells, which occupied the floor space of the long eastern gallery in Bloomsbury, is now suitably exhibited at South Kensington. Some of the corals have been removed, in order to clear the way for the removal of other specimens; and many of the stuffed quadrupeds and mammalian skins which had been stowed away in the old Museum basement are now in the new repository.

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[1] Address of the President, William Spottiswoode, D.C.L., LL.D., delivered at the Anniversary Meeting, November 30, 1882 Continued from p. 137.

The removal of the general collection of mammalia, of the birds, of the entomological specimens, and those of British zoology, will not be undertaken until after the coming winter. The fittings for the galleries prepared for them are not fully completed. The detached building designed for the specimens preserved in spirit cannot be made ready for their reception before the opening of next spring. It is, however, expected that the whole of the zoological collections will have been transferred to the new Museum by the end of June, 1883.

The subject of Technical Education has continued to be prominently under the notice of the country during the past year. The appointment of a Royal Commission on Technical Instruction, to which I have previously referred, has done much towards awakening the interest of manufacturers, and exciting curiosity in regard to the efforts that are being made abroad to improve the education of artizans. The Commissioners issued in March last their first Report, which dealt exclusively with primary education and apprenticeship schools. The Commissioners expressed an opinion adverse to the establishment of apprenticeship schools in this country; and in this view they are supported by nearly all our large manufacturers, and by the action of the City and Guilds of London Institute for the Advancement of Technical Education. At the request of the Executive Committee, I myself gave evidence before the Commission, explaining generally the objects of the City Guilds and Institute, and describing the progress already made towards their attainment. As a member of the Executive Committee of this Institute, I have watched its progress with interest, and have observed with satisfaction that its scheme of Technical Instruction is being gradually matured. The general Examinations in Technology undertaken by this Institute, were held in May last at 147 centres in 37 subjects. Of the 1,972 candidates who presented themselves for examination, 235 passed in Honours, and 987 in the Ordinary Grade. In 1881, 895 candidates passed, showing an increase of 307. The Examinations were held this year for the first time under the revised Regulations, which appear to have worked very satisfactorily. Two points deserve notice with respect to these Examinations. In the first place, the Institute experiences very great difficulty in obtaining properly qualified teachers. The applicants are either practical men working in the factory, or at their trade with no scientific knowledge whatever, or men possessing a very elementary science knowledge, and little or no practical acquaintance with the details of the industry, the technology of which they profess to understand. In order to indicate the kind of qualifications required in an ordinary technical teacher, the Institute has inserted in its programme a paragraph to the effect that persons who are engaged in teaching science under the Science and Art Department, and who at the same time have acquired a practical knowledge of their subject in the factory or workshop, may be registered as teachers of the Institute. The second point calling for consideration is the fact referred to in the Report of the Directors,—that of the 1,220 candidates who, this year, passed the examinations, most of whom are workmen or foremen in various branches of industry, not more than 450 are qualified to receive the full Technological Certificate, by having previously passed the examinations of the Science and Art Department in certain science subjects. This fact clearly indicates that widely beneficial as has been the action of this Department of State, there is still a large field for its influence among the population who are engaged in manufacturing processes, and desire to receive Technical Instruction.

One of the most satisfactory results of the Examinations of the City and Guilds of London Institute is the impulse they have given to the establishment, in different parts of the country, of properly equipped technical schools. At Manchester, Preston, Dewsbury, Hawick, Sheffield, Leicester, and other places, efforts have been made during this year towards organising schools for the technical instruction of artizans and others in the application of science and art to specific industries. At Nottingham, a grant of 500*l*. has been made by the Institute, to be followed by an annual contribution for a limited period of 300*l*., towards the establishment of technical classes in connection with the University College; and at Manchester a subscription of 200*l*. a year has been promised to assist the funds now being raised for the conversion of the Mechanics' Institution into a Technical School. The attention of the Council has been greatly occupied of late with arrangements for the opening of the Finsbury College. Classes in Electrical Engineering and in Technical Chemistry, have been carried on for nearly three years in temporary rooms belonging to the Cowper Street Schools. The attendance at these classes has been eminently satisfactory, much more so than could have been anticipated. During the past session 960 class tickets were sold at fees varying from 5*s.* to 12*s.* The staff of the College has recently been doubled by the appointment of a Professor of Mechanical Engineering, and a Head Master to the new Department of Applied Art, the establishment of which, as I stated last year, was then under the consideration of the Committee. In January next, it is anticipated that the new building in Tabernacle Row, which is already nearly completed, will be opened for the reception of students. The programme of instruction, prepared by the Director and the Professors of the College, has been for some time under the consideration of the Committee, and it is hoped that in the instruction given in this College will be found the realization of a very important part of the Institute's Scheme of Technical Education.

Grants to the Technical Science Classes at University College and King's College, London, to the Horological Institute, to the School of Art Wood Carving, and other institutions, have been continued during the past year.

The Technical Art School in Kennington Park Road, established and maintained by the Institute, has been satisfactorily attended; and a proposition is to be brought before the Committee for supplementing the teaching of this school by technical science classes, with a view of establishing in the south of London a Technical College for Artizans, similar to the one about to be opened in Finsbury.

The building of the Central Institution or Technical High School in Exhibition Road, the foundation stone of which was laid by H.R.H. the Prince of Wales, President of the Institute, in July, 1882, is rapidly advancing and promises to be completed within a year. It is not expected, however, that this school will be ready for the reception of the students before the commencement of the session 1884-5. Meanwhile, the Council and Committee are fully occupied with the development of other parts of their scheme.

In forwarding the Report of the Meteorological Council to the Treasury in December last, the President and Council took occasion to remind their Lordships that the arrangement for the organisation of the Meteorological Office generally, in May, 1877, would terminate with the then financial year. The Treasury, in reply, asked the advice of the Royal Society. After consultation with the Meteorological Council on various points connected with the subject, the President and Council reported fully to the Treasury, and concluded with the following general recommendation: "The President and Council beg leave to express a hope that the constitution of the Meteorological Council may remain unchanged, and that the same gentlemen who have hitherto performed its duties and administered its funds with such intelligence and judgment may be disposed to continue their labours." To this recommendation the Treasury cordially assented; deciding at the same time that no period should be fixed to the Meteorological Council for their tenure of office, but that it might be terminated by either party at any time on twelve months' notice.

The Meteorological Office has completed during the past year a series of charts of sea-surface temperature, for the three great oceans of the globe, and for the representative months of February, May, August and November. The work, which is now in the course of publication, will consist of twelve large charts, for the Indian, Atlantic, and Pacific Oceans respectively; and of four on a reduced scale, showing, for the four months, the isothermal lines of sea-surface temperature over the entire globe. In the preparation of these charts, all the observations existing in the Log Books of the Meteorological Office, and in the Remark Books of the ships of Her Majesty's Navy, have been employed, as well as the information which has been already rendered accessible in scientific memoirs, and in the narratives of the great scientific voyages. The isotherms agree substantially with those which have been already given for the months of February and August, in the wind and current charts published by the Hydrographic Department of the Admiralty; but as the present series is founded on a much larger number of observations than have ever before been available for a similar purpose, it may fairly be regarded as a valuable contribution to a not unimportant part of terrestrial physics. Between the limits of 50° north and 50° south latitude, the mean annual surface temperature, so far as it can be deduced from the data now available, appears to be 74°·9 F. for the Indian, 69°·5 F. for the Atlantic, and 68°·6 F. for the Pacific Ocean. The North Atlantic is 4°·6 F. warmer than the South Atlantic Ocean; the corresponding difference in the case of the Pacific Ocean is only 1°·8 F.

Among other contributions to Ocean Meteorology, which the past year has produced, I may mention (1) the Physical Charts of the Atlantic Ocean, published by the Deutsche Seewarte, at Hamburg; (2) the second volume of the narrative of the voyage of H.M.S. *Challenger*, containing the magnetical and meteorological observations; and (3) a report by Captain Toynbee, F.R.A.S., on the Gales of the Ocean District adjacent to the Cape of Good Hope, which completes the discussion by the Meteorological Council of the meteorology of that tempestuous part of the sea.

The meteorology of our own country has been actively studied during the year. The Scottish Meteorological Society have given in their Journal a series of monthly pressure charts for the British Isles, together with a revised edition of the temperature charts already published by them in 1871. The charts now embody the results of observations extending over a period of twenty-four years; the revised edition, as well as the original publication, are due to the indefatigable activity of Mr. Alexander Buchan, F.R.S.E., the Secretary of the Scottish Meteorological Society. An atlas of convenient size, intended for the use of observers in the United Kingdom, and conveying similar information derived from data partly different, and quite independently discussed, has been already prepared by the Meteorological Office, and will immediately appear.

It is a fact now universally recognised that the greater part of the changes of weather which are experienced in the British Isles are occasioned by travelling areas of excessive or defective atmospheric pressure, which arrive at our shores from the Atlantic Ocean. The importance of a systematic study of the weather of the North Atlantic being thus indicated, the Meteorological Council have resolved to undertake the preparation of synoptic weather charts for the thirteen months beginning 1st August, 1882, and ending 31st August, 1883, and have issued a special appeal to the British shipping interest for active co-operation during that period. It is satisfactory to know that this appeal has not been fruitless, and that there is every prospect that the number of observations available for the discussion will exceed 200 per day.

This is, perhaps, the proper place to make mention of some results having an important bearing on meteorology, obtained by Prof. Tyndall in the course of a larger research on the action of radiant heat on gases.

By methods which he has applied to gases and vapours generally, Tyndall has established anew the action of aqueous vapour upon radiant heat, and the sensibly perfect diathermancy of dry atmospheric air. The phenomena of solar and terrestrial radiation are profoundly modified by the presence of aqueous vapour in the earth's atmosphere, the temperature of our planet being thereby rendered very different from what it would otherwise be.

The celebrated experiments of Patrick Wilson, wherein were observed a rapidity of radiation and a refrigeration of the earth's surface previously unknown, are explained by the fact that when they were made, the amount of aqueous vapour in the air was infinitesimal, the unhindered outflow of heat towards space being correspondingly great. The sagacious observation of Six and Wells, that the difference between the surface temperature and that of the air a few feet above the surface, on equally serene nights, is greatest in cold weather, is explained by the fact that, when the temperature is low, the agent which arrests the surface radiation is diminished in quantity. Wells, moreover, found that the heaviest dews were deposited on nights when the difference between air temperature and surface temperature was small; while the greatest difference between the two temperatures was observed on nights when the deposition of dew was scanty. The explanation offered by Tyndall is this:—copious dew indicates abundant vapour; and abundant vapour, by arresting the terrestrial rays, prevents the refrigeration observed in drier air. Strachey's able discussion of observations made at Madras, point distinctly to the action of aqueous vapour on the radiation both of the sun and of the earth; while the experiments of Leslie, Hennessey, Hill, and other distinguished men, which were long considered enigmatical, are readily explained by a reference to the varying quantities of vapour with which the atmosphere is charged, on days of equal optical transparency. The interesting observations of Desains and Branley, made simultaneously on the Rigi and at Lucerne, are well worthy of mention here. The difference of level between the two stations is 4,756 feet, and within this stratum 17·1 per cent. of solar heat was proved to be absorbed. This absorption being due to aqueous vapour, is tantamount to the transmission of the sun's rays through a layer of water of a definite thickness. A sifting of the rays would be the consequence, and on *a priori* grounds we should infer that the percentage transmission through water at Lucerne must be greater than on the summit of the Rigi. This was the exact result established experimentally by Desains and Branley. H. Wild, the distinguished Imperial Astronomer of St. Petersburg, basing his statement on experiments made by himself according to Tyndall's method, has expressed the opinion "that meteorologists may, without hesitation, accept this new fact in their endeavours to explain phenomena which hitherto have remained more or less enigmatical." The correctness of this statement is illustrated by the foregoing examples, to which, if necessary, many more might be added.

At the recommendation of the Committee on Solar Physics of the Science and Art Department, a grant of 350*l.* was made from the Society's Donation Fund to Captain Abney and Mr. Lockyer in aid of their proposed observations of the total eclipse of the sun at Thebes in May last. Unfortunately the state of Captain Abney's health precluded his taking part in the expedition; but Dr. Schuster generously undertook the conduct of his observations, and, notwithstanding the short time remaining for preparation, he carried them out in the most satisfactory manner.

Three photographs of the corona itself were obtained during the eclipse. They show that the corona had the characteristic features observed during the time of the maxima of sun-spots. The long streamers in the plane of the ecliptic seen during sun-spot minima were absent, and the corona showed much disturbance. A bright comet appeared in all the photographs at a distance slightly less than a solar diameter.

A complete photograph of the spectrum of the prominences and the corona was for the first time obtained. The prominences give a spectrum in which the lines of calcium bear a conspicuous part by their intensity. The ultra-violet hydrogen lines, photographed in star spectra by Dr. Huggins, were seen, as well as a number of unknown lines.

The corona gives a very complicated spectrum. Close to the limb of the sun the spectrum was so nearly continuous and so strong as to hide any lines which might have been present. Further away the continuous spectrum fades off, the solar group G appears as an absorption line, and a large number of coronal lines hitherto unobserved appear in the ultra-violet.

In addition to these photographs one was obtained in a camera, in front of whose lens a prism was placed without a collimator. This photograph allows us to study the spectra of different prominences. As the picture was produced on one of Captain Abney's infra-red plates, all the tints of the prominences ranging from the ultra-red to the ultra-violet made their impressions, and some interesting differences in the spectra of different prominences can be noticed.

But, beside taking part in this expedition, Mr. Lockyer has continued with unwearied perseverance his observations on the spectra of solar prominences and spots, and has recently combined with these the results obtained by him during the late eclipse. During this eclipse he made naked eye observations, which he considers to be of a crucial character between the two rival hypotheses regarding the nature of the sun's atmosphere. The results of this investigation have in his opinion considerably strengthened the views which he first put forward in 1873 on the constitution of the solar atmosphere. A statement of these views will be found in a paper by him recently read before the Society.

In the present state of the questions there raised, it must I think be admitted that, after giving all due weight to the facts and reasonings adduced by Mr. Lockyer, additional and varied observations are greatly to be desired; and that no opportunity reasonably available, for adding to our knowledge of the subject, should be neglected. And, therefore, without committing myself or the Society to the support of any particular proposal or expedition, I think it may be fairly claimed as a *primâ facie* duty on the part of the present generation to obtain as many faithful records of the various phenomena occurring during solar eclipses as possible.

From a discussion of the meridian observations of Mars made during the favourable opposition of 1877, at Washington, Leiden, Melbourne, Sydney, and the Cape, Prof. Eastman has deduced the value 8″·953 for the solar parallax—a value which, though considerably larger than any of those found by other methods, agrees closely with that obtained by Mr. Downing, in 1879, from the meridian observations of Mars at Leiden and Melbourne, as well as with the values found from similar observations in 1862. In this investigation, Prof. Eastman rejects the observations at Cambridge, United States, as they were made in a slightly different manner, and gives (in combination with Melbourne) a very large value for the solar parallax, viz., 9″·138.

The detailed account of the British Observations of the Transit of Venus, 1874, was published at the beginning of the year, and the observations of the transit made at colonial observatories have been recently printed in the Memoirs of the Royal Astronomical Society.

The Transit of Mercury last November was well observed in Australia and other places, and the results are of special interest in connection with the late Transit of Venus. The discordances in the times of internal contact recorded by different observers seem to show that such observations are subject to much uncertainty.

An important memoir on astronomical refraction has been lately published by M. Radau, who, after a discussion and comparison of previous theories, gives formulæ and tables for refraction, in which allowance may be made for difference in the rate of decrease of temperature with the height above the earth's surface at different seasons of the year. M. Radau also discusses the case in which the surfaces of equal temperature in the atmosphere are inclined to the earth's surface.

A new map of the solar spectrum, containing a much larger number of lines than are shown in Ångström's classical normal spectrum, has been published by Prof. Vogel in the publications of the new Astrophysical Observatory at Potsdam. In this work Prof. Vogel has bestowed great care on estimates of the breadth and intensity of each line. In the same volumes are given the results of Prof. Spörer's sun-spot observations at Auclam from 1871 to 1879, in continuation of those for the years 1861 to 1870, previously published. From a comparison of the rotation-angles for 78 spots with the formula, Prof. Spörer finds that the larger deviations are always towards the west, indicating that a descending current has brought down with it the larger velocity of the higher regions of the sun's atmosphere. The law previously deduced by Prof. Spörer, that, about the time of minimum, spots commence to break out in high latitudes, and that the zone of disturbance gradually approaches the equator till at the minimum it coincides with it and dies away, to be replaced by a new zone in high latitudes, is confirmed by the recently published Auclam results, comprising (with Carrington's series) two complete spot-cycles.

In astronomical photography an important advance has been made by the successful application of the new processes to the nebulæ as well as to the comets. Prof. Henry Draper and Mr. Common have obtained photographs of the great nebula in Orion, showing considerable detail, and Mr. Huggins and Prof. Henry Draper have succeeded in photographing its spectrum. Mr. Huggins finds in his photograph a very strong bright line in the ultra-violet at wave-length 3730, in addition to the four nebular lines previously discovered by him in the visible portion. Prof. H. Draper's photographs do not show this bright line, though they have faint traces of other lines in the violet, and he thinks that this may be due either to the circumstance that he had placed himself on a different part of the nebula or to his use of a refractor with glass prism, while Mr. Huggins used a reflector and Iceland spar prism. The most striking feature of Prof. Draper's photographs is perhaps the discovery of two condensed portions of the nebula (just preceding the Trapezium) which give a continuous spectrum.

Prof. Schiaparelli has recently called attention to a peculiar feature on the planet Mars. In 1877 he remarked a number of narrow dark lines, which he called "canals," connecting the dark spots or so-called "seas" of the southern and northern hemispheres. He now finds that these lines are each doubled, so that according to his view the equatorial regions of Mars are covered by a network of pairs of parallel straight lines. It is to be remarked that though the appearance of Mars as depicted by Prof. Schiaparelli differs greatly from previous representations, indications of these double "canals" are to be found in the sketches of other observers.

The two bright comets of this year possess more than usual interest. The bright comet discovered at Boston by Wells, on March 18th, was the first comet since the spectroscope was applied to these objects, which presented a spectrum unlike the hydrocarbon type common to all other comets which appeared since 1864. The eye observations, as well as its photographic spectrum (taken by Mr. Huggins), showed an absence of the hydrocarbon spectrum, which was replaced by a brilliant continuous spectrum and bright lines, including those of sodium.

In September, a very brilliant comet appeared near the sun. It seems to have been discovered independently by Ellery, at Melbourne, Finlay at the Cape, Mr. Common in this country, and also by Thollon and Cruls. This great comet has been a brilliant object in the early mornings during the past two months. On September 17th, an observation, apparently unique in the history of astronomy, was made by Mr. Gill at the Cape, who watched the comet right up to the sun's limb. It could not, however, be detected in the sun, and this circumstance of appearing neither bright nor dark when in front of the sun, appears to suggest a very small substantiality, or great separation of the cometary matter. After perihelion it presented a magnificent appearance, having a tail 30° long, and even on October 30th the tail covered a space greater than the mean distance of the earth from the sun.

On October 9th, Prof. Schmidt discovered a nebulous object not far from the great comet, the orbit of which strongly suggests a connection in the past with the great comet. This fact is of more interest when the orbits of the great comet of this year, of Comet I, 1880, and of the well-known comet of 1843 are compared. The very near approach of the great comet to the sun will lead astronomers to watch with great interest for its return to our system, whatever may be its destiny, to fall ultimately into the sun, or to disappear through a process of gradual disintegration. In the *Astronomische Nachrichten*, just published, Prof. Pickering, who has computed the elements of the orbit of this comet, states, "I believe the deviation from a parabola to be real, although the corresponding period may be very long. These differences seem to indicate that the disturbance suffered by the comet in passing through the coronal region could not have been great."

This comet presented a spectrum similar to that of Comet Wells, but while receding from the sun, the bright lines of its spectrum became fainter, and then the usual hydrocarbon spectrum made its appearance. This observation, taken in connection with those of the previous comet, suggests a modified condition of an essentially similar chemical constitution. The phenomena would admit more easily of explanation if the cometary light is supposed to be due to electric discharge as it is well known how preferential is the electric discharge when several substances are present together in the gaseous form.

Before leaving this subject, I venture to quote the following passage from the *Observatory*, which puts in a very clear form the speculations now current, on the relation of the present great comet to that of 1880, 1843, and possibly 1668.

"The physical appearance of the comet, which like that of 1843, and unlike that of 1880, showed at first a decided nucleus, together with the intimation of a period very considerably greater than that of the interval from 1880, January 27, the date of perihelion of the 1880 comet, suggest that perhaps the 1843 comet suffered disintegration when at its nearest approach, and that the 1880 comet was a portion of its less condensed material, whilst the body of the comet with the principal nucleus, suffering less retardation than the separated part, has taken two and a-half years longer to perform a revolution. The remarkable discovery made by Prof. Schmidt, of Athens, on October 8, of a second comet only 4° S. W. of the great comet, and having the same motion, would seem to confirm this view."

The scientific year now concluded has not been so fertile as its predecessor in the initiation of great national and international undertakings, neither have any of those larger enterprises which I took occasion to mention last year, such as the circumpolar observations, or the Transit of Venus Expeditions, as yet been brought to their final issue. Nevertheless, in some of them we have evidence that good work is already being done, and in the others, of which we have as yet no information, there is no reason to doubt that the same is the case. Nor again, in the border-land between science proper and its applications, have I to record anything so important as the Paris Electrical Exhibition. That Exhibition, however, bore legitimate fruit in the Electric Lighting Exhibition at the Crystal Palace, and in the technical experiments lately carried out on a large scale at Munich. Perhaps the most prominent feature of the Crystal Palace Show was the incandescent light. At Paris that mode of illumination appeared to be little more than a possibility, in London it had become an accomplished fact. The importance attaching to this advance in electric lighting may be measured both by the rapid extension of its use, and also by the fact that not a few of our leading minds consider that the incandescent lamp is the lamp of the future, not merely for domestic, but even for many other public purposes.

But in another way the present year has witnessed the most important step which could have been taken for the promotion of electric lighting in this country. The Legislature has passed the Electric Lighting Bill, and, so far as legislation can effect the object, it has brought electricity to our doors. Up to this time installations of greater or less magnitude had sprung up sporadically in many parts of country, in railway stations, manufacturing works, and occasionally in private houses. But, compared with the lighting of a whole town, or even of separate districts of a large city, even the most important of these must be confessed still to partake of the nature of experiments; experiments, it is true, on a large scale, and, as I believe, conclusive as to the ultimate issue. Indeed, by multiplication of machines it is certainly, even now, possible to increase the lighting power to any required extent; but this can hardly be regarded as the final form of solution of the problem, inasmuch as such a method would be as uneconomical as it would be to use a number of small steam-engines instead of a large one. And when we consider that at the time of the passing of the Act in question, there was but one machine actually constructed which was capable of illuminating even one thousand incandescent lamps (I mean that of Edison), we cannot but feel that much remained to be done before the requirements of the public could be fully met. I do not mean thereby to imply that the Act was passed at all too soon; on the contrary, it has already given just that impetus which was necessary for producing installations on a larger scale. In illustration of this, I cannot help mentioning, as the first fruit of the impetus, a remarkable machine, by our countryman Mr. J. E. H. Gordon, which appears capable of feeding from five to six thousand lamps.

But beside the impulse above described, the Bill will have a scientific influence perhaps not contemplated by its original promoters. Under this Act, for the first time in the history of the world, energy will come under the grasp of the law, will become the subject of commercial contracts, and be bought and sold as a commodity of everyday use. It is, in fact, far from improbable that the public supply of electricity will be reckoned and charged for in terms of energy itself. But whether this be literally the case or not, a measurement of energy must lie at the root of every scale of charge.

And, further, since the Act allows no restriction to be placed upon the use of the electricity so supplied, it follows that it may be used, and undoubtedly will be used, at the pleasure and convenience of the customer, either for lighting, or for heating, or for mechanical, or for chemical purposes. This being so, it is clear that the public must by this process become, practically at least, familiar with the various modes of the transformation of force; and the Act in question might, from this point of view, have been entitled An Act for the better Appreciation of the Transformation of Force.

While offering to the public this new commodity, electricians may, in one respect, especially congratulate themselves, namely, that their article is incapable of adulteration. An electric current of a given strength and given electro-motive force is perfectly defined, and is identically the same whether it comes from a Siemens or a Gramme, from a magneto- or from a dynamo-machine, or as suggested by an eminent counsel before the Select Committee of the House of Commons, from one machine painted red or from another painted blue

It has been said, and perhaps with truth, that the electric light will be the light of the rich rather than that of the poor. But in more ways than one electricity may now become the poor man's friend. The advantages in avoidance of heat and of vitiated atmosphere in workshops and factories have often been pointed out, and may ultimately become an important factor in the physical growth and prosperity of our population. But besides this, when electricity is literally brought to our doors, it will become possible, by converting it into motive power of limited extent, to revive some of the small industries which during the last half century have been crushed by the great manufacturing establishments of the country. There are operations which are capable of being carried out by the wives and families of workmen; there are works of small extent which can be performed more advantageously in a small establishment than in a large one, and it can hardly fail to be a gain to the community if this new departure should give fresh opportunities for the development of our industry in these directions.

The Copley Medal has been awarded to Prof. Arthur Cayley, F.R.S., for his numerous profound and comprehensive researches in Pure Mathematics.

One Royal Medal has been awarded to Prof. William Henry Flower, F.R.S. During the last thirty years Prof. Flower has been actively engaged in extending our knowledge of Comparative Anatomy and Zoology in general and of the Mammalia in particular.

His Memoirs on the Brain and Dentition of the Marsupialia published in the *Phil. Trans.* for 1865 and 1867, established several very important points in morphology, and finally disposed of sundry long-accepted errors.

His paper "On the Value of the Characters of the Base of the Cranium in the Carnivora" (1869), and numerous memoirs on the Cetacea, are hardly less valuable additions to zoological literature.

Prof. Flower has been for more than twenty years Curator of the Museum of the Royal College of Surgeons, and it is very largely due to his incessant and well-directed labours that the museum at present contains the most complete, the best ordered, and the most accessible collection of materials for the study of vertebrate structure extant.

The publication of the first volume of the new Osteological Catalogue in 1879, affords an opportunity for the recognition of Prof. Flower's services in this direction. It contains carefully verified measurements of between 1300 and 1400 human skulls, and renders accessible to every anthropologist a rich mine of craniological data.

The other Royal Medal has been awarded to Lord Rayleigh, M.A., F.R.S.

The researches of Lord Rayleigh have been numerous, and extend over many different subjects; and they are all characterised by a rare combination of experimental skill with mathematical attainments of the highest order.

One class of investigations to which Lord Rayleigh has paid much attention is that of vibrations, both of gases and of elastic solids. The results of most of these researches are now embodied in Lord Rayleigh's important work on the "Theory of Sound," a work which not only presents the labours of others up to the time of writing in a digested and accessible form, but is full of original matter.

The subject of vibration naturally leads on to a mention of other hydro-dynamical researches. Lord Rayleigh has investigated the motion of waves of finite height, and in particular has shown that the "great solitary wave" of our late Fellow, Mr. Scott Russell, has a determinate character; and he has investigated the circumstances of its motion to an order of approximation sufficient to apply to waves of considerable height.

Lord Rayleigh has examined more fully than had previously been done the theory of diffraction gratings, and the effects of irregularities; and also investigated the defining power of optical combinations, and its limitation by diffraction and spherical aberration.

He has lately been engaged in the elaborate re-determination of the B.A. unit of electrical resistance.

The Rumford Medal has been awarded to Capt. W. de W. Abney, R.E., F.R.S. Capt. Abney has contributed largely to the advancement of the theory and practice of photography by numerous investigations. In the Bakerian Lecture for 1880 he has given an account of a method by which photography can be extended to the invisible region below A, which had been hitherto but very imperfectly examined by means of the thermopile.

Making use of plates prepared with silver bromide in a particular molecular condition, Capt. Abney, by means of a diffraction grating containing 17,600 lines to the inch, constructed a detailed map of the infra-red region of the solar spectrum extending from A down to λ 10,650 (Plate XXXI. *Phil. Trans.*, 1880). The lowest limit of this map was fixed by conditions of the diffraction-apparatus, and not by a falling-off of the sensitiveness of the plates at this low point; for, when a prismatic apparatus was used, photographs were obtained which show a continuous spectrum down as far as λ 12,000.

In a subsequent paper (*Phil. Trans.*, 1881, p. 887), Capt. Abney, working with Lieut.-Col. Festing, R.E., applied this new extension of photography to a research on the influence of the atomic grouping in the molecules of the organic bodies on their absorption in the infra-red region of the spectrum. The authors believe that their results indicate, without much doubt, that the complex substances they examined can be grouped according to their absorption spectra, and that such grouping, as far as their experiments go, agrees on the whole with that adopted by chemists. They have more confidence in their results, as they were careful to select such bodies as might be regarded as typical; but, of course, much patient labour of many, for a long period, will be necessary before this new branch of physico-chemical research can be regarded as fully established in any complete form.

Capt. Abney has since carried on his work in this new region of the spectrum at different elevations during a recent visit to Switzerland.

The Davy Medal has been awarded to D. Mendeleeff and Lothar Meyer.

The attention of chemists had for many years past been directed to the relations between the atomic weights of the elements and their respective physical and chemical properties; and a considerable number of remarkable facts had been established by previous workers in this field of inquiry.

The labours of Mendeleeff and Lothar Meyer have generalised and extended our knowledge of those relations, and have laid the foundation of a general system of classification of the elements. They arrange the elements in the empirical order of their atomic weights, beginning with the lightest and proceeding step by step to the heaviest known elementary atom. After hydrogen the first fifteen terms of the series are the following, viz.:—

Lithium	7	Sodium	23
Berylium	9·4	Magnesium ...	24
Boron	11	Aluminium ...	27·4
Carbon	12	Silicon	28
Nitrogen	14	Phosphorus ...	31
Oxygen	16	Sulphur	32
Fluorine	19	Chlorine	35
		Potassium	32

No one who is acquainted with the most fundamental properties of these elements can fail to recognise the marvellous regularity with which the differences of property, distinguishing each of the first seven terms of this series from the next term, are reproduced in the next seven terms.

Such periodic reappearance of analogous properties in the series of elements has been graphically illustrated in a very striking manner with respect to their physical properties, such as melting-points and atomic volumes. In the curve which represents the relations of atomic volumes and atomic weights analogous elements occupy very similar positions, and the same thing holds good in a striking manner with respect to the curve representing the relations of melting-points and atomic weights.

Like every great step in our knowledge of the order of nature, this periodic series not only enables us to see clearly much that we could not see before, it also raises new difficulties, and points to many problems which need investigation. It is certainly a most important extension of the science of chemistry.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The examiners for the Natural Science Tripos in 1883 are Lord Rayleigh, Mr. Vernon Harcourt (Oxford), Dr. A. M. Marshall (Owens College), Dr. R. D. Roberts, Mr. J. N. Langley, Mr. L. Fletcher (Oxford) of the British Museum, Mr. A. Hill, and Dr. Vines.

The time for the presentation of the report of the Syndicate appointed to frame regulations for the Doctorates of Science and of Letters is extended to the end of next term.

The increased work of the museums and the larger number of departments has caused an excess of expenditure over the ordinary income 3000*l.* allowed by the University, during the past year. The expenditure has included a provision of microscopes for the morphological and physiological laboratories at a cost of nearly 150*l.*, and a Bianchi air-pump for the chemical laboratory, costing 83*l.* The balance which has accrued is 804*l.* which is asked for as a special grant from the chest.

Mr. A. S. Shipley, of Christ's College, has been nominated to study at the Zoological Station at Naples for the first six months of 1883.

A Clothworkers' Exhibition of 52*l.* 10s., tenable for three years, will be awarded by means of the examination of the Oxford and Cambridge Schools Examination Board in July next. The successful candidate must be or become a non-collegiate student at Oxford or Cambridge.

There will be an examination at Gonville and Caius College, beginning on March 9, 1883, for one Shuttleworth Scholarship, value 60*l.* per annum, tenable for three years, open to medical students of the University, who are of at least eight terms' standing. The subjects are Botany and Comparative Anatomy; practical work will be given as part of the examination. The scholarship may be held with any other scholarship at the College, and a candidate may be recommended at the same time for a foundation scholarship. Particulars may be obtained from the Rev. A. W. W. Steel, Tutor of the College.

The following nominations have been made to the Electoral Board of the under-mentioned professorships, with varying tenure of office to secure due rotation:—Plumian of Astronomy: Prof. Stephen Smith (Oxford), the Astronomer Royal, Prof. Adams, Mr. Spottiswoode, P.R.S., Prof. Stokes, the Master of Caius (Dr. Ferrers), Prof. Cayley, and Mr. Todhunter. Mechanism and Applied Mechanics: Sir John Hawkshaw, Lord Rayleigh, Messrs. R. F. Martin, W. Airy, and Coutts Trotter (Trinity), the Master of Emmanuel (Dr. Phear), Mr. W. H. Besant, and Prof. Cayley. Physiology: Prof. Humphry, Prof. Huxley, Mr. J. N. Langley, Prof. Burdon-Sanderson, Dr. Vines, Dr. Pye-Smith, Prof. Paget, Prof. Stokes. Knightbridge of Moral Philosophy, Prof. Caird (Glasgow), Mr. Leslie Stephen, Mr. J. Venn, Prof. Fowler (Oxford), Prof. Hort, Prof. Seeley, Mr. Todhunter, and Dr. Campion. The Boards of Physics and Chemistry and of Biology and Geology have concurred in recommending that students who have passed in the Mathematical Tripos may be permitted to enter the second part of the Natural Science Tripos without passing in the first part. It is thought desirable to encourage mathematical students thus to take up the practical and experimental work in physics required of the Natural Science students; at present they have not time for studying the elementary parts subjects required of the latter.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, December 7.—Sir J. Lubbock, Bart., president, in the chair.—The following gentlemen were elected Fellows of the Society:—The Rev. R. Baron, F. O. Bower, T. H. Corry, O. L. Fraser, D. Houston, A. W. Howitt, H. McCallum, E. A. Petherick, S. Rous, and H. C. Stone.—The Rev. R. T. Murray showed specimens of *Althæa hirsuta*, *Vicia Orobus*, and *Phlomis fruticosa*, obtained by him last summer in Somerset.—Mr. W. T. Thiselton Dyer exhibited and explained maps illustrative of the rapid spread of Phylloxera in Spain and Portugal, observing that within the last year quite a wide area of the wine-growing districts therein were affected. He also exhibited photographs and made remarks on the Cinchona cultivation in Ceylon.—Mr. W. B. Espeut drew attention to some Kola nuts, and mentioned their remarkable sobering effects after intoxication by spirituous liquors.—Mr. G. Brook read notes on some little known Collembola and the British species of the genus Tomocerus. Tullberg refers to their occurrence in Sweden, but the four species in question have not hitherto been accorded a British habitat.—A paper by J. G. Otto Tepper was read on the discovery of above ninety species of Tasmanian plants near Adelaide, South Australia.—A contribution by Dr. W. Nylander and the Rev. J. M. Crombie was read, viz. on a collection of exotic lichens made in Eastern Asia by the late Dr. A. C. Maingay. Those enumerated were from British Burmah, China, and Japan; some are interesting as illustrative of lichen distribution, and others as new species and varieties.—Remarks on the genera of sub-family Chalcidinæ with synonymic notes and descriptions of new species of Leucospidinæ and Chalcidinæ was a paper by Mr. F. Kirby.—The Rev. R. P. Murray afterwards made some remarks on cleistogamic flowers of *Hoya carnosa*, producing fertile seed.

Institution of Civil Engineers, December 5.—Sir W. G. Armstrong, C.B., F.R.S., president, in the chair.—The paper read was "On the Sinking of two Shafts at Marsden, for the Whitburn Coal Company," by Mr. John Daglish, M.Inst. C.E.

EDINBURGH

Royal Society, December 4.—The Right Hon. Lord Moncrieff, president, in the chair.—The President, in opening the 100th session of the Society, gave a brief historical statement of its origin.—Obituary notices were read of Mr. Darwin, Prof. Emile Plantamour of Geneva, Mr. Charles D. Bell, Dr. Wm. Robertson, Sir Daniel Macnee, Mr. David Anderson of Morton, Mr. John M'Culloch, Mr. Samuel Rayleigh, and Prof. Spence.—The Rev. Dr. W. R. Smith exhibited specimens of Dr. A. Guébhardt's electro-chemical method of figuring equipotential lines, which had been sent him by the author.—The Astronomer Royal for Scotland communicated a telegram from J. R. Hind, of the *Nautical Almanac*, correcting the time of ingress of Venus upon the sun's disc.

PARIS

Academy of Sciences, December 4.—M. Jamin in the chair.—The President presented to M. Dumas the medal struck in honour of the fiftieth anniversary of his election to the Academy, and M. Dumas spoke in acknowledgment—Presentation of tome iii. of the third part of the "Recueil des Memoires, Rapports et Documents relatifs à l'Observation du Passage de Venus sur le Soleil, en 1874," by M. Dumas.—*Résumé* of measurements of the Daguerrian photographs of the Venus transit in 1874 by the French Commission, by MM. Fizeau and Cornu. About fifty selectep photographs from the four stations were measured by two observers or controlled by an equivalent operation. The 94 results represent 33,840 independent points. In a table are shown the values of the ratio of the distance between the centres to the sum and the radii for all the photographs measured.—Memoir on the vision of material colours in motion of rotation, and on the respective velocities, estimated in figures, of circles, one diametrical half of which is coloured and the other half white; velocities corresponding to three periods of their motion, from the extreme velocity to rest, by M. Chevreul.—On a letter of M. Spœrers relative to a peculiarity of solar mechanics, by M. Faye. If there were surface currents from the solar poles to the equator (as Dr. Siemens' theory requires), the spots should be carried in the same direction. But M. Spœrer's observations for twenty years, and those of Laugier, Carrington, and others, agree in showing displacement of spots in latitudes to be either *nil* or insignificant; and if there is any such tendency in spots far from the equator, it is rather towards than from the poles. The retardation observed in surface rotation towards the poles, M. Faye attributes to ascending and descending movements in the internal mass.—Notice on a new optical apparatus for the study of flexure, by MM. Lœwy and Tresca. It consists of three parts (1) at the observer's end a reticule of horizontal wires viewed by a lens, before which is a total reflection prism throwing lateral light along the optic axis; the eyepiece has also movable wires for measurement; (2) at the opposite end, an object holder, with stretched horizontal wires, illuminated; (3) in the middle, a lens with silvered surface, but transparent at the centre, and of such a focus that it reproduces in the plane of the reticule before the eyepiece, either the image of one set of wires by reflection, or that of the other by transparence.—On *rouget* or *mal rouge* of pigs, by M. Pasteur. This disease, called by Dr. Klein (London, 1878) *pneumo-enteritis* of the pig, has destroyed more than 20,000 pigs this year in the Rhone Valley. M. Pasteur considers Dr. Klein quite mistaken as to the nature and properties of the parasite, which is of figure 8 form, and like the microbe of chicken cholera, but finer, less visible, and quite different physiologically. He has found a method of protective inoculation.—Researches on the presence of nitric acid and ammonia in water and snow obtained in Alpine glaciers by M. Civiale, by M. Boussingault.—Order of appearance of first vessels in the leaves of Cruciferæ; demonstration of the distinctly basipetal ramification in these leaves, by M. Trécul.—On the connections (*enchaînements*) of the animal world in primæval times, by M. Gaudry. He gives a sketch of the first part of a projected work on this subject.—Chemical studies on maize (continued), by M. Leplay. This relates to potash and lime-bases in organic combination with vegetal acids or tissues of maize—On the gallicolar Phylloxera, by M. Henneguy.—On the pendulum, by M. Lipschitz.—Formula for determining how many prime numbers there are not exceeding a given number, by M. de Jonquières.—On a mode of transformation of figures in space (continued), by M. Vanecek.—On the transmission of an oblique pressure, from surface to interior, in an isotropic and homogeneous solid in equilibrium, by M. Boussinesq.—On the effect of oil calming the agitation of the sea, by M. Bourgois. Oil affects the breaking of the waves, but not sensibly the undulations themselves.—Method for determination of the ohn, based on the induction by displacement of a magnet, by M. Lippmann.—On the terrestrial induction of planets, and particularly on that of Jupiter, by M. Quet. The planets probably contain iron. With equality of magnetic powers, Jupiter would (next to the sun) exercise the greatest induction on the earth, because of its great volume and rapid rotation; but if its magnetic power were, *e.g.*, ten times that of the sun, variations of the compass might reveal some of the principal periods of that planet. The compass might, within certain limits, show to what point a planet is rich in iron or magnetic substances.—On the currents produced by nitrates in igneous fusion, &c. (second note), by M. Brard. He describes an electrogenerative fuel, which, in any hearth, yields both heat and electricity; and an electrogenerative hearth in which these agents may be generated with any fuel.—On a method of transformation of tricalcic phosphate into chlorinated compounds of phosphorus, by M. Riban.—On a new hydrocarbon, by M. Louise. This is named *benzylene sitylene*, $C_6H_2(C_7H_7).(C.H_3)_3$. It is got by making benzyl act on mesitylene in presence of anhydrous chloride of aluminium.—On an unalterable linseed powder prepared for poultices, by M. Lailles. The oil is eliminated.—On cerebro-spinal ganglions, by M. Ranvier.—On the microsporidia or porospermida of Articulata, by M. Balbiani.—The migrations of the puceron of red galls of the country elm, by M. Lichtenstein.—Researches on digestion in cephalepod molluscs, by M. Bourquelot.—Geological history of the syssidere of Lodran, by M. Meunier.—Reply to a note of M. Musset, concerning the simultaneous existence of flowers and insects on the mountains of Dauphiné, by M. Heckel.

CONTENTS

PAGE
ANCIENT SCOTTISH LAKE DWELLINGS. By Sir JOHN LUBBOCK, M.P., F.R.S. 145
OUR BOOK SHELF:—
Ward's "Sportsman's Handbook to Practical Collecting, Preserving, and Artistic Setting up of Trophies and Specimens" . 146
Miss Ormerod's "Diagrams of Insects Injurious to Farm Crops" 146
Picou's "Manuel d'Électrometrie Industrielle" 146
Holley's "Falls of Niagara and other Famous Cataracts" . . 146
LETTERS TO THE EDITOR:—
Priestley and Lavoisier.—C. TOMLINSON, F.R.S. 147
The Forth Bridge.—HERBERT TOMLINSON 147
Intra-Mercurial Planets—Prof. Stewart's 24·011d. Period, Leverrier's and Gaillot's 24·25d., and Leverrier's 33·0225d. Sidereal Periods Considered.—A. F. GODDARD 148
An Extraordinary Meteor.—B. R. BRANFILL 149
British Rainfall.—G. J. SYMONS, F.R.S. 149
Swan Lamp Spectrum and the Aurora.—J. RAND CAPRON . . . 149
The Aurora.—J. RAND CAPRON 149
Fertilisation of the Speedwell.—ARTHUR RANSOM 149
Shadows after Sunset.—Prof. DIER 150
Complementary Colours.—CHAS. R. CROSS 150
An Extraordinary Lunar Halo.—S. A. GOOD 150
"Lepidoptera of Ceylon."—L. REEVE AND CO. 150
THE COMET. By A. AINSLIE COMMON; FRANK STAPLETON (*With Illustrations*) 150
ILLUSTRATIONS OF NEW AND RARE ANIMALS IN THE ZOOLOGICAL SOCIETY'S LIVING COLLECTION, X. 151
THE TRANSIT OF VENUS. By the DUKE of ARGYLL; Dr. R. S. BALL, F.R.S., Astronomer Royal of Ireland; Dr. W. DOBERCK; J. L. E. DREYER, CLEMENT LINDLEY WRAGGE; W. F. DENNING; D. TRAILL; HENRY CECIL; R. LANGDON (*With Diagrams*) . . . 154
NOTES 159
OUR ASTRONOMICAL COLUMN:—
Comet 1882 *b* 161
Comet 1882 *c* 161
GEOGRAPHICAL NOTES 161
THE ROYAL SOCIETY, II. Anniversary Address by Dr. WILLIAM SPOTTISWOODE, Pres.R.S. 162
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 167
SOCIETIES AND ACADEMIES 167

THURSDAY, DECEMBER 21, 1882

DISEASES OF MEMORY

Diseases of Memory; an Essay in the Positive Psychology. By Th. Ribot. International Scientific Series, Vol. XLIII. (London: Kegan Paul, Trench, and Co., 1882.)

A WORK on such a subject as this from the pen of M. Ribot, cannot fail to be a good work, and although in the one which he has published there is not much originality either in respect of facts or of theories, it is of value as a clearly arranged account of what we know concerning the psychology of memory, united with philosophically wholesome views of interpretation.

It is first shown that the word memory, as ordinarily used, has a triple meaning: "the conservation of certain conditions, their reproduction, and their localisation in the past" (recollection). The third element here, which is most purely a part of consciousness, appears to be an element superadded to the other two. Neglecting it therefore in the first instance, the author seeks to "reduce the problem to its simplest terms, and try to discover how, without the aid of consciousness, a new condition is implanted in the organism, is conserved and reproduced; in other words, how memory is formed independently of all cognition." Here it is well shown that all analogies drawn from inorganic sources are misleading—such as the facts of insolation, photography, &c. "Conservation, the first condition of recollection, is found, but that alone; for in these instances reproduction is so passive, so dependent upon the intervention of a foreign agent, that there is no resemblance to the natural reproduction of the memory. Hence, in studying our subject, it must never be forgotten that we have to do with vital laws, not with physical laws; and that the bases of memory must be looked for in the properties of organic (? organised) matter, and nowhere else."

The first true analogy to be found is that of muscular fibre responding more feebly at first to the excitation transmitted by a motor nerve than it afterwards does when it has frequently been stimulated, allowing natural periods of repose. This is taken to be a true analogy, because in nerve as in muscle, "everywhere we perceive, with an increase of activity and proper intervals of repose, an increased power of organic functions." But even here, we think, the objection might fairly be made that the analogy is scarcely sound, inasmuch as there is no evidence to prove that the increase of power in a muscle due to use, is due to an increase in the power of the individual fibres. We think some better parallels might have been chosen from the region of muscle physiology—such, for instance, as the effect of the constant current in leaving behind it for several minutes after it has ceased to pass through a muscle a change in the excitability of the fibres, so that they are less responsive to a renewal of the current in the same direction, and more so to its passage in the opposite direction. The following paragraph, however, is in our opinion above all criticism, and should be well burnt into the memory of all who write about memory.

"The true type of organic memory—and here we enter the heart of our subject—must be sought in the group of facts to which Hartley has given the appropriate title of secondary automatic actions, as opposed to those automatic functions which are primitive or innate. These secondary automatic actions, or acquired movements, are the very basis of our every-day existence. . . . In a general way it may be said that the limbs and other sensorial organs of the adult act with facility only because of the sum of acquired and co-ordinated movements which forms for such part of the body its special memory, the accumulated capital on which it lives, and through which it acts—just as the mind lives and acts in the medium of past experience. To the same category belong those groups of movements of a more artificial character which constitute the apprenticeship of the manual labourer, and are called into action in games of skill, bodily exercise, &c."

The first requisite to the formation of these automatic movements is association, the original material being provided by primitive reflex actions, which require by frequent repetition or practice to be properly grouped, some combined and others excluded. Such organic memory resembles psychological memory in all but one point—the absence of consciousness. Thus all the following features are common to both: "acquisition, sometimes immediate, sometimes gradual; repetition of the act necessary in some cases, useless in others; an inequality of the organic memory according to individuals—it is rapid with some, slow, or totally refractory with others (awkwardness is the result of a deficient organic memory). With some, associations once formed are permanent; with others, they are easily lost or forgotten. We observe the arrangement of actions in simultaneous or successive series, as if for conscious recollection, and here is a fact worthy of careful notice; each member of the series *suggests* what is to follow."

Touching the changes produced in nerve-tissue, which constitute the objective side of memory, M. Ribot properly observes that it is scarcely safe to speculate, as they are beyond the reach of histology or of histo-chemistry, though facts in abundance prove that some such changes take place, and the probability is, as expressed in a quotation from Maudsley, that "every impression leaves a certain ineffaceable trace; that is to say, molecules once disarranged and forced to vibrate in a different way, cannot return exactly to their primitive state." But over and above this particular modification, which may be supposed to be impressed upon the molecular constitution of the nervous elements concerned in an act of memory, M. Ribot points out that there must be a "second condition, which consists in the establishment of stable associations between different groups of nervous elements." This, we think, is a most important point, and one which, in our author's opinion, has not hitherto received the attention that it deserves. In his own words, "It is of the highest importance that attention should be given to this point, viz. that organic memory supposes not only a modification of nervous elements, *but the formation among them of determinate associations for each particular act*, the establishment of certain *dynamic* affinities, which, by repetition, become as stable as the primitive anatomical connections. In our opinion, the important feature with regard to the basis of memory is not only the modification impressed upon each element, but the manner in which a number of elements group themselves together and form a complexus." Thus it follows that "a rich and extensive memory is not [merely] a collection of impressions, but [also] an accumulation of dynamical associations, very stable and very responsive to proper stimuli."

The essay then proceeds to consider more especially the case of conscious as distinguished from organic memory:—"The brain is like a laboratory full of movement, where thousands of occupations are going on at once. Unconscious cerebration, not being subject to restrictions of time, operating, so to speak, only in space, may act in several directions at the same moment. Consciousness is the narrow gate through which a very small part of all this work is able to reach us. . . . What has been said of physiological memory applies in a general way to conscious memory; only a single factor has been added." But "dynamical associations have a much more important part to play in conscious memory than in unconscious memory."

These we think are the more important of M. Ribot's preliminary considerations. We have no space to consider others which follow, or to enter into the details of those diseases of memory which constitute the main subject of his work. These diseases are classified under the divisions of General Amnesia, Partial Amnesia, and Exaltations of Memory. Each of these divisions is abundantly illustrated by examples, which, while being adduced in corroboration of philosophical views on the mechanism of memory, furnish in themselves reading of a curiously entertaining kind. We may conclude by rendering, in the words of the author's own summary, the general conclusions which he deems his study of the diseases of memory to have established:—

"1. In cases of general dissolution of the memory, loss of recollections follow an invariable path; recent events, ideas in general, feelings, and acts.

"2. In the best-known case of partial dissolution (forgetfulness of signs), loss of recollection follows an invariable path; proper names, common nouns, adjectives and verbs, interjections, gestures.

"3. In each of these classes the destructive process is identical. It is a regression from the new to the old, from the complex to the simple, from the voluntary to the automatic, from the least organised to the best organised.

"4. The exactitude of the *law of regression* is verified in those rare cases where progressive dissolution of the memory is followed by recovery; recollections return in an inverse order to that in which they disappear.

"5. This law of regression provides us with an explanation for extraordinary revivification of certain recollections when the mind turns backwards to conditions of existence that had apparently disappeared for ever.

"6. We have founded this law upon this physiological principle: Degeneration first affects what has been most recently formed; and upon this psychological principle: the complex disappears before the simple, because it has not been so often repeated in experience.

Finally our pathological study has led us to this general conclusion: Memory consists of a process of registration of variable stages between two extreme limits, the new state, the organic registration."

George J. Romanes

EASTERN ASIA

Im Fernen Osten, Reisen des Grafen Bela Szechenyi in den Jahren 1877-1880. Von Gustav Kreitner, Mitglied der Expedition. Two Vols. (Vienna, 1881.)

AFTER rambling for more than three years over a great part of Japan and China, the forerunners of Count Szechenyi's party reached the Irawadi delta in March, 1880, in such a plight that they were actually refused admission to Jordan's Hotel in Rangoon. The expedition was undertaken, not to seek the cradle of the Magyar race in Central Asia, as was given out at the time, but simply to seek distraction from a heavy domestic affliction experienced by the Count in 1876. It was organised with the disregard of economic considerations so characteristic of the open-handed Hungarian nobility, and consisted originally of four members—the Count, Balint de Szent Kotolna, philologist, Ludwig von Loczy, geologist and Gustav Kreitner, geographer. Unfortunately Balint got no further than Shanghai, where his health completely broke down. Hence the linguistic results were *nil*, notwithstanding the sensational story circulated in some American papers regarding a Magyar-speaking nomad tribe said to have been discovered in the Gobi desert These marauders were stated to have captured and condemned the whole party to death. But on overhearing them casually exchange a few words in Hungarian, the nomad chief, overcome with emotion, fell on his knees, and addressed Count Bela "in the purest Magyar," acknowledging him and his associates as their long-lost brethren, descendants of the warlike hordes, who migrated westwards ages ago, but whose memory was still kept alive in the yurts of their Asiatic kinsmen. This story throws a curious light on the analogous statements long current in popular writings touching the Irish, Welsh, and Basque-speaking Delawares, Algonquins, Guaranis, and other American aborigines. The only difference is that in these critical times such veracious accounts have no longer much chance of surviving their authors.

The expedition has found a competent historian in its geographer, Gustav Kreitner, whose chief fault is perhaps an excessive Teutonic conscientiousness, which omits nothing, and leaves little to the imagination of the reader. Hence these bulky volumes, mostly going over tolerably beaten ground, are apt to grow all the more tedious that the journey was on the whole singularly free from stirring adventures. The camp was broken into and looted during the night by some prowling Tanguts in Mongolian Kansu; a terrific sandstorm nearly overwhelmed the caravan on the skirt of the Gobi; Herr Kreitner on one occasion got entangled in the intricacies of the loess region in North China; an attempt to penetrate into the precincts of a Buddhist monastery at Batang was met by a shower of stones from the doughty but inhospitable llamas; lastly the train conveying the explorers from Prome to Rangoon narrowly escaped the flames of a burning jungle in Pegu. But there was little else to record of an exciting character, beyond the ordinary incidents, mishaps, and hardships of eastern travel.

On the other hand many opportunities were afforded for original observations on the lands and peoples visited by the expedition, which has certainly materially increased the stock of our information on oriental matters. In Yesso the Ainos were carefully studied by Herr Kreitner, whose independent testimony fully confirms this writer's views regarding the Caucasic affinities of those aborigines. "That the Ainos have nothing in common with the Japanese and Chinese is evident even from a cursory glance. The cranial formation is nobler, the forehead higher and broader, the prominent nose firmer. But it is the horizontal position of their large brown eyes that more especially assimilates them to the Caucasic type" (p. 318). A minute examination of the hair resulted in the curious discovery that its seeming abundance is due rather to its coarse texture than to its denser growth on a given square surface. In this respect it appears to be inferior even to that of the Japanese, at least on the scalp, while the body is on the other hand covered with a fur coat averaging 40 millimetres in length, and in the ratio of about 30 hairs to the square centimetre. The contradictory statements regarding the Aino complexion were shown by a practical experiment to be due to the more or less grimy state of the subjects examined. "The more I rubbed the lighter became the dark colour of the Aino, and the browner grew my hand. How often has the complexion of this race been described as darker than that of the Japanese, by those who forget to apply the test of soap and water!" (p. 296).

In this thoroughly practical spirit many other controversial points, doubts, and mystifications were cleared up. The colour of the "button" on the Mandarin's cap is commonly supposed to indicate official rank. But "such is not the case. It is a mere decoration or order. Very frequently we noticed Mandarins with the red button (first and second mark of distinction) taking his place *after* others decorated with the blue (third) or even with the gold (eighth) button" (p. 190). In the same way by a series of shrewd calculations based on a few given data it is plausibly shown that the population of China has been enormously over-estimated, and that instead of 300 or 400 millions it does not probably exceed 150,000,000, or 100,000,000 less than that of British India! (p. 556). In connection with this point, the opium question raised by over-zealous missionaries and political free-lances, is demonstrated to be a pure bogus. The practice, not always injurious, and in certain fever-stricken districts positively beneficial when kept within moderate bounds, would seem to be indulged in by not more than 850,000–900,000 altogether. The inveterate opium smokers are reduced to about 700,000, or not much more than ½ per cent. of the whole population, taking it even at its lowest estimate.

Archæologists will rejoice to hear that the famous Nestorian monument of Signan-fu, hitherto reported as "lost or missing" since the Panthay rebellion, has been re-discovered by our explorers. For a time neglected and overlooked during those terrible times, it has been recently set up in a place of honour within the precincts of a Buddhist monastery to the west of the city. Three impressions of the well-known inscription were taken, together with a copy of another which has lately been added to the reverse side of the slab, and which runs thus: "A pious Mandarin caused this stone to be restored over twenty years ago, and set up where it now stands." In the same neighbourhood a brick inscribed with the symbol of the Han dynasty was also obtained from a pagoda said to be over 2000 years old.

From Sining-fu an excursion was made to the monastery of Kum-bum, partly for the purpose of testing Huc's extraordinary account of the famous tree of Buddha. The result must be told in the author's words:—

"A few steps brought us to the chief temple. Before it stood, surrounded by a railing, the tree concerning which the Abbé Huc tells us that its leaves bear the natural impress of Buddha's likeness and of the Tibetan alphabet. We sought in vain for such phenomena. Neither image, nor letters, but a waggish smile playing about the corner of the mouth of the elderly priest escorting us. In answer to our inquiries he informed us that *a long time ago,* the tree really produced leaves with Buddha's image, but that at present the miracle was of rare occurrence. A few God-favoured men alone were privileged to discover such leaves. The last so favoured was a pious Mandarin, who visited the monastery seven or eight years ago. Next day Count Szechenyi succeeded in finding a leaf on which a rude likeness of Buddha had been etched, probably with some acid. The Lamas allow no one to pluck leaves or blossoms from the tree, and the leaves that fall are carefully collected and sold to the pilgrims as a specific against affections of the larynx. The tree belongs to the Oleaceæ, and I believe it to be Syringa L. (white lilac), which in all probability reached Europe originally from China" (p. 708).

A careful survey was made of the vast region of "yellow earth," to which a total area of at least 360,000 square miles is assigned in the Hoang-ho basin. The origin of this unstratified loess formation is assigned with Richthofen to the weathering of the rocks on the lofty Tibetan plateaux, combined with the prevailing west winds, by which the pulverised particles are wafted eastwards. From a rough calculation of the rate of the deposit, which in Shensi was found to attain a thickness of 1800 feet, a period of at least 260,000 years is supposed to have been needed to remove the detritus from the plateaux to the lowlands.

One of the most cherished objects of Count Szechenyi was to reach Lhassa from the east or north-east. But like Prejevalsky, Gill, Desgodins, and so many other recent explorers, he was baffled all along the Tibeto-Chinese frontier line from Kuku-Nor to Batang. Hence no new territory was anywhere traversed except a small district south of Batang on the road to Tali-fu. Here a fresh route was struck across the Chung-tien plateau, which occupies the extreme west of Se-chuen, within the great bend of the Kinsha-kiang. In this Alpine region several altitudes were taken, some new wild tribes were visited, but no opportunity was afforded of throwing any fresh light on the many interesting hydrographic problems which still await solution in South-East Asia. At Tatsien-lu these problems formed a chief topic of discussion with the Abbé Desgodins, who has probably more practical knowledge of the subject than any living European. The question was again approached during the now familiar route from Tali-fu to Bamo across the narrow, gorge-like valleys of the great Indo-Chinese rivers. The result of these discussions and observations is set forth in the accompanying map of China and East Tibet, which substantially adheres to the lines already laid down on D'Anville's map, prepared in 1735 on data previously collected by the Jesuit missionaries in China. Here the Sanpu appears as the upper course of the

Brahmaputra; the Great and Little Irawadi, forming the two upper branches of the main Burmese artery, are carried through the unexplored Pomi country as far as 32° N.; while the Lu-Kiang (Salwen) and Lantsan-Kiang (Me-Khong) are both traced still higher to 34° N. 92° E. within a short distance of the Murui-ussu (Yangtze-kiang) valley. Thus the basins of five of the great Asiatic streams are crowded at one point into a narrow space of less than 280 miles, where the several water partings are formed merely by a series of lofty ridges following in rapid succession between Sechuen and East Assam. Such a hydrographic disposition is of course elsewhere absolutely unparalleled, and is altogether of such a phenomenal character that it can hardly be finally accepted until the main rivers are actually traced to their respective sources.

The jealousy with which the Tibetan frontier is everywhere guarded Herr Kreitner is disposed to attribute rather to the Lhassa than to the Pekin authorities. The Chinese government is represented as possessing very little practical power in Tibet, which is gradually becoming a sort of fee simple of the Sacerdotal class. The Dalai-lama himself is a mere puppet in the hands of this priestly caste, which has set up no less than 103 living Buddhas altogether, and which now embraces two-thirds of the population of Tibet, grinding the rest to dust, and living in opulence, idleness, and profligacy on the contributions of the countless devotees who periodically visit the vast monastic establishments overshadowing the land. The whole trade of the country is monopolised by the llamas, "who buy in the cheapest and sell in the dearest market," and whose efforts are steadily directed against the intrusion of all foreign competition. These llamas are the greatest curse that ever afflicted an ignorant and superstitious people, plundering and oppressing them in their combined capacity of sorcerers, priests, traders, money-lenders, serf-owners, and landed proprietors. "No Tibetan peasant claims as his own the land he tills, or the house he builds. All is held at the will of the llamas, who eject him whenever he dares to brave their displeasure. And in the power, rapacity, and boundless authority of these priests must be sought the impassable barriers which have hitherto encircled the whole land. By them is Tibet closed to the outer world, and by them will it long remain hermetically sealed" (p. 855).

The work is abundantly illustrated by original woodcuts, which, if not always remarkable for artistic merit, are at least always to the point. It is also unfortunately disfigured by several mis-statements and inaccuracies, some of which are quite unaccountable. Thus the length of the Suez Canal is given at 80 instead of 100 English miles. The Wahhabis are brought to the west of Mecca, where they have never been seen since their overthrow by the Egyptians in 1819. Harakiri and other customs, legally abolished since the Revolution of 1868, would appear to be still practised in Japan. The Shogun is still the "Tykun," while the Mikado, representing the oldest monarchy in the world, is said to have sprung "from the Kubo (Shogun) dynasty, founded in 1603"! Shintoism is described in one place as "a Buddhist sect," and in another, although rightly called the original national religion, it is wrongly said to be now mostly superseded by Buddhism and the Confucian moral system. The upper course of the Yangtze-Kiang, we are told, is called the "Murui-ussu" by the Tibetans, who certainly do not speak Mongolian. The Tibetans themselves are stated to be called "Si-fan" by the Chinese, and at p. 831 the extraordinary statement is made that Tibet "ist leblos auf Thierwelt," the very opposite being notoriously the case.

A. H. KEANE

OUR BOOK SHELF

Die Insekten nach ihren Schaden und Nutzen. Von Prof. Dr. E. Taschenberg. Mit 70 Abbildungen. Pp. 1-300, 8vo. (Leipzig: G. Freytag, 1882.)

THIS forms the fourth volume of a German series of popular works issued under the title "Das Wissen der Gegenwart." It consists of an examination of certain insects injurious, or otherwise, in field, garden, and forest. The author is a man of scientific training, and as a specialist has acquired that practice of accuracy of statement that necessarily results from the education of a specialist. Much of the contents will prove useful to Englishmen who can read German; a portion, however, concerns insects that happily do not occur with us. The figures are mostly very good, many are excellent, a few are indifferent. We recognise most of them as reproductions, or reductions, from varied sources. The "Colorado Beetle" is introduced, and appears somewhat strangely out of place in a work that almost exclusively concerns German insects. Possibly the opportunity for indulging in a little satire (p. 124) may form sufficient excuse. But the author aims his satire at the wrong butt. He alludes to newspaper reports as to Colorado beetles having been sent over by Irish Americans, in order to spite "Englanders," but omits to suggest that the "scare" existed long before these newspaper reports.

Out in the Open. A Budget of Scraps of Natural History gathered in New Zealand. By T. H. Potts, F.L.S. (Christ Church, 1882.)

THIS little volume contains a reprint of a number of interesting papers contributed by the author from time to time to the *New Zealand Country Journal.* These chiefly relate to the ferns and birds of the country, but comprise also an account of a visit in 1878 to Hikurangi, where the Maoris were seen at home. In another paper a good account of the Kia (*Nestor notabilis*) is given. It would seem that it does not do much damage to the flocks of sheep except during periods of severe snow, when the parrots are deprived of their usual food. The work is evidently the result of a good deal of intelligent observation carried on over a number of years.

Catalogue of Mammalia in the Indian Museum, Calcutta. By John Anderson, M.D., F.R.S. Part I. (Calcutta: printed by order of the Trustees, 1881.)

THIS part contains the Primates, Prosimidæ, Chiroptera, and Insectivora of the Indian Museum, Calcutta. Till 1865 this Museum was the property of the Asiatic Society of Bengal, and a catalogue of the mammalia therein was drawn up in 1863 by the late Edward Blyth, so well known to all Indian naturalists of that period. The collection has increased enormously since, from in 1863 150 species of the four orders catalogued by Dr. Anderson to 252 at present existing in the Museum of these same orders. Extensive and important details are given about many of the more remarkable species, especially the Primates. The synonimic lists seem well worked out, and this part will have a value for the working naturalists far beyond that of a mere catalogue. We trust the second part will soon be published, and we congratulate the Trustees on the excellent work done by their superintendent.

The Microscope and some of the Wonders it Reveals. By Rev. W. Houghton, M.A., F.L.S. Fourth Edition. (Cassell, Petter, and Galpin.)

IT seems sufficient to notice the appearance of the fourth edition of this little volume, which, like so many works issued by the same firm, bears no date of its appearance.

The Flora of Essex County, Massachusetts. By John Robinson. (Salem, 1881.)

THIS enumeration of the plants of Essex county embraces, besides the Phanerogams, the Vascular Cryptogams, and the algæ (marine) and lichens among the Thallophytes. Essex County would seem to be an attractive field to the botanist. Besides open country, deep woods and numerous swamps, the Merrimac furnishes a fine fertile valley. The freshwater ponds, over fifty in number, are from four to four hundred acres in extent, and are rich in water-plants. A sub-alpine flora is to be met with, while a long sea-coast affords suitable dwelling-places for a large number of plants peculiar to such quarters. To this well compiled flora an interesting series of sketches of the lives of some of the early botanists of the district—Cutler, Osgood, Oakes, Pickering—is attached.

Catalogue of the Fossil Foraminifera in the British Museum (Natural History). By Prof. T. Rupert Jones, F.R.S. (London: Printed by order of the Trustees, 1881.)

THE Foraminifera which are in a living state to be found widely distributed in the seas of the present day, are also known to enter as fossils into the composition of several of the stratified rocks, forming in some places such vast thickness of limestone, as to command the attention of the Palæontologists. It is found somewhat difficult to draw the line between recent and fossil forms; and it would seem to be equally difficult to be sure what is a foraminiferous form and what is not. In this most useful catalogue, however, all descriptive details and all controversial questions are omitted. Eozoon appears in the list, and so also does Orbitoides. The classification adopted is that of H. B. Brady, and the species are grouped according to their local occurrence and geological age.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Aurora and its Spectrum

IN the recent correspondence in your columns on the subject of the aurora, no notice has been taken of an old observation by Anjou, in Siberia, that whenever the aurora flashed up past the moon, a halo was formed. This, with numerous other observations, which need not be detailed here, have led me to the conclusion that suspended crystals of ice have most probably something to do with the aurora; and my object in writing is to suggest to some of your readers who are well equipped with suitable apparatus, that if they could contrive to pass a glow or phosphorescent discharge of electricity through fine-falling or loosely-compacted snow, they might very possibly be rewarded by the discovery of the origin of the green and red lines in the aurora spectrum.

Mr. Capron's experiments seem to show conclusively that it is not an air spectrum, and it is also evident that the conditions of discharge in an atmosphere laden with ice crystals are very different from those in the clean vacuum tubes usually employed by experimenters.

While on the subject perhaps I may be permitted to add one small contribution to the question. I have examined most of the auroras recorded by the Meteorological Office during the last four or five years with reference to the synoptic conditions of pressure with which they are associated. The result is, that though the larger number may be grouped round a few types of pressure distribution, it is not easy to see any one constant condition.

RALPH ABERCROMBY

21, Chapel Street, London, S.W., December 18

Swan Lamp Spectrum and the Aurora

MR. J. RAND CAPRON'S experiment with the Swan lamp is very interesting; but his inference that the aurora may not be an electric discharge in the upper atmosphere because it does not show nitrogen lines in the spectrum is hardly justified by the experiment. On the contrary, the true significance of that experiment appears to be that there is a certain degree of rarefaction of the air (or vacuum) at which the nitrogen lines disappear. Such a vacuum is given by the Swan, and probably other electric incandescence lamps. According to Mr. Capron's result, when more air got into the bulb and vitiated this fine vacuum, the nitrogen lines appeared. We may say, then, that if the aurora is an electric discharge in the upper air, the rarefaction must be approximately that of a Swan lamp, if there are no nitrogen lines visible in the spectrum of the light. To study this further some one ought to examine the discharge in vacuum tubes containing air at different degrees of density.

J. MUNRO

West Croydon, December 18

The Meteor of November 17

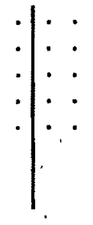
MR. CAPRON's letter (p. 149) gives an interesting confirmation of the meteoric nature of the light seen on November 17; as showing that it is physically impossible that it can be an aurora, according to accepted theories of that light. Setting aside the impossible estimate of forty-four miles, it should be noticed that the heights assigned are in close agreement, 170 miles being merely stated, like other elements in my letter, as a minimum. The oblique direction of the meteor from 10° altitude in due east to horizon in due south-west, as shown by several observations, is another evidence of its extra-terrestrial origin.

Bromley, Kent

W. M. F. P.

Invertebrate Casts

THE communication in NATURE, vol. xxvii. p. 46, induces me to state the following fact. Engaged this summer in an economic survey of the North Transcontinental Survey for the North Pacific Railroad in the camp just opposite Umatilla, near the Columbia River, Washington Territory, I observed, on June 26, the nympha of a new species of Ophiogomphus, then very common, emerging out of the water for transformation. The Columbia River had been very high, the water beginning to recede, was still more than 30 feet higher than usual. The country around the camp belonged to the so called sagebrush desert, but near the river was a bank of wet sand, flat and smoothed by the receding water. There were no plants around, and only one willow tree, now about 100 feet distant from the river, for five miles on one side and twelve on the other side. I had observed before on the sand a number of traces like the diagram. In the middle a straight furrow, and on each side two series of equidistant dots. By chance I was able to discover that these tracks are made by the nympha of Ophiogomphus (family *Gomphina* in *Odonata*). The straight furrow is made by the end of the abdomen, which is heavy and slides upon the ground. The forelegs are shorter, and make, with the end of the tibia, the inner series of dots. The other legs are longer, and make the outer series. More remarkable was it that the furrows were made in a straight line from the water to tree, as it is scarcely probable that a nympha so near its transformation can see well at a distance of about 100 feet. Nevertheless I caught the nympha just at the end of the track—which I saw made—in ascending the tree. The two outer series of dots are one inch distant one from the other. I remember having seen an account of similar tracks on fossil slabs, but I have not been able to find the publication.



H. A. HAGEN

Cambridge, Mass., November 27

The Scream of the Young Burrowing Owl sounds like the Warning of the Rattlesnake

WHILE working upon the tertiary beds of the plains east of the Rocky Mountains recently, I had numerous opportunities of making observations on the habits of those peculiar creatures the Burrowing Owls (*Speotyto hypogæa*). Among others made at the time is one relating to the extraordinary similarity between the sound of the cry of the young owl when disturbed, and that of the warning of the Rattlesnake (*Crotalus confluentus*), which I do not find to have been noticed by ornithologists. My attention was first called to the peculiar likeness by my friend, Dr. V. T. McGillicuddy, who had in his possession a couple of owlets nearly as large as the adults. The capture of a number of both snakes and birds enabled me by experiment to determine to what extent one might be deceived by the resemblance. At the distance of a few feet the shrill tremulous scream would deceive persons quite familiar with the sound of the rattling of the Crotalus. When not noticing or thinking of the birds, their cry produced on us the same effect as the sudden springing of the rattle by an angry snake. The experiments left no doubt that the cries produced a similar effect on other animals which unwittingly disturbed young owls. And in this way they led to a consideration of the possible benefit of this close resemblance, or, as it might be called by some, mimicry. As you know, the birds are fond of the deserted holes of different burrowing animals, especially so of those of various Spermophiles or Prairie Squirrels. They are common in and about colonies of the so-called "Prairie Dogs" (*Cynomys ludovicianus*), where they take possession of vacant burrows, and sometimes even of those in use, sooner or later dispossessing the rightful owners, as the dogs seem disinclined to bring eyes and noses into contact with the sharp beaks and claws in the passages however familiar they may be with the birds around the mouths of the dwellings. In the same localities the snakes are numerous, and the squirrels form a considerable portion of their prey. Naturally enough the rodents—as also the weasels, foxes, and coyotes (*Canis latrans*)—dread the fangs and venom, and recognise and profit by the warning. May it not be that the peculiar protest or scream of the young owl, by its resemblance to the danger-signal, insures safety by preventing the approach of the mammals, and, possibly, of the dull-eared snakes themselves? The scream of the old bird is rather more hoarse and somewhat less like the shrilling of the serpent. On ordinary occasions, the note of this owl is a cackling or chuckling, chatter or laugh, varied with what seem very much like imitations of the barking and squealing of the squirrels. When caught, it gives utterance to the hoarse, long-drawn, rattling scream. The owlets ate greedily of fresh meat, stopping to utter their strange cry of alarm at every attempt to approach them. In behaviour the adults were similar, but much less tractable. One, which had his wing broken, was allowed the freedom of the camp, and usually he stowed himself under the waggon. A halt in a "dog-town" one day brought him near one of the holes, which after a time he discovered. At once his soldierly walk quickened; it became a quick step as he neared the opening. Chuckling to himself, down into the darkness he plunged, and that was the last we saw of him. S. GARMAN

Cambridge, Mass., U.S.A., December 3

Fertilisation of the Common Speedwell

IF Mr. Ransom will refer again to my letter, he will see that it was written in order to draw attention to the *adaptation* of the flower for cross-fertilisation, and not especially to the *fact* that Diptera in settling upon it, draw down the stamens. This latter, if we consider the close attention paid of late years to the commoner European wild flowers, has in all probability been frequently observed before. As I have not seen Schenck's handbook, I would be glad if Mr. Ransom will quote the passage to which he refers. On looking at my note-book I find that not only *V. officinalis*, but also *V. Chamædrys* and *V. Beccabunga* are shown as fertilised in the same manner. May I suggest that the separation of the stamens, and the difference of inclination between stamens and pistil, have been brought about in order to prevent self-fertilisation? The looseness of the corolla would then, in such a flower as *V. Chamædrys*, bring the anthers to a level with the stigma when an insect alighted upon it, and would thus promote cross-fertilisation. From want of more extended observations, however, I could not say what would happen in the case of a proterogynous species, or of such a flower as *V. spicata*. In reply to Mr. Ransom, I may add that I have nowhere stated *V. officinalis* to possess larger flowers than *V. hederæfolia*, and that Mr. Darwin ("Cross and Self-fertilisation," p. 369), in a brief reference to the genus, simply states that *V. agrestis* is self-fertilising, and mentions species of Syrphidæ as visiting the flowers of *V. hederæfolia* and *V. officinalis*.

A. MACKENZIE STAPLEY

The Owens College, Manchester, December 15

Complementary Colours at the Falls of Niagara

IF Mr. Cross, whose letter on the above subject appears in Nature (vol. xxvii. p. 150), will make what is for him a very short excursion from Boston to Niagara, he will see a very perfect and permanent illustration of contrast-colours. In the American fall, the pure, green, even sheet of water is "trimmed," as it were, at regular intervals by broad bands of foam, which although, of course, really white, appear of a delicate rose-pink hue. I noticed, and "made a note of this" ten years ago, and again this year. The effect heightens the beauty of the beautiful fall, and I am surprised that no poet has made capital out of it. I should like to call attention to the rapidity with which the Canadian fall is deepening its horse-shoe. An immense mass broke off near the middle of the curve in October, 1874 (many windows in the adjacent museum were broken by the concussion), and altogether the fall has receded twenty-four feet in ten years. H. G. MADAN

Eton College, December 15

M. DUMAS

THE following is a translation of the addresses delivered in the Paris Academy of Sciences on the 4th inst., on occasion of a commemorative medal being presented to M. Dumas:—

The President, M. Jamin, said: Gentlemen and dear Fellow-Members: The Academy considers it a duty to celebrate the golden wedding of those fellow-members who have honoured it during half a century, a duty which is always dear to us, but to-day is dearer than ever; for M. Dumas now completes his fiftieth Academic year. You have had prepared, by an able artist, a medal which happily recalls his features, and must perpetuate them; it bears on the back this inscription:

A M. DUMAS
SES CONFRÈRES, SES ÉLÈVES, SES AMIS,
SES ADMIRATEURS.

I have nothing to add, except that it is not all his admirers, all his friends, all his scholars, but only those who sit here; the Academy has not been willing to share with any stranger the duty of a homage which it has exclusively reserved to itself. I have the honour to offer in your name, with respect, to our illustrious and venerated fellow-member, this token of our affection and of our gratitude.

My dear Teacher: If you will carry back your thoughts to the commencement of your career, you may well be content with your lot and with yourself. When twenty-two years of age, you were at Geneva; you began with Prevost, by discoveries that are still celebrated in physiology, on the urea, on the blood, and on generation. From that moment your name was known, and you acquired confidence in yourself. Then you perceived two things: the first, that physiology must be built upon chemistry, that chemistry was not made, and that it was necessary to make it; the second, that Geneva was not a large enough theatre for your projects. And so you came to Paris, having no other wealth than yourself, than your courage, than a programme resolutely determined, than the will to fulfil it, than confidence, still unconscious of the future that was promised you. Now the time has advanced, your dreams have been realised, your hopes exceeded, and you have reached the highest degree of glory a savant can conceive. Like Franklin, you may say: If I were to recommence my life, I could not seek anything better.

It is between that departure and this point of arrival that the most brilliant phase of your career is placed. Your discoveries followed one another like improvisations. The composition of ethers was unknown, you analysed them; you enunciated the law of substitutions and of the conservation of chemical types; a constant preoccupation brought you frequently to the atomic theory, that fundamental base of chemistry; and you furnished, for measurement of the density of vapours, a method so simple and so perfect that it is easy to the most unskilful; we know what light it has thrown on the study of organic compounds. But it belongs not to me to speak of your innumerable researches. The scholar may not arrogate to himself, without irreverence, the right of praise or of criticism; in presence of the teacher, he has only the right of respect.

But it is permitted him to remember, and who does not remember, the charm and the marvels of your teaching at the Athénée, at the École Polytechnique, at the Sorbonne, at the École de Médicine, at the Collége de France, at the École Centrale? Everywhere that you have appeared, and you have appeared everywhere, youth and ripe age have been drawn, held, charmed, carried away, to such an extent, that it may be said that you have even rendered more service by the vocations you have decided, than by your own proper works.

Fifty years ago, this Academy opened her gates to you; she has intrusted to you since, and ever congratulates herself for it, the formidable heritage of her illustrious perpetual secretaries. The French Academy has seated you in the chair of Guizot, a professor like yourself; but we have not been therefore jealous. They honoured you, and we did not lose you. Then comes the moment when preoccupations of another order have been imposed by your very renown; you have resigned yourself to those duties which enlarged your *rôle*, because your authority was necessary, because science mixes with all, because chemistry addresses itself to the lighting, sanitation, hygiène, and all the industrial requirements of a large city.

Circumstances have now set you free from manifold cares, and restored you to sciences and to letters. These possess you wholly; and whether it be art or industry, physics or chemistry, electricity or astronomy, it is to you people apply, it is your authority they seek. They find you ever ready for work, ever equal to the most difficult missions. When one recapitulates the work you have accomplished, the services of every kind you have rendered, the discoveries you have made, the lectures you have given in all the chairs, the literary works you have written, the ideas you have sown—all this existence, in fine, which has never known rest, one is astonished that you have not taken more than half a century to fulfil so large a programme; and when one has the happiness of seeing you and hearing you, one marvels that a half-century of labour without truce has still left you so much of youth to expend. It is because, of all human passions, that of study is the most healthy, because it leaves to the organs all their force, to the mind all its serenity—for it is wisdom.

Enjoy, my dear teacher, enjoy these fruits; all the good things that come from God have been given you without stint; genuine happiness, a health which nothing has affected, hearty good will towards all, a mental vision which has not ceased to grow; and all human recompenses have come to be superadded; an authority which makes itself felt and survives all *régimes*, a respect which disconcerts envy, and the affection of your fellow members which has prompted the gift of this medal: it is merely a small fragment of gold, but it will be precious to you, because it is amalgamated with our gratitude.

M. Dumas then spoke as follows:—

Mr. President and my dear Fellow-Members: Since my earliest steps in the way of science, the Academy has been to me the object of a reverence so profound that I cannot receive, without the most lively emotion, the inestimable present with which she honours the close of my career.

As far back as sixty years ago she gave a kindly attention to the work of my youth; half a century ago she received me into her bosom; and since then she has not ceased to accord to me marks of her esteem and of her confidence; nothing had prepared me, however, to think that among my fellow members many should wish even now to call themselves my scholars. Of all the testimonies to which an old teacher might lay claim, the secret has been found of offering that one which is dearest to his heart. Your kindness overwhelms and confounds me!

Ah, my beloved scholars, I go back often enough to these thirty years of an apostolate, which has not been sterile, thanks to the talents of disciples like you; but I believed the remembrance of it to be buried in the tomb of companions in the fight, whom we have lost, or to have passed from the memory of those who survive them. These prelections, then, of another time, of a time so happy, are still not forgotten, since you have wished to recall, in a durable way, on this medal, impressions that are ordinarily apt to be soon attenuated or even extinguished.

You are right! The Professoriate must be honoured, because speech is a power; because from the height of his public chair the professor fulfils a sacred mission. His loyal and penetrating conviction warms hearts, and raises minds towards the disinterested regions of the Ideal. He reflects the present state of science, like a faithful mirror, he prepares the discoveries of the future, he revives the grand traditions of a glorious past. Opening his whole heart and all his thought to his auditors, he teaches them to love the truth, to respect genius, to cherish the fatherland, and to serve it well.

Whoever has found himself surrounded by attentive youth, taking fire at the accents of the teacher, vibrating to his emotions, hastening full of faith towards the conquests indicated to its ardour, that man, believe me, has known the noblest enjoyments of the human soul.

But stay, there is a greater joy still; it is that experienced in seeing oneself outstripped by those to whom one formerly showed the way. This joy you have caused me to taste every day. May you, for the honour of French science, and for the moral greatness of our dear country, you who are of more value than I, have in your turn scholars who surpass you in genius, and equal you in heart.

Mr. President, and all of you my dear Fellow-Members, receive once more the profound expression of my grateful sentiments; the medal which I receive from your hands will be piously preserved by my family as the dearest of souvenirs of my existence, and by my descendants as the most honourable of titles of nobility.

THE METEOROLOGICAL OBSERVATORY ON BEN NEVIS

THE importance of high-level stations in any satisfactory handling of the scientific and practical problems of meteorology which have now come prominently to the front, is everywhere recognised, and accordingly in almost all civilised countries such stations have been established, and their number is steadily increasing. On the continent of Europe, many of the more salient positions available for high-level stations are already occupied in France, Spain, Italy, Switzerland, Austria, Hungary, Germany, and Russia; and as regards other countries, the United States, Mexico, India, and our Australian colonies, have also established stations at great elevations, in an energetic prosecution of this important department of meteorology. Singularly enough, Great Britain alone stands aloof from participation in the general movement, and notwithstanding the heavy responsibility which her geographical position and vast pecuniary interests and resources impose upon her, none of the mountains that rear their heads in the very tracks of the storms which sweep over Europe from the Atlantic, is yet occupied by either observatory or station for systematic and continuous observation of the weather, the highest station in these islands being Dalnaspidal, which is only 1450 feet above the level of the sea.

At high-level stations near the equator, where temperature varies but little throughout the year, atmospheric pressure, which may be regarded as measuring the mass of air overhead, is subject also to very small variation. Thus at Bogota, in South America, 8727 feet high, where the mean temperatures of January and July are respectively 57°·2 and 56°·2, the normal atmospheric pressure is 22·048 inches and 22·058 inches. Let us look now at the results obtained at Pike's Peak, where a first-class meteorological observatory was established by the United States Government about ten years ago, at a height of 14,151 feet above the sea. Mr. Henry A. Hazen, in a recently published paper on "The Reduction of Air-pressures to Sea-level at Elevated Stations," shows that the normal pressure on Pike's Peak is 0·632 inch less in winter than in summer. The difference is mainly due to the low temperature of winter as compared with that of summer; the reason being that the atmosphere in winter being condensed by the cold, sinks below the summit of the mountain, thus giving a lower pressure there. Now since a lowering of the temperature implies a proportionate condensation, or greater massing of the atmosphere in its lower strata, with a corresponding diminution of pressure in the upper regions, it necessarily follows that at considerable heights in the northern hemisphere the normal pressure is relatively higher in equatorial regions during the winter months, as compared with any other season of the year, than in higher latitudes at the same heights; and that generally the diminution of the normal pressure in the upper regions is in proportion to the lowness of the temperature of the lower strata. From this state of things it results that, during the colder months, the upper atmospheric currents flow northwards in greater volume, velocity, and persistency, bearing with them the higher temperature and humidity of lower latitudes. It is doubtless from the disturbing influences thus called into play, particularly the disturbing influence of the aqueous vapour from the Atlantic, that the notoriously stormy weather of the winters of North-Western Europe is to be traced.

But the fluctuations of pressure at great heights in the atmosphere are not merely seasonal changes following the annual march of temperature through the year; they also follow the changes of temperature which occur from day to day, notably those great and striking changes of temperature which accompany storms. Now it is the investigation of these changes, together with changes in the humidity, cloudiness, and motions of the atmosphere, in their relations to the cyclones and anticyclones of Europe, with the stormy and settled weather that respectively accompanies them, which give to meteorological observations made on Ben Nevis their international significance.

The observations made during the summer of 1881 on the top of Ben Nevis, in connection with the Scottish Meteorological Society, by Mr. Wragge, with an enthusiasm, physical endurance, and undaunted devotion to the work beyond all praise, have now been to some extent discussed, with the result that they amply bear out the strong opinion here advanced of their great value in forecasting weather. The time was sufficiently extended for the determination of the approximate normal differences between observations at the top of the Ben and at Fort William, near sea-level. During the unsettled weather of the summer of 1881, departures from the normal values, and these departures often large, were of frequent occurrence. Now the remarkable and frequent differences from the normals thereby disclosed in the vertical distribution of atmospheric temperature, humidity, and pressure in the aerial stratum between the top of Ben Nevis and sea-level, taken in connection with the weather that followed, give the strongest grounds for the assurance that observations made on the top of Ben Nevis would contribute invaluable aid, if directly wired to London, in framing forecasts of weather for the British Islands and North-West Europe generally. The observations also threw no little light on several controverted points respecting the movements of cirrus clouds, upper currents, and the time when the centres of storms reach higher and lower levels respectively.

The observations were resumed last summer on a more extended scale, the new observations embracing a more complete investigation into the varying states of the atmospheric stratum between the top of the mountain and the sea, by a string of intermediate stations at different heights, and by a very elaborate and carefully worked out system of ozone observations. The weather of 1882 differed materially from that of 1881, and when the observations of 1882 come to be discussed, they will doubtless yield new results in the further extension of our knowledge of weather phenomena. Among the new results may be mentioned the remarkable observations with the hygrometer in the second week of August and at the equinox. The most striking of these were the observations of September 21, when the dry and wet bulbs on the top of the mountain read as follows:—

	Dry	Wet		Dry	Wet
9 a.m. ...	49°·1 ...	39°·4	10·30 a.m. ...	51°·9 ...	39°·0
9·30 ,, ...	49·5 ...	39·7	11 ,, ...	51·1 ..	37·6
10 ,, ...	49·4 ...	37·9	11·30 ,, ..	53·7 ...	41·4

the barometer at Fort William being high at the time and nearly steady. No such relatively warm and dry air was recorded at Fort William where during the time the temperature was only from 1°·9 to 4°·6 higher than that of Ben Nevis, instead of the normal difference 15°·7. It is instructive to note that these hygrometric states of the atmosphere were observed on the top of Ben Nevis, during, or more strictly speaking, towards the termination of a rather protracted and heavy storm from the north, which rolled huge breakers on the beach of the Moray Firth, and poured down deluges of rain on the high northern slopes of the mountain range stretching from near Foyers to Huntly, which flooded the rivers to an unusual height. The unwonted warmth and dryness of the air, and the deluges of rain that fell immediately to the northward, warrant us in classing the singular phenomena recorded by Mr. Wragge on the top of Ben Nevis on the morning of September 21, as quite analogous to the föhn of Switzerland. If the supposition be a correct one, the difference between the two classes of phenomena is, that whilst the föhn of Switzerland has its origin in a saturated atmosphere discharging its superabundant vapour in deluges of rain on the southern slopes of the Alps, and after crossing these mountains, descending the northern steeps of the mountain-range as a dry warm wind, the föhn of Ben Nevis had its origin in the highly saturated air, which, advancing from the North Sea, discharged its vapour on the higher slopes looking down on the Moray Firth, and after ascending to some height, thereafter blew down on Ben Nevis as a descending wind, characterised by a dryness and relative warmth rarely felt at lower levels. The value of these observations from their important bearings on the theory of storms and other atmospheric movements, cannot easily be over-estimated by the meteorologist, and it is important to note that the observations at none of the lower stations gave indications of the ascensional and descensional movements of the atmosphere to which attention is here directed.

We observe from a circular we have before us, signed by the Duke of Richmond and Gordon, President of the Scottish Meteorological Society, that the Society has obtained from Mrs. Cameron Campbell of Monzie, a suitable site for the proposed observatory on the top of Ben Nevis, that the grounds and buildings are to be invested in the Royal Society of Edinburgh, and that the charge and management of the observatory will be in the Council of the Scottish Meteorological Society, in conjunction with two representatives of the Royal Society of Edinburgh, and one representative of the Royal Society of London, the representatives of the former Society being Prof. Tait and Prof. Chrystal, and that of the latter Sir William Thomson.

It is satisfactory to learn that a good beginning has been made towards raising the 5000*l.* required to establish the observatory, by a number of noblemen and gentlemen, who have intimated handsome subscriptions to the fund. Since, however, a large sum remains yet to be subscribed, we earnestly hope that in the interests of science the remaining balance of the 5000*l.* will soon be subscribed, so that next summer may see the Ben Nevis Observatory an accomplished fact.

NOTES ON THE GEOLOGY OF HONGKONG

WRITING in 1843, Dr. Abel determined the main structure of the island to be of basaltic trap, granite, siliceous and schistose rock. Mr. Kingsmill in 1865, in his excellent papers on the Geology of the Kwangtung Province, was the first to notice the trachytic porphyry of Victoria Peak (1823), the summit of which overlooks the town. This trachytic rock has been apparently forced upwards through the granite after the overflowing and partial hardening of the trap on the west side of the island. It was Mr. Kingsmill also who explained the nature and formation of the pseudo-boulders, with which the island is so plentifully covered. Towards the extreme south-east, near Cape d'Aguilar, these pseudo-boulders assume very large dimensions, and their weather-beaten aspect proves that the chemical action of water and plants, which forced them from the parent rock, occurred a long time ago. Indeed the island must have undergone great changes in course of time: the hill beyond Shekko, for instance, must have been originally nearly or quite as high as Victoria Peak, whereas its present elevation is not more than 500 feet. The rapid action of the heavy rains and rich vegetation is nowhere more apparent than in the high hill (directly back of the peak from which the colony takes its name) known as the Hog's Back, or High West. Its eastern slope is literally covered with pseudo-boulders, rendering the ascent from that side not a little dangerous, and in the rainy season large masses of rock are borne down into the valley beneath.

Now that the population of the island has increased, amateur geologists and mineralogists have become tolerably plentiful, and frequent excursions are made, hammer in hand, to the less known and wilder portions of the island. In this manner traces have been found of not a few minerals and several interesting rocks. Silver has been observed in small quantities, also galena, lead, and iron pyrites; slate near Aberdeen, syenite and dolorite on a cliff overlooking that one-time piratical *rendezvous*, Saiwan, feldspar and grey mica abundant.

One of the most interesting finds is that of molybdenite, near the village of Sau-ki-van. Molybdenite, molybdenum glance MoS_2, was not known hitherto to be among the mineral products of China. Germany, Sweden, and Cornwales are the chief localities for this rare mineral, and it has been found in several parts of the United States. The South China specimens show all the well-known characteristics of European molybdenite—colour, lead-grey, streak the same; thin foliated hexagonal plates, closely resembling graphite; flexible, non-elastic laminæ, H. = 1-2, G. 4, 5. A local chemist corroborated the determination by analysis, and found the composition to be—

Sulphur ... ... = 40·0
Molybdenum ... ... = 60·0

Molybdenum sulphide ... ... = 100·0

It will be seen from this analysis that there is a slight decrease in the quantity of sulphur, compared with European molybdenite. Dana gives the composition of American molybdenum sulphide as follows:—

Sulphur ... ... = 41·0
Molybdenum ... ... = 59·0

100·0

The mineral was found in small lumps imbedded in the granite.

F. WARRINGTON EASTLAKE

Hongkong, November

TRANSIT OF VENUS, 1882—BRITISH EXPEDITIONS

AN operation which requires for its success the collection of nearly simultaneous astronomical observations over widely separated portions of the earth's surface must always be liable to great risks of failure. These risks may be diminished by a careful selection of stations, and an increase in their number; but they can never be entirely removed.

The telegrams already received show, however, that the British expeditions have been most fortunate; and the success of the work is now assured.

This is not the proper place for a technical discussion of the different methods which may be adopted for the determination of the sun's distance from a discussion of observations of Venus in transit; but it is desirable that some facts should be stated which may enable the reader to form some conception of the strength of the method which has been relied upon in the organisation of the British expeditions, and the probable accuracy of the sun's distance which may be deducible from a careful discussion of the observations which have been collected.

On December 6, 2h. 20m. G.M.T., the sun was distant from the earth about 90,620,000 miles, whilst at the same time Venus was distant only about 24,330,000 miles. The ratio of these two numbers is very accurately known, but the expression of either of these two distances in terms of any unit of length which is directly known to us, as a mile, is a point of great difficulty on account of the small dimensions of our earth, of which the diameter is only about 7912 miles, in comparison with such distances as those of Venus and the sun.

The greatest possible displacement of Venus, as seen projected on the sun's disc from any two places on the earth's surface is only about a twenty-ninth part of the solar diameter. It is from such displacements that the relation between the distances of Venus and the sun and the separation of the observers, which is known in miles, is established; but the maximum displacement is never practically available.

These displacements may be measured in many different ways: we can take photographs of the sun's disc at the different stations, and afterwards measure from the photographs the distances between the centres of the planet and the sun, as seen at the different stations; or the distances between the centres may be directly measured with a heliometer or any equivalent instrument; or we may avoid the difficulties and errors which arise from the use of all such measuring instruments, use the sun itself as our circle of reference, and infer the displacements by observing the differences in time at which Venus is apparently in contact with the sun's limb, as seen from the opposing stations. The last method is that upon which reliance has been chiefly placed in the organisation of the British expedition. For the success of this method, we have to place observers at two sets of opposed stations, at one of which Venus is thrown, from the effects of perspective, towards the centre of the sun at ingress, whilst at the other set of stations Venus is thrown from the effects of perspective from the centre. The former stations are called stations of accelerated ingress, the latter those of retarded ingress.

The stations of the egress observations are chosen from similar considerations, and divide themselves into stations of accelerated egress and retarded egress. In the selection of stations, the most important points are, that the effects of the apparent displacements on the difference in the times of contact should be considerable, that the climatic conditions should be generally favourable, and that the altitude of the sun should be sufficient to render a good observation possible.

The principal stations selected for the British observations of accelerated ingress were Madagascar and the Cape; but it is hoped that good observations may have been secured by Mr. Meldrum, the director of the observatory at Mauritius, although the altitude of the sun is very low for that station at the time of contact. The observers at Madagascar were the Rev. S. J. Perry and the Rev. W. Sidgreaves, with Mr. Carlisle as an assistant. The instruments provided were excellent 6-inch equatorials. The expedition was placed under the care of Commander Aldrich, of H.M.S. *Fawn*, with instructions to establish the observers near the coast on the south-western part of the island. A telegram has been received, stating that the *Fawn* had returned to Natal, and that the observations had been perfectly successful.

The observations at the Cape have also been successful. At the Observatory there, Mr. Gill reports that seven observations of contact were made. The instruments available at this station were a fine 7-inch telescope by Merz, which belongs to the Observatory, and a new 6-inch equatorial by Grubb, sent out by the Committee for the observation of the transit, and a good Dollond, which was used at the last transit, with an aperture of nearly four inches, and Mr. Gill's heliometer. The goodness of the contact observations with the latter instrument may be open to doubt, from the construction of the instrument with a divided object-glass. Three of the contacts must, I fear, have been made with inferior instruments, but at least three good observations of contacts have been made at this station alone.

Mr. Marth, who is in charge of the station at Montagu Road, on the railway between Cape Town and Beaufort West, reports that two good observations of contact have been secured. The observers at this station were Mr. Marth and Mr. C. M. Stevens, with Corporal Thornton as assistant. The instruments provided were a 6-inch equatorial by Grubb and the fine Dallmeyer instrument which was kindly lent by Dr. Warren de la Rue for the observation of the transit.

At Aberdeen Road, Mr. Finlay, B.A., First Assistant at the Cape Observatory, and Mr. Pett, Third Assistant, were the observers. The instruments were 6-inch equatorials by Simms, provided by the Committee; a marine artilleryman, Gunner Shean, was sent out from England with the instruments, and was attached to the party as an assistant. The observations have been perfectly successful, and the definition is reported to have been fine.

Mr. Neison has also observed the contacts at Durban, Natal, with a fine equatorial provided by the liberality of the colonists for the observations. Therefore we have at least ten first-rate observations of the internal contact at the phase accelerated ingress made upon one uniform plan and with instruments of the same class.

The longitudes of all the Cape stations have been directly connected with that of the Cape Observatory by telegraph, and the longitude of that station has recently been connected by telegraphic determination with Greenwich.

The Greenwich times of the phase of accelerated ingress range from about 2h. 11m. 0s. for Madagascar to 2h. 12m. 8s. for the Cape Observatory. The Greenwich mean time for the general body of observations known to have been secured would not differ greatly from 2h. 11m. 48s.

But the observations made of this phase would have been perfectly useless unless observations for comparison with them had been made at stations of retarded ingress.

The stations selected for the observation of retarded ingress were Jamaica, Barbadoes, and Bermuda. But the Canadian Government also provided three 6-inch telescopes; and one of their observers, Lieut. Gordon, Director of the Observatory at Toronto, came over to England to secure the necessary additions to the instrumental means, and to Oxford to make himself acquainted with the arrangements of the other British stations. It is to be feared that this spirited conduct on the part of the Canadian Government has not been followed by the success which could have been wished; but no official reports have yet been received from the Canadian stations.

The phase of retarded ingress has been successfully observed by all the observers sent out in the British expeditions, and the observations, from the telegrams received, appear to have been perfectly satisfactory.

The observers at Jamaica were Dr. Copeland and Capt. Mackinlay, R.A. Mr. Hall was to have observed the contact at another part of the island away from telegraphic communication, and he has not yet reported.

The observers at Bermuda were Mr. Plummer, Lieut. Neate, R.N., and Capt. Washington, R.E.

The observers at Barbadoes were Mr. Talmage and Lieut. Thomson, R.A.

We have therefore, at these stations alone, seven good observations of retarded ingress.

The Greenwich times of the phase of retarded ingress range from about 2h. 22m. 35s. at Barbadoes, to 2h. 24m. 25s. at Bermuda. The Greenwich mean time for all the observations will not differ greatly from 2h. 23m. 33s., and the available difference between the opposed stations of accelerated and retarded ingress will therefore be about 700s., and an error of five seconds in the determination of the difference of the observers would not give rise to an error of 700,000 miles in the determination of the sun's distance, because 5 seconds is only the 140th part of the available interval. But even if the separate results at a station should occasionally disagree, 10 seconds of time *inter se* there is no reason whatever why the mean difference in time between the opposed stations derived from seven good contacts at one end, and ten at the other, should have an error of three seconds of time. So that from the British observations of ingress alone it should be possible to estimate the sun's distance within 300,000 miles.

The stations at Bermuda, Jamaica, and Barbadoes, which served for retarded ingress, are also available for accelerated egress.

The egress observations at Jamaica and Barbadoes are reported as satisfactory. Those at Bermuda were apparently only picked up through clouds. It is possible, therefore, that the Bermuda observations may not be available, on this account, for combination with the other observations; but with the Jamaica and Barbadoes observations alone we have at least four good contacts.

These accelerated egress contacts were made, roughly, about 7h. 47m. Greenwich mean time.

Corresponding to these, we have for the phase retarded egress the New Zealand observations and the observations by Ellery and his staff at Melbourne. The observers at New Zealand were Lieut.-Col. Tupman, R.M.A., and Lieut. Coke. The internal contacts must have been made about 8h. 0m. 30s. G.M.T.

Observations of this phase which were secured by Mr. Ellery and his assistants at Melbourne must have been made about 8h. 1m. 30s. G.M.T.

The failure of the Brisbane observations through clouds and the partial failure at least of the Bermuda observations at egress have considerably weakened the weight of the determination of the sun's distance from the egress observations. But the observations secured with the large available difference of time of about 840s. should most certainly give a determination of the sun's distance from the egress observations alone with an error less than 500,000 miles.

Besides the above observations, Capt. Wharton, H.M.S. *Sylvia* has been provided with two good telescopes; and, if the weather has been favourable, will have secured observations both of the ingress and egress, having established himself at some station on the South American continent, not far from the Falkland Islands.

The Greenwich mean times of internal contact at Capt. Wharton's station may be taken at about 2h. 15m. at ingress, and 7h. 52m. at egress. The computed times for the contacts at Capt. Wharton's station would be but little affected by any error in the assumed mean distance of the sun, but they are influenced as much as the other stations by any error in the assumed distance between the centres of Venus and the sun at which the contacts take place. Observations, therefore, at such a station are of importance as a check upon the results obtained from the comparison of results from stations of greatly accelerated and retarded phase.

The longitudes of stations in Jamaica and Barbadoes have already been connected with Greenwich through Washington by the American observers by means of telegraphy. Lieut. Neate has determined the longitude of Bermuda through Washington, by the conveyance of chronometers between Bermuda and New York, where Washington time is available. Arrangements have been definitely made for Lieut. Darwin, R.E., to connect Port Darwin with Singapore, and thus the telegraphic longitude of the Australian and New Zealand stations, which have already been connected together, will be determined.

The longitude of the Madagascar station has been determined by the conveyance of chronometers between Durban, in Natal, and Madagascar, the sea rate of the chronometers being ascertained by their rates during the voyage between Durban and Cape Town.

It will be seen, therefore, that there will be no difficulty in the discussion of the observations from the want of accurate knowledge of the position of the observing stations.

The observations of the British Expeditions have been made by observers of skill, with excellent instruments, under approximately similar conditions of illumination and with sufficient optical powers. The observers have all been trained to observe the same kind of contact, and that one of so distinctive a character that no doubts about the time record which refers to the kind of contact required for comparison with those made at other stations should be possible. This point is one of the utmost importance. In all attempts to determine the sun's distance from these contact observations we have to assume that the "contacts observed" took place with the *same* angular separation of the centres of Venus and the sun as seen from the observers' position on the earth's surface. There is no reason whatever why this assumption should be true unless the "contacts observed" are contacts of the same class. There is an interval of more than 20m. between the "external contact" at a station and the "internal contact" at the same station. If, therefore, any one should combine the time of external contact at one station with the time of internal contact at another station, without allowing for the motion of the earth and Venus, in the interval of about twenty minutes he would obtain a startling but very erroneous result for the sun's distance. The error thus indicated would, however, differ nothing in kind, but only in degree from those which have, to some extent, unfortunately, brought this method of contact into doubt.

The success of the British observations, particularly at ingress, has, however, been so complete, that the method of contact will now have a fair trial.

I await the result with perfect confidence. Neither the method of contact nor any other known method can, with our present instrumental means, settle the sun's distance to a hundred thousand miles. But the extreme range of possible uncertainty, as shown by the difference between the results obtained from Mr. Gill's heliometer measures of Mars east and west of the meridian at the opposition of 1877, and those obtained from the differences in North Polar distances between Mars and stars on the meridian as observed at our principal northern and southern observatories at the oppositions of 1862 and 1877, is about 1,700,000 miles. All our other recent determinations, which have stood the test of examination, fall within these limits, and do not generally differ much from 92,000,000 miles. The contact observations of the British expedition will, I feel confident, fix the true distance, without any greater error than 300,000 miles, and should settle the question whether either of the extreme values mentioned can be the true distance, or whether their mean is not much nearer the truth than either of them.

E. J. STONE

We have received the following additional communications on the transit:—

THE observation of the transit of Venus here to-day was attended with a remarkable, and I think hitherto unnoticed phenomenon.

When the planet had entered nearly one-half its diameter on the solar disc, its contour was barely traceable outside by the faintly luminous line of light noticed by previous observers. But in addition to this a spot of light extending through nearly 30° of the planet's circumference, and from its periphery *inwards* for about one-fourth of the radius was distinctly seen. The brightness appeared greatest at the outside, and faded toward the centre. This appearance was noted by me through the great equatorial, by the aid of a polarising eye-piece, and a magnifying power of 244. The position-angle of the bright spot was approximately 178°, as estimated by me (for, owing to the fact that the polarising eye-piece has no position-circle, only an estimate was possible).

At the same time an assistant (Mr. J. E. Keeler), observing with a telescope of only $2\frac{1}{4}$ inches aperture and a power of 70, was able to see the same bright spot quite independently, and estimated its position-angle at 168°. The position-angle of the planet itself on the solar disc was approximately 147°. The bright spot was therefore distinctly on one side of a line passing through the centres of the sun and Venus.

The observation was repeated at intervals through passing clouds for seven or eight minutes, and whatever may be its interpretation, of the fact of observation there can be no question.

There would seem to be no analogy between this very peculiarly disposed and definite bright spot upon the planet's edge, and the small central spots described by some (and whose existence is denied by others) which have been seen on Mercury and Venus in transit, when they have completely passed on to the disc.

S. P. LANGLEY,
Director of the Observatory
Allegheny Observatory, Allegheny, Pennsylvania,
December 6

THE transit was observed here in a cloudless sky up to sunset, but the low position and great atmospheric disturbance rendered measurements and observations of contact unreliable.

When Venus was half in on the sun, I distinctly perceived a fine curved thread of subdued light on the south-eastern edge outside the sun, and not reaching to the latter, nor extending far on any side. With three-fourths on, the thread of light reached round the remaining fourth outside, and completed the periphery. The segment of light disjoined, as when first observed, would seem to indicate a superior refractive power of the planet's atmosphere in the locality at the time.

A short time before complete ingress, the solar cusps appeared to project out from the disc in double concave forms to join the aureole. The aureole disappeared after complete ingress, but the outer portion of the planet seemed much less dark than the central, which was perfectly black within a dark brown ring of from 5″ to 10″ in breadth. I saw no trace of the black drop or ligament, and, indeed, I should imagine that the aureole crossing the position of the ligament would prevent its appearance. I found nothing like a satellite. I thought the micrometer showed a diameter of the planet rather greater from east to west than from north to south, but the *boiling* of the limbs prevented any measures that could be depended on. I remarked no distortion of the planet as recorded by observers of the previous transit.

JOHN BIRMINGHAM.
Millbrook, Tuam, December 8

IN a published letter, dated "Palermo, December 13, 1882," Signor Cacciatore, Director of the Royal Observatory there, writes as follows:—"The observations of the transit of Venus, effected at our Observatory, present results, both as regards the direct observations and the spectroscopic, to which the attention of astronomers and physicists may fairly be invited. Prof. Riccò, with the spectroscope, when the planet was on the sun's disc, and her image entered upon and left the slit, observed near the spectral line B of the more refrangible side, a very weak absorption band, and also near the line C he saw traces of obscuration, but much more weak and uncertain. The same phenomenon, P. Tacchini writes me, was observed by him at Rome. Moreover, my direct observations yielded an indication of the ingress of the atmosphere of Venus upon the sun, as, from those of Prof. Millosevich in Rome, this indication was obtained on the external portion of the planet. The agreement of such observations made in different places is of no little importance for determination of the existence and the constitution of the atmosphere of Venus."

NOTES

M. BERTRAND, perpetual secretary of the Paris Academy of Sciences, intimates that the French Government is anxious to collect any information relating to Fermat, whose statue will be unveiled very shortly at Toulouse. Those who possess any documents relating to Fermat are requested to communicate with the secretary of the Institute.

THE Johns Hopkins University Circulars contain a great amount of important scientific, as well as other information, concerning the work of that institution, which is rapidly developing into one of the most comprehensive and efficient institutions for research and education anywhere. In the number for November, for example, we have notes on the papers read by members of the University at their various societies as well as elsewhere, in mathematics, physics, philology, biology, &c., synopses of recent American scientific journals (mostly issued from the University), besides abstracts of lectures, critical notes on various subjects, and much other information. From the seventh Annual Report moreover, it is evident that the University has taken a strong hold on the American people, and that both in the spirit and the letter it is amply fulfilling the intentions of the founder. The list of the academical staff alone, professors, associates, lecturers, instructors and assistants, fills three pages, while the account of work in the various departments shows that research has become a part of the everyday life of the institution.

PROF. TYNDALL will on Thursday next (December 28), at the Royal Institution, at three o'clock, give the first of a course of six lectures (adapted to a juvenile auditory) on Light and the Eye.

THE death is announced of Dr. Theod. Lud. Wilh. von Bischoff, formerly Professor of Anatomy and Physiology at Munich University, as well as keeper of the Anatomical Institute in that city. He died on December 5 last, aged seventy-five.

ON December 1 the Agricultural Museum of Berlin was opened to the public. The curator, Herr Settegast, has arranged the zootechnical division in a commendable manner. Numerous paintings and sketches illustrate German domestic animals in their agricultural aspect. In the zoological division there are a number of interesting skeletons and skulls, amongst them a human skull from the shell-tombs at Santos (Brazil).

THE French official journal publishes a report on oyster culture, which is in favour of the Portuguese oyster. It appears that 100 grammes of the flesh of this mollusc contains about 1-10th gramme of iodine, bromine, and chlorine, just twice as much as the common oyster.

MESSRS. FOSTER AND MARTIN, of Melbourne, have sent us a graceful photograph of the comet, about which we have had so much correspondence. The photograph was taken with a 3-inch euryscope of 24 inch focus on an ordinary camera, not equatorially mounted, which doubtlessly accounts for the elongation of the nucleus. The photograph is creditable to Messrs. Foster and Martin, though it is not the first time a comet has been photographed; more than a year ago we reproduced the photograph of the comet of the period, taken by Dr. Janssen of Paris.

Acta Mathematica is the name of a new mathematical journal which will appear this month, simultaneously published in Stockholm, Berlin, and Paris. The editor in chief is Prof. J. Mittag-Leffler, of Stockholm, and the publication has been promised the support of the most distinguished mathematicians of Scandinavia, Germany, and France.

LAST week the Crystal Palace Company inaugurated an Exhibition of Electricity and Gas which gives even greater promise of success than that in which electricity was the sole object of attraction. Gas at present occupies the largest display. Exhibitors demonstrate the utilisation of gas, and there are many practical illustrations going on. The South Nave contains a great collection of all the best systems of improved gas lighting, Sugg's stand being distinguished by an immense standard lamp of 1000 candle power and a series of suspended lanterns of tasteful pattern of 600-candle power. There are similar great gas lights by Bray, Siemens, and others, which are submitted as competitors against the electric arc lights. In the North Nave there are numerous stands of electric apparatus and material.

The annual meeting for the distribution of prizes and certificates in connection with the Institute for the Advancement of Technical Education was held on Thursday night in the hall of the Goldsmiths' Company, Foster Lane. After the presentation of the prizes, Dr. Siemens said that Sir Frederick Bramwell had prevailed upon him to present the prizes on this occasion, and had urged that he was a fit person to do so. The distinction made between ordinary and honour prizes, marking the addition of some scientific knowledge to proficiency in applied science, was worth the attention of all students. It was not sufficient for after-life to be efficient in a craft or calling. Unless the workman also mastered entirely the scientific principles underlying that calling, he might, in consequence of some invention changing the *modus operandi* in an occupation, be left high and dry, whereas with a knowledge of fundamental principles he could adapt himself to changed circumstances. With regard to the school in Cowper Street, he might say, having recently visited it, that the lecture rooms and the laboratory for physical science and chemistry were the most perfect he had seen, and he contrasted them with those in which he had himself received scientific instruction. He remarked upon a deficiency he had noticed in the Finsbury School—the indifferent accommodation and provision for the study of drawing, both artistic and mechanical. He hoped that art and literature would not be neglected in this scheme of education. Dr. Siemens said he hoped that through the dissemination of pure and practical science a higher spirit would take possession of the artisan, and that he would work with the object of attaining higher results and higher ends instead of discussing with his employer questions of hours and wages.

The American papers have been devoting considerable space to Prof. Henry Draper, whose comparatively early death is regarded as a great loss to American Science. The *Tribune* has a long and interesting biographical article.

Mr. A. E. Garrod has published, through Parker and Co., his able and elaborate paper, re-written, on "Nebulæ," which gained the Johnson Memorial Prize (Oxford) in 1879.

One of the largest avalanches ever known in Western Switzerland fell a few days ago near Ormons Dessus in Canton Vaud. It carried away several houses, piled up a mass of ice and snow 200 feet thick, and covered three square kilometres of ground. Some of the ice blocks were 18 feet long. The inmates of the houses struck were got out safely.

Near Tabiana (Italy) the remains of a fossil elephant have been discovered Two enormous tusks, two teeth, and several bones from the skull were found. The objects found were submitted to scientific investigation by Prof. Strobel and Dr. Mariotti of Parma. They declared them to belong to *Elephas* (*Loxodon*) *meridionalis*, Falconer. The tusks measure 3·2 metres in length, and 0·28 metres in diameter at the thickest part. The skull bones were so much decayed that they could not be removed. It was resolved, therefore, to cover up the remains with earth until next summer, when it is hoped that warmer weather will be more favourable to further excavations.

During a stay near the Suez Canal last winter, Prof. Keller of Zurich made a study of the animal migrations due to the opening of this means of communication. These are very positive, though certain causes quite stop some species, or at least retard their movements, especially (1) the too sandy nature of the ground; (2) the large lakes; (3) the currents; (4) the passage of ships, which derange the ova and larvæ; (5) the too great saltness of the canal water. From the Mediterranean to Suez have passed since 1870, *Solen vulgaris*, *Umbrina cirrhosa*, *Labrax lupus*, *Balanus miser*, *Ascidia intestinalis*. Some Mediterranean species are now on their way through (*Solea vagina*, *Cardium edule*, *Sphæroma*), several fishes (*Pristipona stridens*, *Crenidens Forskali*, &c.), and some molluscs (*Cerithium scabridum*, *Mactra olorina*, *Mytilus variabilis*) have passed from the Red Sea to the Mediterranean, while quite a numerous "caravan" is now resting in the basins of the great Bitter Lakes. The fauna of the canal is still too poor for large carnivorous species to find a living in it; hence rays, cuttlefishes, &c., do not migrate. Red Sea corals also have not passed into the canal.

The *Overland China Mail* gives an account, taken from the Manila papers, of the typhoon which visited the Philippine Islands on October 20. The typhoon began at eight o'clock in the morning, and continued with unabated fury until about two o'clock in the afternoon. Not a house in Manila escaped injury. During the storm it was utterly impossible to walk in the streets, owing to the force of the wind, which was rolling carriages along like playthings, and keeping sheets of iron roofing floating in the air like pieces of paper. It is said that during the typhoon several shocks of earthquake were felt. No such destructive typhoon has visited the islands since 1831. The record taken at the Observatory says that the greatest velocity attained by the anemometers reached 144·4 English miles per hour; nothing could resist this force of wind. The vortex was touched at 11·40 a.m., when the minimum barometer reached 727·60 millimetres. The greatest violence of the hurricane could not be indicated, because all the anemometers were rendered useless before the severest gusts came.

The eleventh annual *soirée* and exhibition of the Lambeth Field Club and Scientific Society will take place on Monday evening, January 1, 1883, at St. Philip's Schools, Kennington Road, S.E.

Prof. Gustav von Hayek, an eminent Vienna naturalist, is editing a large Atlas of Natural History. Five parts have appeared, and the work will be complete in fifteen. Each part contains eight plates folio size. Moritz Perles, of Vienna and Leipzig, is the publisher.

"Mythologie und Civilisation der Nordamerikanischen Indianer" is the title of a little work just completed by Herr Karl Knortz, and published by Paul Frohberg of Leipzig.

A violent shock of earthquake was felt at Siders (Canton Valais) on December 5 at 3.40 p.m.; the direction of the shock was from east to west.

The French Lower House has adopted the project of subterranean telegraphic lines, which had elicited some criticism.

On the 2nd inst. Dr. Finsch, who recently returned to Berlin, after an absence of over three and a half years, reported upon his travels before the Geographical Society of Berlin. He first proceeded to Micronesia in order to study the ethnology of the rapidly disappearing natives of those groups of islands. From Honolulu he proceeded to Oahu, where he visited the old burial-grounds. At Maui he succeeded in obtaining a specimen of a bird that is very nearly extinct, and from the scarlet feathers of which formerly the royal mantles were made. In March 1880 he accompanied the German Consul, Herr Hernheim, to the Caroline group, and saw the ruins of the celebrated colossal edifices there. In July Dr. Finsch proceeded to New Britain. He stayed there for eight months, and then went to visit the Maoris in New Zealand. At the beginning of 1882 he proceeded to New Guinea, in August he left for Batavia and then for Europe, having travelled over 30,000 miles. His collections comprise 4000 ethnological objects from 43 localities, 290 skulls, 200 samples of hair, 200 casts of faces taken from living individuals in 66 different places, 6000 vertebrata, 30,000 invertebrata, 1000 plants, numerous minerals, 400 photographs, and 200 sketches.

The Russian Geographical Society has addressed to other scientific societies of Russia a proposal to collaborate in the publication of a general description of Siberia. The Geographical Society undertakes for its part the publication of a geographical description and of a general bibliographical index of all works and papers on Siberia.

The Belgian expedition for the investigation of the Upper Congo has left Antwerp on board the steamer *Harkaway*. The party consists of Dr. van der Heuvel, Herr Schaumann, an Austrian officer, and several mechanics. The expedition takes out large stores of goods, including samples of the seeds of all nutritious vegetables grown in Belgium. They are to proceed as quickly as possible to the furthest of Stanley's stations, and then penetrate further if possible.

The additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius* ♀) from South Africa, presented by Mr. J. W. Browne; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Lady Sibyl Tollemache; a Smooth-headed Capuchin (*Cebus monachus*) from South-East Brazil, presented by Mr. A. J. McEwen; a Squirrel Monkey (*Chrysothrix sciurea* ♂) from Guiana, presented by Mr. M. Escaré; a Rhesus Monkey (*Macacus erythræus* ♂) from India, presented by Mr. G. V. Sawyer; two Leadbeater's Cockatoos (*Cacatua leadbeateri*) from Australia, presented by Mr. C. J. Harvey; a Common Barn Owl (*Strix flammea*), British, presented by the Rev. A. Reece; a Ring-hals Snake (*Sepedon hæmachetes*), a Rhomb-marked Snake (*Psammophylax rhombeatus*) from South Africa, presented by Mr. H. Pillans; a Lesser White-nosed Monkey (*Cercopithecus petaurista* ♂) from West Africa, deposited; a Long-eared Owl (*Asio otus*), British, a Marbled Cat (*Felis marmorata*) from Assam, purchased; a Red Kangaroo (*Macropus rufus* ♂) born in the Gardens.

OUR ASTRONOMICAL COLUMN

Measures of Double Stars.—We receive at about the same time several important series of measures of double stars.

(1) "Results of double star measures made at the Sydney Observatory, N.S.W., 1871 to 1881," under the direction of Mr. H. C. Russell, Government Astronomer for New South Wales. From 1871 to 1874 the instrument employed was a very fine 7¼-inch refractor by Merz; after 1874 the 11½-inch refractor by Schröder was substituted, the same method of observation being followed with both instruments. For the more difficult objects, a power of 330 was applied on the Merz telescope, and one of 800 on the larger refractor. The objects measured include about 746 of Herschel's stars, and it is unnecessary to say more than this, to show the importance and value attaching to the catalogue, no measures of a large number of the stars having been put upon record since the publication of Sir John Herschel's Cape Volume. In addition to these objects, however, Mr. Russell's catalogue includes measures of 350 new double stars detected at Sydney, and he remarks that it would have been easy to double the number if he had adopted the same limit of distance as Sir John Herschel, and without making any very strict examination of the southern heavens, which will be a hint to future workers in this branch of astronomy in the other hemisphere. Some of Herschel's stars, Mr. Russell says, present considerable difficulty, but are probably in motion; thus γ Lupi, an easy double-star in 1836, is now single under the highest power on his large equatorial; π Lupi, which Herschel found "excessively difficult," is now quite an easy object with the Sydney refractor; *h* 4854 is another star of the same character; in June, 1872, it was easily divided with power 230; in June, 1874, it could not be divided with any power; and in July, 1880, it presented only a round disc with all powers on the large telescope.

Mr. Russell has made an innovation in the manner of expressing the dates of the separate sets of measures, which appears an unfortunate one: instead of giving them according to the usual method, as fractions of the different years, he has three columns with "Day of the month," "Month of the year," and "Year in the 19th century," and this inconvenient expression of dates is not remedied without some trouble, by means of the table at p. 68, showing day and fraction of year. The computer of double-star orbits in taking means of sets of measures for an epoch to work upon, will hardly appreciate this innovation.

(2) "Micrometric measurements of double-stars" in vol. xiii. part 1, of "Annals of the Astronomical Observatory of Harvard College." This is a valuable catalogue of measures of about 350 stars in upwards of one thousand sets, made with the 15-inch refractor at Harvard College, chiefly in the years 1866-1872, under the direction of Prof. Winlock, but including a few obtained by the Bonds, and by Mr. Waldo, which have previously appeared in the *Proceedings* of the American Academy of Arts and Sciences, and in the *Astronomische Nachrichten*. The catalogue includes nearly all the more interesting binaries and many difficult objects. In addition, Prof. Pickering publishes a list of 179 double stars discovered at Harvard College Observatory, some of which have been independently detected by Mr. S. W. Burnham; these were found to a considerable extent during an exploration of the southern heavens, occasionally instituted in the intervals of other observations. In the cases of some of the principal revolving doubles as γ Virginis, 70 Ophiuchi, &c., the measures extend to the year 1876.

(3) "Measures of the principal double stars in rapid orbital motion," made in the years 1875-1882, with the Merz refractor of the Observatory of Brera, Milan by Prof. Schiaparelli; an important series of results which will be most welcome to those who are engaged in the investigation of double star orbits, since in most cases, there are measures later than any others available at the present moment. We extract a few of the more recent mean results:—

		Position	Distance
ζ Cancri (A : B) ...	1882·247 ...	75°·07 ...	0″·980
ω Leonis	1882·363 ...	89·99 ...	0·55
ξ Ursæ Majoris ...	1882·386 ...	261·06 ...	1·928
η Coronæ Borealis ...	1882·503 ...	135·37 ...	0.594
μ^2 Bootis	1882·521 ...	120·40 ...	0·795
ζ Herculis	1882·602 ...	101·55 ...	1·473
τ Ophiuchi	1882·600 ...	252·13 ...	1·860
70 Ophiuchi	1882·609 ...	51·83 ...	2·336

No trace of the companion of γ Coronæ Borealis was visible in the years 1875-1881. In 1882 a prominence was once suspected at 120°, but at other times the star was single. In 1875-1879, however, this star was single in the Washington 26-inch refractor.

PHYSICAL NOTES

Prof. W. Kohlrausch gives the following as the results of recent experiments on the electric conductivity of the haloid salts of silver. Chloride, bromide, and iodide of silver at temperatures above their melting-points conduct far better than the best conducting liquids (sulphuric acid, &c.) at ordinary temperatures do. Chloride of silver conducts best, iodide worst of the three. The chloride and the iodide of silver change their resistance very greatly and suddenly on solidifying, the resistance increasing more than a million-fold by cooling through 20°. More remarkable still, iodide of silver undergoes absolutely no change of conductivity at its melting-point (540°), but shows a rapid decrease at the temperature (145°) at which it passes from the amorphous to the crystalline state.

New combinations to serve for direct-vision prisms have been suggested recently by several persons. Mr. C. D. Ahrens uses a bisulphide prism cemented between two flint glass prisms, giving a wide dispersion with little loss of light. Herr Fuchs employs a single isosceles glass prism in the position of minimum deviation, a silver-faced mirror being attached to the basal face of the prism to rectify the ray after emergence. Signor A. Ricco has described a similar combination, a total-reflexion prism being substituted for the mirror. He has also constructed the second prism of the combination of a four-sided form, so that it not only rectifies the ray which has been deflected by the first prism, but also augments the dispersion of the first prism by a nearly equal amount.

The electric resistance of mercury is, according to R. Lenz, affected by pressure. Between the limits of 2 and 60 atmospheres' pressure, the resistance of a quicksilver column 1·2 metres long, inclosed in thermometer tubing, diminished ·02 per cent. for each additional atmosphere.

Prof. Melde of Marburg proposes to study the force of electric reaction as exhibited in the rotation of Hamilton's well-known "mill," by attaching the "mill" to a torsion fibre, and observing the *torque* produced by the electric reaction. As Tomlinson has shown, the "mill" will work when surrounded by turpentine or other insulating liquid; hence Prof. Melde's suggestion promises to prove of some interest.

Dr. H. P. Bowditch has recently published in the *Journal of Physiology* a paper on the optical illusions of motion, in which he deals chiefly with the peculiar illusions of rotation, &c., studied a few years ago by Prof. Silvanus P. Thompson. He entirely agrees with the latter experimenter in rejecting the explanation advanced by R. Addams, and more recently by Javal, that these illusions are due to muscular slip, and declares that such an explanation is worthless, being contradicted by the fact that motor after-effects in opposite directions are possible for the same retina at the same time. Dr. Bowditch also thinks that these persistent after-impressions of motion cannot be the product of experience or association; because experience cannot overcome, nor volition control or reverse them. He looks for an explanation in the narrowness of the limits of distinct vision.

M. Berson has contributed to our knowledge of the magnetic properties of metals by some recent researches on their degree of magnetisation at different temperatures. The experimental method followed consisted in comparing the magnetic moments of different bars by Gauss's method at different temperatures while placed in a magnetic field of constant intensity. The following are the results:—With iron the total and temporary magnetisations both increase up to 260° C., above which the temporary magnetisation falls off rapidly, but the permanent slowly. In steel the total magnetisation is also a maximum at 260° C., but the permanent magnetisation attains its maximum about 240° C. The magnetisation of a steel bar magnetised while cold is diminished by heating, whilst that of a bar magnetised while hot is diminished by cooling. This result appears to be important, as it would follow that a magnet has its permanent maximum power at that temperature at which it was magnetised. With nickel the total magnetisation increases up to 240°, and diminishes above 280° so rapidly as to be zero at 330°. But if magnetised at 280°, the magnetic moment during the subsequent cooling first increases, then diminishes slightly, but still remains greater than at the temperature at which it was magnetised. Cobalt behaves like steel.

M. Hesehus publishes in the last volume of the *Journal* of the Russian Chemical and Physical Society an interesting paper on his researches on "residual elasticity" (a rather difficult term to translate), the *elastische Nachwirkung* of W. Weber. Without attempting to deal with the immense range of phenomena concerning permanent changes of shape of elastic bodies under the influence of small but continually acting forces, M. Hesehus has studied these changes in a few bodies, especially in lead and caoutchouc, and has made an attempt to bring these changes into connection with other physical phenomena. He comes to the conclusion that residual elasticity depends to a great extent upon the mass of the body, and its surface; that the elastic conductibility depends upon, and increases with, temperature; and that the laws of residual elasticity afford close analogies with those of heating and cooling of solid bodies, as well as with those of phosphorescence and of residual magnetism and electricity.

At a meeting of the Russian Physical Society, M. Kraevitch made an interesting communication on the results of his researches on the elasticity of air. Rarified air does not obey the Boyle-Mariotte law, that is, in proportion as it becomes more rarified its elasticity diminishes more rapidly than its density, and becomes equal to zero, while the density has still a measurable value. M. Kraevitch observes that it would result from these experiments: (1) that the atmosphere of the earth is limited; and (2) that our weights of gases contain an error, as, however perfect the pneumatic machine, it cannot pump all air from a vessel, if this vessel is lower than the pneumatic machine, or the air is pumped from above. Prof. Mendeleeff, recognising the importance of these researches, advised M. Kraevitch to continue them on heavy gases.

In a paper relating to recent studies of the Rhone glacier (read at last meeting of the Helvetic Society of Sciences), Prof. Forel formulates these four questions as, in his opinion, the most urgent for a theoretic knowledge of the phenomena of glaciers: (1) How and in what measure does the velocity of flow vary in different layers of the depth of the glacier? (2) How and in what proportion does the surface-velocity vary if the glacier increases or diminishes in thickness? (3) What is the temperature of the internal mass of the glacier? (4) What are the laws of periodic variations of different glaciers? (For this study it is desirable to know, in the case of each glacier, the epochs of commencement of periods of elongation or shortening).

Herr Hertz has recently measured with special apparatus, the pressure of saturating vapour of mercury at different temperatures, from 0° to 220° (*Wied. Ann.*, No. 10). His numbers are considerably smaller than those of Regnault; and with Herr Hagen's they agree only between 80° and 100° C, being greater below, and smaller above these limits. Between 0° and 40° he finds the elastic force of the vapour of mercury to vary from 0·00019 mm. to 0·0063 mm. It follows that at ordinary atmospheric temperatures it is less than $\frac{1}{1000}$ mm. This result is important in reference to barometers, machines, and Geissler tubes.

Signor Martini has studied the sounds produced by outflow of water through a cylindrical hole in a metal disc at the bottom of a long glass tube filled with the liquid (*Atti del. R. Ist. Veneto*, 5 ser. t. viii. 1882). In such a case one does not hear a series of sounds of decreasing pitch, though the liquid charge continually shortens; but a certain number of distinct sounds. The sound is due, as Savart proved, to the vibrations of the liquid vein; and the author verified Savart's law, that the numbers of these are proportional to the liquid charge and inversely as the diameter of the hole. A pure sound of clear tonality is only got if the sound of the vein is one of those the liquid column can yield. The series of sounds from a liquid column of constant length is that of the harmonics of an open pipe. The air column above the liquid strengthens some of the sounds. The sound is quenched if the tube is kept from vibrating. These experiments afford a means of comparing the velocities of sounds in different liquids. One has only to find what lengths the columns must have to yield a particular sound (all air-bubbles must be expelled). The author has tried alcohol, sulphuric ether, and petroleum, and found numbers agreeing with those by other methods.

It appears from recent experiments by Herr E. Wiedmann (*Wied. Ann.*, No. 12) that a number of water-containing salts, when heated, undergo chemical transposition even before fusion. He has, in this inquiry, found two new modifications of zinc-sulphate and magnesium-sulphate, and determined the changes of volume attending their formation. The general result, he points out, is of interest with reference (1) to determination of tension, inasmuch as it is necessary, first, to ascertain whether a given salt remains unaltered or not within the range of temperature considered; (2) to researches on heat of solution, &c., of a salt partly deprived of water by heating; it should be exactly determined in what form water and anhydride salt are combined.

CHEMICAL NOTES

A recent patent by Mr. Morris, of Uddingston, N.B., claims to have solved a problem which has long baffled the skill of technical chemists. By heating an intimate mixture of alumina and charcoal, in a current of carbon dioxide, Mr. Morris says that metallic aluminium is produced; the metal is purified from carbon and aluminium by fusion.

What may perhaps be called the kinetic theory of chemical actions, the theory, namely, that the direction and the amount of any chemical change is conditioned not only by the affinities, but also by the masses of the reacting substances, by the temperature, pressure, and other physical circumstances—is being gradually accepted, and illustrated by experimental results. Thus Hammond (*Monatsheft fur Chimie*, 3, 149) concludes, from experiments on the hydration of salts, that when a saline solution is gradually concentrated various hydrates are formed, but that the crystallisation of any one of these from the liquid depends on the relative quantities of the various hydrates, and on the temperature of the solution. Another example of the establishment of a state of equilibrium between antagonistic chemical systems is furnished by the recent observations of L. de Boisbaudran (*Compt. rend.*, 95, 18) on gallium protochloride. When gallium is dissolved in cold concentrated hydrochloric acid a stable solution of the protochloride is obtained; but when water is added to the solution hydrogen is evolved and gallium perchloride is produced.

HERR SCHWARZ describes (in *Berichte der Deut. Chem. Ges.*, xv. 2505) three lecture experiments illustrative of the action of zinc on sulphur: 2 parts of fine zinc powder are carefully mixed with 1 part of flowers of sulphur, and the mixture is ignited by an ordinary match; combination occurs with evolution of much light. Vapour of carbon disulphide is passed over zinc powder, which is gently heated in a piece of glass tubing; zinc sulphide is produced, and a considerable quantity of carbon is separated. Sulphuretted hydrogen is passed through carbon disulphide, and the mixed gases are then conducted over hot zinc powder; zinc sulphide is produced, and a gas, which is passed through potash and collected in a small gas-holder; this gas burns with a slightly luminous flame, and explodes when mixed with air; it is marsh gas.

NILSON has prepared the rare metal thorium in considerable quantity, and determined its atomic weight to be 232·35, specific gravity about 11, and atomic volume about 21 (*Berichte*, xv. 2519).

M. MIKLUKHO-MACLAY ON NEW GUINEA [1]

AMONG the queries that were submitted to M. Milukho-Maclay before his departure from Europe, was one of Karl von Baer, who advised the traveller to visit the Philippine Islands, and to bring home several skulls of the natives, in order to ascertain whether the primitive inhabitants of these islands are brachiocephalic, or not. During a five days' stay of the clipper *Izumrud* at Manila, M. Maclay visited the Mariveles mountains, and discovered there Negritos who lived in their *pondos*, or small huts made out of palm-tree leaves. Numerous measurements (favoured by the custom of the men shaving the back of the head) proved that they really are brachiocephalic, the index being no less than 87·5 to 90 Their size is altogether small; one woman, mother of two children, measured only 1·30 metre. Their faces proved to be very much like those of the Papuans of New Guinea, while their customs are much akin to those of the inhabitants of many Melanesian islands. For instance, when M. Maclay threw some remains of food in the fire, the Negritos immediately extinguished it, and asked him not to do so again. The same prejudice exists with regard to spitting in the fire (a very widely-spread prejudice, we may observe, as it exists also in Russia and Siberia). Another interesting custom of the Negritos is that everybody, before eating, must loudly shout out several times, an invitation to partake of his food, to all those who may be in proximity. This custom is very rigidly observed, and those who do not comply with it are punished, even by death.

In August, 1874, M. Miklukho-Maclay undertook a journey into the interior of the Malay peninsula, in order to settle the question as to the race of its inhabitants—the Orang-Sokays and the Orang-Semongs—about which question there existed a controversy between Messrs. Logan, Newbold, Crawford, and Waitz. M. Maclay went, therefore, from Singapore to Johore. The Maharajah of Johore received him very kindly, and gave him the necessary men for the journey, as well as orders to his subjects to help him in every way during his journey. In exchange, M. Maclay was bound to prepare a map of the dominions of the Maharajah. The Russian traveller crossed the Johore country twice—from west to east and from north to south. The journey was very difficult, on account of the rainy season; the rivers and streams had inundated the country, even the woods, and the party had to walk in water that reached to the knees, and often to the breasts of the oxen. For seventeen consecutive days they were quite wet, as well as their baggage. Reaching thus the mouth of the Moar river, M. Maclay journeyed up this river in a flat boat, passing by Malayan villages, reached its confluence with the Pallon, and went up the last river. At its sources he discovered in the woods the first huts of the so-called "orang-utangs." This name is given by the Malayans, not to the ape, *Pithecus satyrus*—they never call apes by the name of "orang," but to "forest-men." "Orang" signifies "man," and "utang" a forest. Therefore the Malayans say *orang-bukit* (men of the hills), *orang-ulu* (men at the source of a river), *orang-dalah*, *orang-laut* (men of the interior of the sea-shore), and so on. However, the name *orang-utang* could be applied also to a Malay who stays in the woods, but still it is used to designate a tribe of Malays crossed in various degrees with Papuans, as also with Melanesians.

Though the different tribes M. Maclay met with during his journeys in Johore differ from one another, still none of them are Melanesians. They forget their primitive language, and adopt that of the Malays. M. Maclay presumes that formerly they had several languages, and were divided into several tribes; some difference still remains in their customs. They are at a very low stage of human culture. They wander in the woods, and only occasionally come to stay in their miserable huts. The Malays distinguish two different tribes of orang-outangs: the orang-outang-dina (or tame, who are in intercourse with them), and the orang-outang-liar, quite nomadic: These last use a weapon, *sumpitan*, which deserves to be mentioned. It consists of a hollow bamboo cylinder, two metres long and two or three centimetres wide, through which they blow against their enemies very light poisoned arrows, as large as knitting-needles. The end of these arrows breaks, and remains in the wound. The Malays say that the slightest scratch of such an arrow kills a man in ten or fifteen minutes. M. Maclay purchased quantities of their poison, which proved always to be made of a condensed infusion of the bark of the Javan tree, *Antiaris toxicoria*, or Upas, to which different tribes add other poisons, such as the poison of snakes, of poisonous kinds of strychnis, &c. A small prick of a poisoned arrow kills a dog or a cat, the death being accompanied by tetanus or not, according to the secondary poisons added to the chief one. The Orang-outangs are rapidly disappearing since they were driven by Chinese and Malayans from the sea-shore to the woods of the interior. Besides, the Chinese and Malays purchase their best-looking and healthier girls, leaving them the feeblest, who leave but a weak progeny. The children from the Malays and Orang-outang girls are far more like the former than the latter.

After having crossed the Johore country from the mouth of the Moar River to the entrance of the Indan into the Chinese Sea, that is, from west to east, M. Maclay crossed the same country from north to south, that is from the Indan to the Selat-tebran Strait, which separates Singapore Island from the mainland. He contracted, of course, a strong fever during this journey, fifty days long, and went to Bangkok. There he happened to receive from the King of Siam a letter to his vassals of the Malay peninsula, enjoining them to help M. Maclay during his further travels on the peninsula. Provided with this recommendation, the Russian traveller undertook a most adventurous journey, namely, to walk from Johore to Siam. It was considered by all his acquaintances as quite impossible, but he accomplished it, as the small rulers of the southern part of the Malay peninsula did not venture to stop him on his way, and preferred, each of them, to despatch him to the next ruler. In this way M. Maclay reached Siam, after a journey that lasted for 176 days.

In the mountains at the sources of the Pakkan River, M. Maclay finally met with undoubtedly pure Melanesians, Orang-Sakays, and made on them a few anthropological measurements. They differ as much from the Malayans, as the Malayans differ from the Papuans, and are like the Negritos of Luzon. The height of the men varies between 1·46 and 1·62 metres, and that of the women from 1·35 to 1·48; the skull is nearly brachiocephalic, that is, the widest is between 74 to 82 for men, 75 to 84 for women, and 74 to 81 for children. The diameter of the curls of the hairs is the same as with the Papuans, that is, from 2 to 4 millimetres. The colour of skin is between the numbers 28 to 42, and 21 to 46 of the table of Broca. The *plica semilunaris*, or the so-called *palabra tertia*, is more developed than with other races; its width reaches sometimes 5 and 5·5 millimetres, instead of the 1·5 to 2 millimetres of the Caucasian race. Finally the Orang-sakays have also a fold of the skin at the interior corner of the eye which is known, when pathologically developed, under the name of *Epicanthus*. Like the Orang-utangs they are disappearing; they nomadize in forests, stopping at a few places to mass collections of camphor and caoutchouc tree, of rotang and elephant bone, which they exchange with Malays for tobacco, salt, iron knives, and various rugs which they use for their dress. The dress of the men consists of a girdle, a part of which covers the *perinacum*: the women have also a girdle of rotang, to which two rugs are adjusted. The women are tattooed by lines and round spots. The Orang-sakays, like other Melanesians, put in the partition of the nose the *hayanmsh*, that is, a long stick of bamboo, or a spike of the *Hystria*.

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[1] Continued from p 138.

The Orang-sakays are very kind to their women and daughters, who can even inherit the title of *Pateu*. Their wedding customs contain survival of the custom of stealing brides. On a day agreed to before, the bride, in presence of her parents and friends, runs away to the forests, and the bridegroom, who follows her after some time, must find her during a fixed lapse of time. If she does not wish to marry him she can always conceal herself in the woods so as not to be found. They have maintained also the communal marriage, that is, the wife passes from one man to another for a certain time. They are much afraid of death, and if a member of the community becomes seriously ill, they abandon him in the forest with a supply of food, and leave their huts for ever.

In his fourth lecture, M. Miklukho-Maclay gave an account of his cruise among the islands of the Malayan Archipelago, and the islands of Micronesia and Melanesia, as well as of his work in Australia. The anthropological researches in the Malayan archipelago were far more successful than in Melanesia or New Guinea. He had no longer to deal with wild tribes, and the schools, hospitals, and prisons maintained by the Dutch on these islands gave him many opportunities for anthropological studies. M. Maclay thus made very numerous measurements and photographs of Malayans, which will afford the necessary materials for comparing them with other allied races.

In 1876, when going for a second time to the Maclay coast of New Guinea, the indefatigable traveller had an opportunity of visiting the islands of Western Micronesia. He found there that the Micronesian race is very nearly akin to the Polynesian; but still, he thinks it is most probable that it contains a mixture of Melanesian blood, which appears, especially in the more curly hair; in several instances (on the Pelan Islands), the hairs were purely Melanesian, and the darkness of the skin and several distinctive features of the skull showed unmistakable traces of mixture with Melanesians. On the Lub, or Hermit Islands, M. Maclay found a mixture of Melanesians with Micronesians, and on the next group of islands, Escheker, or Eschikie, he discovered the true border-line of the straight-haired Micronesian race. The most important results of this journey were published in the *Sitzungsberichte der Berliner Gesellschaft für Anthropologie*, &c., meeting of March 3, 1878.

In 1879 M. Maclay left Sydney on board an American schooner for a cruise among the islands of Melanesia. He visited New Caledonia, and journeyed in the interior of it, the Loyalty Islands, and many islands of the New Hebrides, making everywhere anthropological measurements and drawings. Many inhabitants of the New Hebrides proved to be brachiocephalic. Thence he proceeded to the Santa Cruz Islands (where Commodore Goodenough, several sailors, and Bishop Paterson were killed by poisoned arrows), and made measurements of those natives who came on board the schooner. Reaching then the Admiralty Islands, he stayed there for two months, and was enabled to complete to a great extent the observations of the *Challenger* expedition. Visiting further the Lub or Hermit islands, which are said to have been peopled by a few natives landed in a canoe from the Admiralty Islands, and where the Melanesians are continually crossed with Micronesians, as the inhabitants bring every year slaves and women from the Ninigo group—M. Maclay proceeded to the Trobrian group of islands which are very rarely visited by Europeans, and thence to the Solomon and the Luisiadea Islands. The journey lasted for 409 days, out of which 237 were spent on shore. The results were as numerous as important, the chief of them being that brachiocephaly is far more usual in Melanesia than it was before; indexes measuring 81, and even 85, being not rare. Further results of this journey were published in the *Izvestia* of the Russian Geographical Society for 1881, and in a letter to Prof. Virchow, which appeared in the *Sitzungsberichte* of the Berlin Society of Anthropology.

M. Miklukho-Maclay concluded his journey by landing at Somerset, on the northern extremity of Australia, and at several places of the eastern coast, in order to make acquaintance with the black Australian race. It is known that several opinions are current as to the origin of this race. Some anthropologists consider them as Papuans, while others consider them as Polynesians, and Prof. Huxley has made of them an independent race of Astraloids. As far as M. Maclay's observations go, he is inclined to consider them, like Huxley, as a race *sui generis*. But he intends to return again to Australia in order further to study this question.

When staying at Brisbane, M. Maclay undertook an excursion into the interior of the country to see if there really exists an "unhaired" race, of which he was told in Europe. Close by Saint George's Town, on the Ballon River, he discovered a few members of one family who really were representatives of the *Atrychia universalis*; they had only a dozen hairs on their eyelids, and said that they were already a third generation of unhaired people. More details of this occurrence of *atrychia*, together with observations on inherited *hypertrichosis* (hairs covering all the body and face) were given by M. Maclay in a letter to Prof. Virchow which appeared in the *Verhandlungen* of the Berlin Anthropological Society for 1881, as well as several other papers on Australians ("Ueber die Mika Operation in Central Australien; Langbeinigkeit der australischen Frauen," &c.).

At Brisbane M. Maclay had at his disposal very rich anthropological material for the study of the comparative anatomy of the brain of the Australian, Melanesian, Malayan, and Mongolian races, as the authorities had given him all facilities for having fresh brains of representatives of all these races who died in the hospitals of the port, or were executed. The Survey Office of Brisbane offered him the use of its excellent photographs, which rendered him very great services. This rich material, left mostly at Sydney, has not yet been worked out by M. Maclay; but he can already state that the brains of different races afford substantial differences in the development of the *corpus callosum*, the *pons varolii*, and the cerebellum, as well as in the relative development of nerves and in the grouping of the sinuosities of the great brain.

Further interesting studies in comparative anatomy were made by M. Maclay on the brains of Marsupials, as well as of the *Ornithorhynchus*, the *Echidna*, and others. The chief occupation of M. Maclay in Australia being thus comparative anatomy, he proposed to the Linnean Society of New South Wales to open a biological station where everybody could "undisturbed and undisturbing" carry on biological and anatomical studies. The success which has met his proposal our readers already know of. The recently-founded "Australian Biological Association" will take care of the new station.

The Geographical Society not having at its disposal the necessary sums for publishing the scientific work which M. Maclay proposes to publish, the Emperor has allowed the sum of 2200*l*. for covering the expenses of the publication.

THE RECENT AND COMING TOTAL SOLAR ECLIPSES[1]

THE following note has been drawn up in anticipation of the detailed accounts of the work done by me in Egypt on the eclipsed sun of 1882, May 17, which I am preparing to lay before the Royal Society, because as the next total eclipse occurs next May, there is no time to be lost if any attempt is to be made to secure observations, and I am of opinion that such observations are most important.

I have prefaced the statement of the work done by a reference to the considerations which led me to undertake it, and I have added a scheme of observations which, in the present state of our knowledge is, I think, most likely to produce results of value.

1. In order to understand the recent change of front in solar research which has followed the introduction of the view of the possible dissociation of elementary bodies at solar temperatures, and suggested the later laboratory, and especially the later eclipse observations with which we are now chiefly concerned, we must first consider what facts we may expect on the two hypotheses. In this way we can see which hypothesis fits the facts best, and whether there are any inquiries possible during eclipses of a nature to throw light on the question.

2. On the old hypothesis the construction of the solar atmosphere was imaged as follows:—

(1.) We have terrestrial elements in the sun's atmosphere.

(2.) They thin out in the order of vapour density, all being represented in the lower strata, since the solar atmosphere at the lower levels is incompetent to dissociate them.

(3.) In the lower strata we have especially those of higher atomic weight, all together forming a so-called "reversing layer" by which chiefly the Fraunhofer spectrum is produced.

3. The new hypothesis necessitates a radical change in the above views. According to it the three main statements made in paragraph 2 require to be changed as follows:—

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[1] Paper read at the Royal Society, Nov. 23, by J. Norman Lockyer, F.R.S

(1.) If the terrestrial elements exist at all in the sun's atmosphere they are in process of ultimate formation in the cooler parts of it.

(2.) The sun's atmosphere is not composed of strata which thin out, all substances being represented at the bottom ; but of true strata like the skins of an onion, each different in composition from the one either above or below. Thus, taking the sun in a state of quiescence and dealing only with a *section*, we shall have as shown in (Fig. 1) C say containing neither D nor B, and B containing neither A nor C.

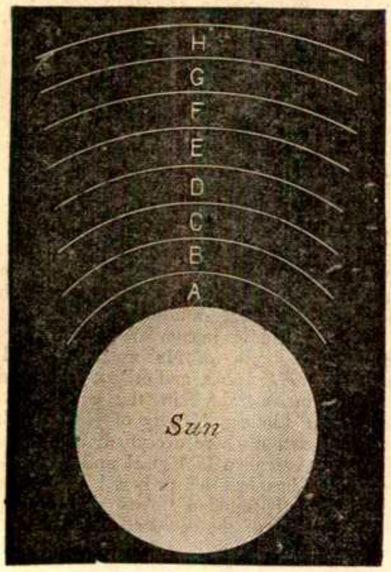

FIG. 1.

(3.) In the lower strata we have not elementary substances of high atomic weight, *but those constituents of all the elementary bodies which can resist the greater heat of these regions.*

4. The conditions under which we observe the phenomena of the sun's atmosphere have not, as a rule, been sufficiently borne in mind, and it is quite possible that the notion of the strata thinning out has, to a certain extent, been based more upon the actual phenomena than upon reasoning upon the phenomena.

5. Take three concentric envelopes of the sun's atmosphere, A, B, C, so that C extends to the base of A, and B also to the base of A, that is, in both cases to the photosphere. Then, whether we deal either with the sphere or a section of it, the lengths of the lines in the spectrum of the strata C, B, A will give the heights to which the strata extend from the sun, and show whether B and A respectively thin out. As the material is by hypothesis continuous down to the sun, the lines will be continuous down to the spectrum of the sun seen below as shown.

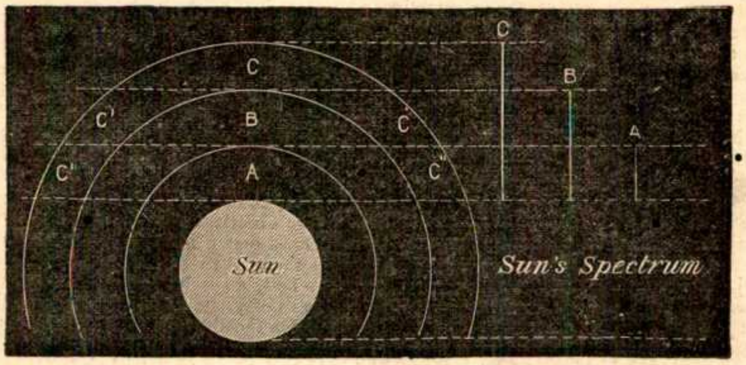

FIG. 3.

6. Now take three concentric envelopes, A, B, C, so that only A rests on the photosphere. The phenomena will *in the main* be the same as in the former case, *i.e.* the line C will still appear to rest on the spectrum of the photosphere, for it will be fed, so to speak, from C′ and C″, though absent along the line C B A at B and A.

7. Thus much having been premised with regard to the observations as conditioned by the fact that we are observing a sphere, we can now proceed to note *how the two hypotheses deal with the facts.*

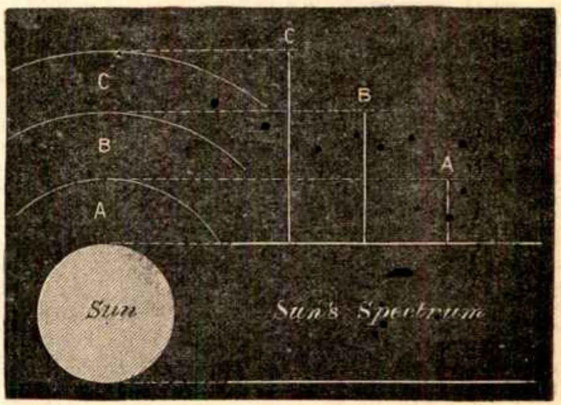

FIG. 2.

Old Hypothesis.	*New Hypothesis.*
1. The spectrum of each element as seen in our laboratories should be exactly represented in the solar spectrum.	The spectra should *not* resemble each other.

FACT.—There is a very wide difference between the spectra.

2. Motion in the iron vapour, *e.g.* in a spot or a prominence, should be indicated by the contortion of all the iron lines equally.	Motion should be unequally indicated because the lines are due to divers constituents which exist in different strata according as they can resist the higher temperatures of the interior regions.

FACT.—The indications show both rest and motion.

3. The spectrum of iron in a prominence should be the same as the spectrum of iron in a sun-spot.	The spectrum of iron in a prominence should be vastly different from the spectrum of iron in a sun-spot, because a spot is cooler than a prominence.

FACT.—The spectra are as dissimilar as those of any two elements.

Old Hypothesis	*New Hypothesis*
4. The spectra of spots and prominences should not vary with the sun-spot period.	The spectra should vary because the sun is hotter at maximum.

FACT.—They do vary.

5. The spectrum of the base of the solar atmosphere should most resemble the ordinary Fraunhofer spectrum.	The spectrum of the base should least resemble the Fraunhofer spectrum, because at the base we only get those molecules which can resist the highest temperatures.

FACT.—As a rule the lines seen at the base are either faint Fraunhofer lines, or are entirely absent from the ordinary spectrum of the sun.

6. *Quâ* the same element the lines widest in spots should always be the same	*Quâ* the same element the lines widest in spots should vary enormously, because the absorbing material is likely to originate in and to be carried to different depths.

FACT.—There is immense variation.

7. The spectra of prominences should consist of lines familiar to us in our laboratories, because solar and terrestrial elements are the same.	The spectra of prominences should be in most cases unfamiliar, because prominences represent outpourings from a body hot enough to prevent the coming together of the atoms of which our chemical elements are composed.

FACT.—When we leave H, Mg, Ca, and Na, most of the lines are either of unknown origin or are feeble lines in the spectra of known elements.

8. From the above sketch, hasty though it be, it is, I think, easy to gather that the new view includes the facts much better than the old one, and in truth demands phenomena and simply and sufficiently explains them, which were stumbling blocks and paradoxes on the old one.

This being so, then, it is permissible to consider it further.

9. Let us first suppose, to take the simplest case, that the sun when cold will be a solid mass of one pure element, *i.e.* that the evolution brought about by reduction of temperatures shall be along one line only. Let us take iron as the final product. Then the sun's atmosphere on the new theory *quâ* this one element may be represented as follows :—

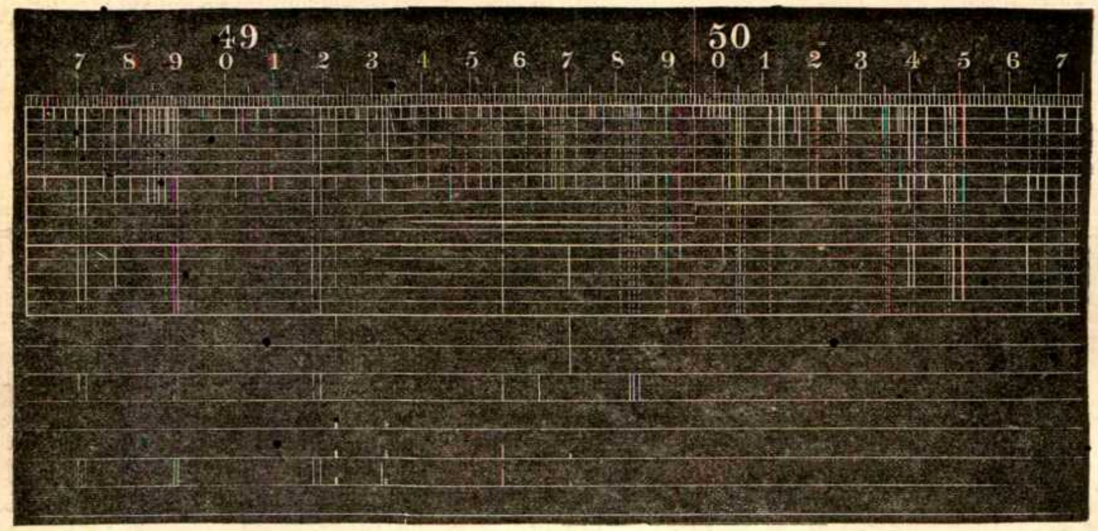

FIG. 5.

Assume strata A—L. Then—

(1.) The Fraunhofer spectrum will integrate for us the absorption of all strata from A to L.

(2.) The darkest lines of the Fraunhofer spectrum will be those absorbed nearest the outside of the atmosphere.

(3.) We shall rarely, if ever, see the darkest lines affected in spots and prominences.

(4.) The germs of iron are distributed among the various strata according to their heat-resisting properties, the most complex at L, the least complex at A.

(5.) Whatever process of evolution be imagined, as the temperature runs down from A to L, whether A, 2A, 4A; or A + B, 2[2(A + B)], or X + Y + Z, the formed material or final product is the work of the successive associations rendered possible by the gradually lowering temperature of the successive strata, and can therefore only exist at L.

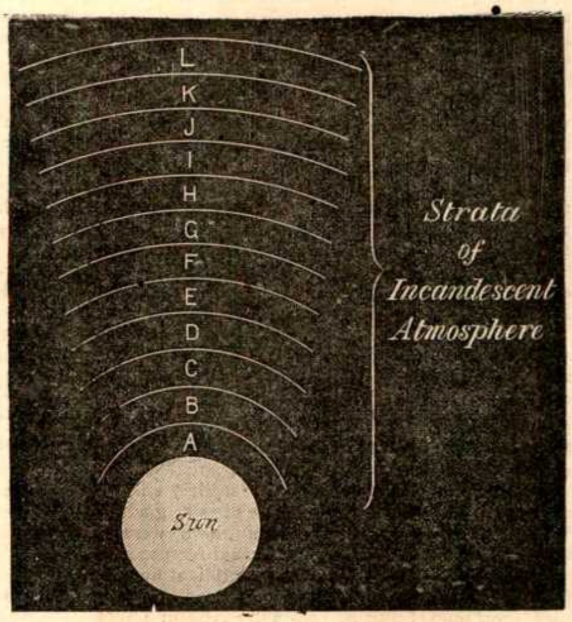

FIG. 4.

10. Now at this point a very important consideration comes in. It was stated (in 6) while discussing the conditions of observation, that whether we were dealing with strata of substances extending down to the sun or limited to certain heights, the spectral lines would always appear to rest on the solar spectrum, and that the phenomena would *in the main* be the same

11. This, however, is true in the main only, there must be a difference, and this supplies us with a test between the rival

hypotheses of the greatest stringency. The stratum B, being further removed from the photosphere than the stratum A, will be cooler, its lines therefore will be dimmer, and the lines of C will be dimmer than the lines of B, and so on. So if we could really observe the strata, *the longer a line is, i.e.*, the greater the height at which the stratum which gives rise to it lies, *the dimmer the line will be.*

12. Now our best chance of making such an observation as this is during a total eclipse. We do not see the lines ordinarily in consequence of the illumination of our air. As during an eclipse before totality the intensity of this illumination is rapidly diminishing, the lines first visible should be short and bright, and should remain short while the new lines which become visible as the darkness increases should be of gradually increasing length, so that the spectrum should become richer in the way indicated in Fig. 5.

13. Further, the lines in 1 should be lines seen in prominences, and not in spots, and relatively brighter in the spark than in the arc, while the longer lines added in 2 and 3 should be lines affected in spots, and *not* in prominences.

14. All these phenomena were predicted for the Egyptian eclipse a year before its occurrence, and were verified to the letter for the lines of iron over a purposely limited region.

15. The actual observations of the iron lines made at Sohag are shown in the accompanying map, and these actual observations are contrasted with the lines thickened in spots, the lines observed in the prominences by Tacchini, those intensified on passing from the arc to the spark. The Fraunhofer lines are also given according to Ångström and Vogel, and the iron spectrum of the arc and spark according to Ångström and Thalén. The observations during the eclipse were made 7 minutes, 3 minutes, and 2 minutes before totality as the air was gradually darkened, by which darkening, successive veils, as it were, were lifted so that the more delicate phenomena could be successively seen.

16. We begin with one short and brilliant line constantly seen in prominences, never seen in spots. Next, another line appears, also short and brilliant, constantly seen in prominences, and now, for the first time, a longer and thinner line appears, occasionally noted as widened in spots, while last of all we get very long, very delicate relatively, two lines constantly seen widened in spots, and another line not seen in the spark and never yet recorded as widened in the spots.

17. The procession from the hot to the colder is apparent, and the simplicity of the spectrum as opposed to the Fraunhofer spectrum even yet, is eloquent of the gradual approximation which would be still possible if the darkness could be greater and our attack more complete.

18. It will be noted over what an excessively small range the observations extend. We want similar observations over a wider range during future eclipses, and to do this work properly many observers armed with similar instruments must divide the whole or part of the solar spectrum amongst them, preferably that part between F and D which has been most closely watched in prominences and spots by Tacchini and myself.

19. I next pass to another point on which an observation was made in Egypt.

20. In Fig. 4 we considered the sun's atmosphere, taking the simplest case, that of one element; but evolution and the chemistry of our earth teach us that when the sun cools it will be a very complex mass chemically. If the laws of evolution hold we need not expect that this will largely increase the complexity of the hottest layers A and B, but higher up, say at H—L, the complexity of chemical forms produced by evolution along the fittest lines will be very considerable.

21. These strata H—L may be taken to represent the corona. Its spectrum, therefore, should not be a continuous one, but should consist of an integration of all the radiations and absorptions of these excessively complex layers.

22. The spectrum of the corona, as I saw it in Egypt exactly answered to this description. Instead of the gradual smooth toning seen, say in the spectrum of the limelight, there were maxima and minima producing an appearance of ribbed structure, the lines of hydrogen and 1474 being, of course, over all. This observation, however, requires confirmation, for the look I had at the corona spectrum was instantaneous only.

23. This observation should certainly be repeated during future eclipses with the proper instrumental conditions, *i.e.* small intensely bright image on narrow slit and spectroscope of small dispersion. I believe that, under these conditions, photographs could readily be obtained with the new plates.

24. Now an eclipse occurs next May at a critical time of the sun's activity, for, so far as we can see, we shall be nearly at sun-spot maximum, and I hold that it will be a disgrace to our nineteenth century science, if efficient steps are not taken by those who are regarded as the leaders of science in this and other civilised countries to secure adequate observations.

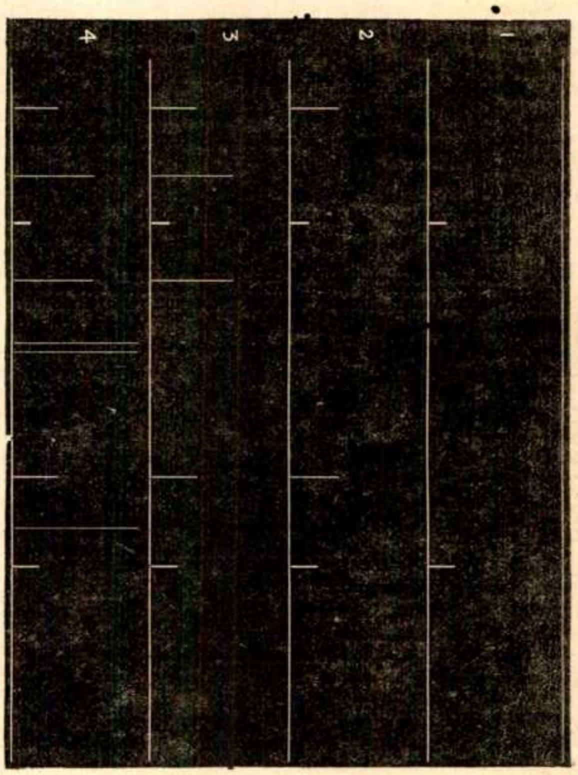

Fig. 6.

25. So far I have only referred to those special observations undertaken this year to discriminate between two rival hypotheses, but both hypotheses may be wrong in many points, so that we must not limit ourselves to such observations, but collect facts over the whole field, as has always been the custom in eclipse expeditions.

26. In my opinion the following scheme shows the observations which, in the present state of our knowledge, it is *most desirable* to secure. The scheme, I am aware, is by no means exhaustive. I give the observations in the order of importance I attach to them, having regard to the present position of solar theory and the conditions of eclipse observations.

(1.) 6-inch equatorial of long focus, perfect clockwork, spectroscope with dispersion of at least five prisms of 60°.

Clamp point of disappearance of sun at base of normal slit, and record phenomena observed from ten minutes before totality to actual totality.

a. Order in which lines appear.

b. Brightness and length when first visible.

The spectrum from λ 4800 to λ 5900 should be distributed among at least nine observers.

Repeat observations after totality on point of reappearance.

(2.) 6-inch photographic lens of 4-feet focus, perfect clock, same dispersion as above.

Clamp point of disappearance of sun on centre of tangential slit and record phenomena observed from ten minutes before totality to actual totality.

a. Order in which lines appear.

b. Brightness and length when first visible.

Repeat observations after totality on point of reappearance. Same part of spectrum, same distribution as in (1).

(3.) 6-inch photographic lens as in (2).

Photographic phenomena before and after totality on slowly ascending or descending or rotating plate, taking care to expose only narrow strip of plate.

(4.) Ditto. Spectroscope of small dispersion, long slit.

Photograph spectrum of corona during totality on both sides of dark moon.

(5.) Prismatic camera. 6-inch photo. lens as in (2), but with grating.

Use first order spectrum on one side and second order on the other.

Commence two minutes before totality. Continue till two minutes after totality on gradually ascending or descending or rotating plate.

(6.) 6-inch photo. lens as in (2), mounted on alt-azimuth. Fine slit. One prism of 60°. To observe spectrum of corona.

(7.) Photographs of corona of short, medium, and very long exposure to determine form and true solar limit of apparent corona due to the illumination of our air, using for the latter purpose the photographic intensity of the image of the moon.

I am aware that because Solar Physics is a new subject, and one so entirely in the domain of pure science, the above scheme may appear ridiculous to many, for if carried out in its completeness its cost would perhaps amount to the sixtieth part of the sum expended on the Transit of Venus in 1874. I have, however, felt myself bound to put it forward as an ideal scheme and one which, if several civilised Governments do each a little, concerted action may help us in part to realise. I am informed that the French and Italian Governments are already making preparations for observations, and my desire is that we may be represented on an occasion which, having regard to the duty which is incumbent upon us to secure observations for the use of those who come after us, is one of high importance.

SCIENTIFIC SERIALS

The American Naturalist, November, 1882, contains:—On the ancient man of Calaveras, by W. O. Ayres.—On the grey rabbit, by S. Lockwood.—On the genus Nebalia and its fossil allies, representing the order Phyllocarida, by A. S. Packard, jun.—American work on recent mollusca, 1881, by W. H. Dall.—Progress of invertebrate palæontology in the United States in 1881, by C. A. White.—On the number of bones at present known in the pectoral and pelvic limbs of birds, by R. W. Shufeldt.—The Editor's table—Recent literature.—General notes.

Zeitschrift fur wissenschaftliche Zoologie, Bd. 37, Heft 3, November 1, 1882, contains:—On the structure and development of *Dinophilus apatris*, by Dr. E. Korschelt (plates 21 and 22). The author would place the forms belonging to this genus in a new family of the Turbellaria.—Studies among the Lampyridæ, by H. Ritter v. Wielowiejski (plates 23 and 24).—On the deposition of bone in the skeleton of bony fishes, by Max Köstler (plate 25).—On the origin and development of the green cells in Hydra, by Dr. Otto Hamann (plate 26); see remarks on this paper by Prof. Lankester, NATURE, vol. xxvii. p. 87.

Bulletin de la Soc. Imp. des Naturalistes de Moscou, 1882, No. 1, contains:—On the geology of the Windimir district, by H. Trautschold.—New lepidoptera of the Amur land, by H. Christoph (conclusion).—On the stone-growth of Sarepta—list of the Staphylinidæ, and on some new plants of Sarepta, by A. Becker.—On the geographical distribution of the hop in ancient times, by Dr. C. O. Cech.—A protest relative to palæontological nomenclature, by H. Trautschold.—Remarks on some anomalies found in the form and colour of the plants in the various countries of the Russian territory, by Dr. A. von Riesenkampff.—Note on an instrument to measure the intensity of gravity, by A. Issel.—On crinoids, addenda and corrigenda, by H. Trautschold.—Materials for a fauna of the Black Sea, fasc. iii. Vermes, by V. Czerniavsky. In Russian, but the diagnoses of new genera and species are in Latin.

Revue internationale des Sciences biologiques, October 15, 1882, contains:—Translation of Prof. Pringsheim's "Researches on Chlorophyll."—M. Roujon, on the faculty of speech in mammals.—Prof. Abel, on the dangerous properties of fine coal dust (translation).—M. Viguier, on orientation and its organs in animals and in man.—*Proceedings* of the Academy of Sciences, Paris.

Rendiconto delle Sessioni dell' Accademia delle Scienze dell' Istituto di Bologna, 1881-82.—We note the following: On the succenturiate spleen of the dog, and on the reproduction of the spleen by pathological processes that have abolished the function of that viscus, by S. Tizzoni.—On adaptation of species to environment; new researches on the genetic history of Trematodes, by S. Ercolani.—On the craniology of lunatics, by S. Peli.—On congenital deviations of the vertebral column in domestic animals, by S. Gotti.—Function of the cœcum and the rest of the large intestine, by S. Vella.—On polydactylia and polymelia in man and vertebrates, by S. Ercolani.—On the variations and the course of the river Po, by S. Predieri.—Meteorology applied to the study of botany, with a description of a new geothermometer, by S. Bertoloni.—On some new electric figures, by S. Villari.—On electric shadows, by S. Righi.—On the minute anatomy of the muscles in insects which move their wings, by S. Ciaccio.—The elevation of the Bolognese Apennines by direct action of gravity and of lateral pressures, by S. Bombieri.—Experimental researches on nerve-stretching, by S. Rossi.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 23.—"Monthly Means of the Highest and Lowest Diurnal Temperatures of the Water of the Thames, and Comparison with the corresponding Temperatures of the Air at the Royal Observatory, Greenwich." By Sir George Biddell Airy, K.C.B., F.R.S., late Astronomer Royal.

The observations were instituted at the suggestion of the conductors of the Medical Department in the Office of the Registrar General of Births, Deaths, and Marriages, with the view of supplying some knowledge of an element which may possibly affect the sanitary condition of the metropolis. The plan of observations was arranged at the Royal Observatory of Greenwich; and the instruments were procured and mounted, and repaired when necessary, under the care successively of James Glaisher, Esq., and William Ellis, Esq., superintendents of the Magnetical and Meteorological Department of the Observatory. The self-recording instruments were attached to the hospital ships successively anchored in the Thames, nearly opposite to Greenwich; and their records were read and registered by the medical officers of those ships, and these written registers were transmitted every week to the Royal Observatory.

I have been favoured by Mr. Ellis, who, at my request, has kindly superintended the preparation of the results of observations of thermometers in the water of the Thames, with the following remarks on the nature of the observations and the elements for their reduction.

"The thermometers were inclosed in an upright wooden trunk attached to the side of the ship, its lower portion projecting into the water; the trunk was closed at the bottom; the closing plate, and that portion of the sides which was under water, being perforated with holes, to allow the water easily to flow through. The thermometers were suspended in the trunk, so as to be about two feet below the surface of the water, and one foot above the bottom of the trunk.

"The instruments employed throughout were, one for highest temperature, and one for lowest temperature. For highest temperature two constructions have been successively used: the earlier, in which the mercury, with rising temperature, pushes up a steel index, leaving it detached when the temperature falls; the later, in which the column of mercury becomes divided on fall of temperature, the principal portion of the column being left in the tube. For lowest temperature, a spirit thermometer was employed, its index being contained within the column of spirit. The index-errors of the two thermometers in use were properly determined, and corrections for them were applied when necessary.

"The thermometers were read every morning at 9 a.m.

"The observations of atmospheric temperature at the Royal Observatory were made with the thermometers in ordinary use at the elevation of 4 feet above the ground."

It will be remarked that the indications of the thermometers in the Thames were read only once in each day. I could have wished that a greater number of readings could have been taken, sufficiently numerous to exhibit the dependence of the temperature of the Thames-water upon the phase of the tide. But under the circumstances this was impracticable. To establish a self-registering apparatus was out of question; and if on a few occasions we had gone through the labour of making observations at every hour of day and night, the conclusions deduced from those few instances might have been vitiated by accidents. But I am able to assert positively, as a result from the reductions to be exhibited in the following pages, that nothing has been lost from the restriction of the plan of observation. It will be seen that the daily change of temperature, produced by the aggregate of strictly diurnal change (depending on the solar hour) and tidal change (depending on the moon's apparent position) is so small that it is impossible to attach with any certainty a sensible value to either of these causes.

I now proceed to describe the principal steps in the reduction of the observations.

In the weekly publication of these observations by the Registrar General, the weekly means of each observed element were also exhibited. In preparation for a detailed publication of the whole, I had the entire series of these weekly means collected, each being accompanied with notes of the principal phases of the moon, the occurrence of remarkable storms, &c., occurring within the week. (This *résumé* exists, and is available for any discussion which might be suggested; I propose to offer it for deposit at the Royal Observatory.) But on general examination of the collected means, I did not perceive that any result could be expected which would justify the labour and expense of printing the whole. For instance, if there were any remarkable dependence on the phase of tide, different values for the "excess of mean temperature of the water above mean temperature of the air" would occur in the weeks which included respectively new moon, first quarter, full moon, third quarter, and these would recur with little alteration for several months. But on general examination, I do not see anything which would justify more technical discussion directed to this point. Finally I decided on exhibiting only the means of deductions as to temperature for each calendar month, and omitting all other phenomena. As the succession of weeks and the succession of entire months do not generally coincide, the rule was established to adopt the first entire week in each calendar month as the first of the weeks to be used, in conjunction with three or four weeks following, to form the monthly mean. Thus, some months contain four weeks, and some contain five weeks. For instance, the month of March, 1846, contains the five weeks, March 1–7, 8–14, 15–21, 21–28, 28–April 4; but the next month contains only the four weeks—April 5–11, 12–18, 19–25, 26–May 2.

By this system, the results, as far as they appear to possess any value, are brought into the compass of five convenient Tables of Double Entry, which, with their columnar and lateral means, appear to give all the information that can be desired. The contents of the several tables are:—

Table I. Monthly Mean Temperature of the Water of the Thames.
Table II. Monthly Mean Atmospheric Temperature at the Royal Observatory.
Table III. Monthly Mean Excess of Thames Temperature above Observatory Atmospheric Temperature.
Table IV. Monthly Mean of Diurnal Range of Temperature of the Water of the Thames.
Table V. Monthly Mean of Diurnal Range of Atmospheric Temperature at the Royal Observatory.

The last line only of each of these tables is given in the present communication to NATURE.

Monthly Means, through a Range of Thirty-five Years, of the Principal Elements of the Temperature of the Water of the Thames

Month.	Temperature of the water of the Thames.	Atmospheric temperature at the Royal Observatory.	Excess of temperature of the Thames above atmospheric temperature.	Diurnal range of temperature of the Thames.	Diurnal range of atmospheric temperature.
January	39°·4	38°·9	+0°·5	1°·9	9°·6
February ...	40·7	40·4	+0·3	2·0	11·5
March	43·6	42·8	+0·8	2·0	15·0
April	50·0	48·7	+1·3	2·3	18·5
May	56·3	54·4	+1·9	2·4	19·9
June	62·6	60·6	+2·0	2·2	20·5
July	65·7	63·9	+1·8	2·1	21·2
August	64·4	62·6	+1·8	2·0	19·6
September ...	59·9	57·9	+2·0	1·9	18·1
October	52·9	50·7	+2·2	2·0	14·3
November ...	44·3	42·3	+2·0	2·1	11·3
December ...	40·4	39·8	+0·6	2·1	9·1

And the following appear to be the legitimate epitomised inferences:—

(1). The mean temperature of the Thames-water is higher than that of the Observatory thermometers by 1°·5. But the locality of the Observatory thermometers is, in hypsometrical elevation, about 160 feet above that of the Thames thermometers. It would seem probable therefore that the mean temperature of the water is higher than the climatic temperature by only a small fraction of a degree.

(2). This difference is not uniform through the year. With some irregularities, the greatest excess of Thames temperature occurs in September, and the least in February. But the autumnal difference exceeds the spring difference by only 1°·6. It seems not improbable that this is the effect of a slight communication with the sea, whose surface-waters have accumulated in autumn the effect of solar radiation through the summer; with contrary effect at the opposite season.

(3). The mean range of temperature through the day is 2°·1. And this expresses the numerical change from the lowest solar temperature, or the lowest temperature in the first tide, or the lowest temperature in the second tide (whichever may be the lowest), to the highest solar temperature, or the highest temperature in the first tide, or the highest temperature in the second tide (whichever may be the highest). It is evident that the change of temperature due to the diurnal change of solar action, and the change of temperature due to each of the tides, must each, individually, be very small.

(4). It appears to me that the fundamental inference must be this: that the material water is very little changed at Greenwich by the tide. Although a vast body of water rushes up at every flow, running with great speed, and sometimes raising the surface by 20 feet, yet nearly the same water runs down at ebb, and is again brought up, with all its contents, at the next flow. These expressions are to be taken as modified by the descent of fresh water from the land; but the amount of that water must be small, in comparison with the mass which it joins in the Thames at London.

(5). I do not imagine that the tidal action has any beneficial effect on the climate of London, except that probably the agitation of the water produces mechanical agitation of the air, and thus destroys injurious stagnation.

Mathematical Society, December 14.—Prof. Henrici, F.R.S., president, in the chair.—Messrs. T. Woodcock, Hugh Fraser, Major Allan Cunningham, R.E., and Capt. P. A. Macmahon, R.A., were elected members.—The chairman announced that in consequence of ill-health Mr. C. W. Merrifield, F.R.S., had been obliged to resign the office of treasurer, and that the council had elected Mr. A. B. Kempe, F.R.S., to undertake the duties of the office. Dr. Hirst spoke in feeling terms of the work Mr. Merrifield had done for the Society, and in accordance with his suggestion a vote of thanks for his services in the past, and of condolence with him on account of the reasons which had led him to sever his official connection with the Society was carried.—The following communications were made:—On the vibrations of a spherical shell, by Prof. H. Lamb.—On the absolutely least periods of the elliptic functions, by Prof. H. Smith, F.R.S.—On certain relations between volumes of loci of connected points, by Mr. E. B. Elliott.—Geometrical proof of Griffiths' extension of Graves' theorem, by Mr. J. J. Walker.—On polygons circumscribed about a tricuspidal quartic, by Mr. R. A. Roberts.—Note on an exceptional case in which the fundamental postulate of Prof. Sylvester's theory of Tamisage fails, by Mr. J. Hammond.—On certain quartic curves, which have a cusp at infinity, whereat the line at infinity is a tangent, by Mr. H. M. Jeffery, F.R.S.

Zoological Society, November 28.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. W. B. Tegetmeier exhibited and made remarks upon the skull of a rhinoceros from Borneo; also the horns of a buffalo and deer from the same country.—Mr. J. E. Harting exhibited a specimen of the South African Eagle-Owl (*Bubo maculosus*), said to have been obtained many years ago near Waterford in Ireland.—Mr. R. Bowdler Sharpe exhibited and made remarks on some specimens of Swifts from the Congo. Mr. Sharpe also exhibited a specimen of *Machærhamphus alcinus* which had been obtained in Borneo by Mr. Everett.—A communication was read from Prof. Owen, C.B., on the sternum of *Notornis* and on sternal characters.—A communication was read from Dr. A. B. Meyer, C.M.Z.S., in relation to the adoption by naturalists of an international colour-scale in describing the colours of natural objects.—A communication was read from Dr. W. Blasius, of Brunswick, containing a description of a small collection of Birds made by Dr. Platen in the island of Ceram. The collection contained 49 specimens referable to 21 different species, one of which was new to the fauna of Ceram.—A communication was read from Mr. E. P. Ramsay, containing the description of a new species of *Monarcha* from the Solomon Islands, proposed to be called *Monarcha* (*Piezorhynchus*) *browni*.—Mr. W. Bancroft Espeut read a paper on the acclimatisation of the Indian Mungoos (*Herpestes griseus*) in Jamaica. The author explained that the object in introducing the Mungoos into Jamaica was the destruction of the rats, which had committed serious ravages among the sugar and coffee crops. The first Mungooses were introduced in 1871, and so beneficial was the effect produced, that the saving to the sugar and coffee planters now was estimated at least at 100,000*l*. a year.—Lieut.-Col. Godwin-Austen read a paper describing specimens (male and female) of *Phasianus humiæ*, Hume, which had been obtained by Mr. Ogle on the peak of Shiroifurar in North-East Munipur, upon the Naja Hills.—A communication was read from Mr. A. Thomson containing the results of some observations made by him during the rearing of a species of Stick-insect (*Bacillus patellifer*) in the Society's Insect-house.

Chemical Society, December 7.—Dr. Gilbert, president, in the chair.—The following papers were read:—On the condensation products of oenanthol, by W. H. Perkin, jun. The author has endeavoured to obtain evidence as to the constitution of these bodies. By the action of dilute alcoholic potash on oenanthol, an acid, $C_{14}H_{26}O_2$, was formed, boiling at 270°-298°, and two aldehydes, $C_{14}H_{26}O$, boiling 277°-279°, and $C_{28}H_{50}O$, boiling 330°-340°. Zinc chloride forms with oenanthol principally $C_{14}H_{26}O$; nascent hydrogen converts this last substance first into an alcohol, $C_{14}H_{28}O$, and finally into the alcohol, $C_{14}H_{30}O$. Alcoholic potash converts $C_{14}H_{26}O$ into heptylic acid and an acid, $C_{14}H_{26}O_2$. The author concludes that the substance $C_{14}H_{26}O$ is hexylpentylacrylic aldehyd.—On the condensation products of isobutyl aldehyd, by W. H. Perkin, jun. Fossek has also recently worked on this subject, but has used aqueous potash, the action of which seems to be very different from that of alcoholic potash. Thus the latter forms an acid, $C_{12}H_{22}O_3$, not solidifying at − 10°. Fossek obtained with aqueous potash an acid, $C_8H_{16}O_3$, melting at 75°. The author prepared an aldehyd, $C_{12}H_{22}O_2$, and from this, by nascent hydrogen, an alcohol. By the action of stronger potash upon isobutyl-aldehyde, higher condensation products were obtained.—On a condensation product of phenanthraquinone with ethylic acetoacetate, by F. R. Japp and F. W. Streatfield. This substance has the formula $C_{20}H_{16}O_4$, and crystallises from benzene in white silky needles, fusing at 185°; it is ethylicphenanthroxyleneacetoacetate; by treatment with hydriodic acid it forms ethylicphenanthroxylenisocrotonate, fusing at 124°. A new acid and a new compound, which the authors believe to be the desoxybenzoin of phenanthrene, have also been obtained.—On the constitution of lophin, by Dr. Armstrong. The author considers that the symmetrical formula proposed by Radziszewski is to be preferred to that proposed by Dr. Japp.—On the constitution of basic ferric sulphate, by S. U. Pickering. By the determination of its molecular weight, this salt has the formula $Fe_2(SO_4)_3, 5Fe_2O_3$.—On the chemistry of Hay and "Ensilage," by F. W. Toms.—On certain brominated carbon compounds obtained in the manufacture of bromine, by S. Dyson. In a bye product the author has detected carbon tetrabromide, bromoform, and chlorobromoform;—Note on the preparation of diphenylenketone ether, by W. H. Perkin.

Anthropological Institute, November 28.—General Pitt-Rivers, F.R.S., president, in the chair.—Dr. W. G. Parker read a paper on the language and people of Madagascar. The language belongs to the Malayo-Polynesian group, being most nearly allied to the Malay proper. The various dialects, numbering more than sixteen, are essentially only one language. It is soft, musical, phonetic, and easily learned by Europeans. Until the early part of the present century it was a spoken language only, but the English missionaries reduced it to its present form, our own English alphabet being adopted, with the exception of the letters *c*, *q*, *u*, *w*, *x*, which have no equivalent sounds in Malagasy. The vowels are four in number, and the consonants sixteen, pronounced as in English, with the exception of *g*, which is always hard (as in *gate*), and *j*, which has the sound of *dz* (as in *adze*). There are only two real diphthongs. In pronunciation every vowel or diphthong must be clearly sounded, and the accents properly placed, because often the alteration of one vowel, or of the place of the accent, is the only means of distinguishing similar sounding words. The author then gave the six chief rules of syntax, and explained the grammatical structure of the language. In the second part of the paper the peculiar geographical position of Madagascar was first noticed. Its estimated population (from four to four and a half millions), and its chief structural features, with a special notice of the central plateau. There are a great many tribes in Madagascar, but all are divisible into two distinct classes, according to their race-origin, Malay and African. Their forms of government are (1) petty absolute monarchies over the greater part of the island; (2) among the Hovas tribe it is nominally an absolute monarchy, really an oligarchy, the head of which has almost regal power. The office of *Prime Minister* is not peculiar to the Hovas, tribes on the north and west coasts also possessing the same institution; but only among the Hovas is the Prime Minister not only the factotum, but also the "ex-officio husband to the queen." A short sketch of the new code of Hova laws was next given, this being the only tribe which possesses a code of laws. An outline of the history of Madagascar was given, showing the origin of the present form of government among the Hovas, the tribe which seeks to possess the entire island. Lastly, reference was made to the French claims against Madagascar, now being put forward, and their effect upon British interests. These claims are: (1) the demand that French subjects should be allowed to buy, sell, and own land in Madagascar; (2) the claims of private individuals; (3) the establishment of a French Protectorate over a large part of the island. The French are now acting in accordance with a preconcerted (and published) plan for invading and conquering the whole of the island. As affecting the interests of the British Empire, the possession of Madagascar by France will enable that Power, if at war with us, to endanger or even stop our lines of communication with our Indian, Australian, and other colonies, by the Red Sea and the Cape of Good Hope route. In the discussion that followed the Rev. James Sibree, the Rev. W. C. Pickersgill, Prof. Gustav Oppert, Mr. A. H. Keane, and others took part.

BERLIN

Physiological Society, November 24.—Prof. Du Bois-Reymond in the chair.—Dr. A. Fraenkel read a paper upon the further results of experiments which he had made in conjunction with Dr. Geppert to determine the influence of a rarefied atmosphere upon the animal organism. Some of the results of these investigations had been brought before the last meeting of the Society by Dr. Geppert (*antea*, p. 120). Besides the general phenomena and the behaviour of the gases of the blood in animals which breathe in a rarefied atmosphere, investigations were made as to the influence of rarefaction upon blood-pressure. The blood-pressure was read off upon a manometer which was outside the box in which the animal, the subject of experiment, was kept exposed to various degrees of rarefaction. One arm of the manometer communicated through the side of the box with an artery of the animal, while the other arm was in communication with the general cavity of the box. When the atmospheric pressure sank to half the normal amount, the blood-pressure showed no change; when the pressure sank to a third of an atmosphere, a small rise took place in the blood-pressure. This rise, however, passed away during the sleep that occurred under the influence of this amount of rarefaction, and the pressure became normal again. When the air was still further rarefied till the pressure was as low as one quarter of an atmosphere or less, the pulse became weak and small, the blood pressure went down, and then if normal quantities of oxygen were not quickly restored, the heart stopped. The chief aim of the whole investigation was the definite determination of the influence of a rarefied atmosphere upon metastasis (Stoffwechsel), upon which question, up to the present, only few, and even these contradictory, data were existent. The authors agreed in general with M. Paul Bert, in regarding the effect of a rarefied atmosphere as inducing a chemical change which was brought about by a diminished supply of oxygen. The amount of urea secreted in the twenty-four hours was taken as the measure of metastasis. During a lengthened period of observation on those days in which the animals thus experimented on had the same amount of food, the quantity of urea secreted in the twenty-four hours remained constant. Nor was there any alteration in the amount of urea when they were exposed to variations of pressure down to half an atmosphere.

On the diminution of the pressure to one third of an atmosphere, at and under which pressure the amount of oxygen contained in the blood is markedly diminished, and the animal falls into a deep sleep, there was, after this degree of rarefaction had lasted several hours, a very remarkable increase in the amount of urea. This increase did not occur till the next day in the case of animals which had been fed, whereas it occurred on the day of the experiment in the case of those animals which were kept hungry, but it in all cases lasted over a couple of days after the experiment. Dr. Fraenkel's belief is that the rarefaction influences the metastasis by depriving the blood and the tissues of some of their necessary oxygen, and that this want of oxygen entails an excessive destruction of albumen, the constituents of which are in part deposited as fat, and in part are changed into urinary products. Besides the increased elimination of urea, fatty degeneration of tissues (*e.g.* of the heart) is observed when the system is in want of oxygen.

PARIS

Academy of Sciences, December 11.—M. Jamin in the chair.—M. Faye presented the *Connaissance des Temps* for 1883, and noted some improvements.—Observation of the transit of Venus in the Argentine Republic, by M. Mouchez. Good observations were made at the two stations, by MM. Beuf and Perrin.—M. Mouchez stated that the weather prevented observations on the Pic du Midi.—Installation and preliminary operations of the mission for observation of the transit of Venus, at Fort-de-France, by M. Tisserand.—Observations of the transit at Marseilles Observatory, by M. Stephan. There were five observers. The phenomenon was seen through a veil of vapours, and M. Stephan does not think this unfavourable; perhaps, indeed, it is the best condition possible (the solar intensity being weakened), if the observer do not lose his *sang-froid*, through fearing too great obscuration. The black drop was not seen. (Data for the first two contacts are furnished.)—New facts concerning rabies, by M. Pasteur, with MM. Chamberland, Roux and Thuillier. All forms of rabies come from the same virus. Death after inoculation with rabid saliva may be either from a microbe found in the saliva, from formation of much pus, or from rabies. The virus of rabies is found not only in the *medulla oblongata*, but in the brain and spinal cord. Rabies may be produced certainly and quickly, either by trepanation and inoculation, or by intravenous injection. The symptoms are different in the two cases. Animals sometimes recover after the first symptoms of rabies, never after the acute symptosm. The authors have now four dogs which cannot take rabies, however inoculated. Whether this is from having had a mild form of it and recovered, or from being naturally refractory, he cannot at present say.—Separation of gallium, by M. Lecoq de Boisbaudran.—New studies tending to establish the true nature of *glairine* or *barégine*, and the mode of its formation in the thermal and sulphurous waters of the Pyrenees, by M. Joly. The concrete glairine of chemists, found in all those waters, consists essentially of detritus of a host of animals and plants, with some inorganic substances (crystals of sulphur, sulphate of iron, silica, &c.), and very often *Sulphuraria*, a true *Oscillator*.—On the conservation of solar energy; reply to Dr. Siemens, by M. Hirn. —Examples of black seen as orange red, by M. Trécul. A lady's black veil, in full sunlight, seemed orange red at the nodes of the tissue, while the internal parts remained black. The dye in this case was a very dark blue; and the orange-red is complementary.—Effects of lightning on the top of the Puy-de-Dôme, by M. Alluard. The anemometer cups (of red copper) at the top of an iron mast (6 m. high, and having a ladder and stand largely iron; also connected with earth plates), all show traces of fusion in their upper parts, and the fused matter is raised as a cone. (St. Elmo's fire often appears at the salient points of the mast, &c.) —Observations made during the viticolar campaign 1881–82, by M. Boiteau.—Factitious purulent ophthalmia produced by the liquorice liana, or jequirity, by M. Moura-Brazil.—M. de Lesseps presented a note on M. Wiener's explorations in the regions of the Amazon.—The Secretary read telegrams from Brazil, Washington, Oran, Nice, Bordeaux, &c., regarding the transit of Venus.—Observation of the transit at Châtsandun, by M. Lescarbault. He describes a greyish yellow luminous fringe seen all round the planet when this was three-fourths on the sun; it persisted till entrance was completed.—Observations of the transit at the Roman College, by P. Tacchini. He observed the contacts successfully with the spectroscope applied to a Merz refractor; while Prof. Millosevich observed in the ordinary way. He verified the absorption by the Venus atmosphere, found the planet's diameter $67''{\cdot}25$, &c.—Observations of solar spots and faculæ during the third quarter of 1882, by the same. The spots show a diminution, with well-marked secondary minimum in August. The faculæ had nearly the same extension as in the preceding quarter. While their size diminishes, that of the spots increases.—On the great solar spot of November, 1882, and the magnetic perturbations that accompanied its appearance, by the same. It showed maximum activity at the middle of the disc, and afforded the rare opportunity of distinguishing solar protuberances in the disc as easily as on the limb.—Observations of the great Southern Comet, by M. Jacquet. A sketch is furnished, taken on board the *Niger*, at the mouth of the Rio de la Plata.—On Fourier's series, by M. Halphen.—On solids of equal resistance, by M. Léauté.—On a communication of M. Deprez, relative to the transport of force, by M. Lévy.—Displacements and deformations of sparks by electrostatic actions, by M. Righi.—On the atomic weight of yttrium, by M. Cléve. He obtains 89·02 and 88·9, with different values for O and S.—On a fish at great depths in the Atlantic, the *Eurypharynx pelecanoides*, by M. Vaillant. This was brought up from a depth of 2300 m. off the coast of Morocco, during a cruise of the *Travailleur*. It is about a foot and a half long, quite black, and remarkable for its enormous and disproportionate mouth (like a pelican's). It has no swimming-bladder and few fins, peculiar branchiæ, &c. The genus *Malacosteus* seems the nearest.—On a new fossil insect of the order of Orthoptera, from the coal-mines of Commentry (Allier), by M. Brongniart. It is of remarkable size, and is named *Titanophasma Fayoli*. The author canot say whether it was winged or not.—On the meteorological fauna of the Varangerfjord, by MM. Pouchet and de Guerne.—The Suctociliates, a new group of Infusoria, intermediate between the Ciliates and the Acinetians, by de Merejkowsky.—Influence of excitability of muscle on its mechanical work, by M. Mendelssohn. For a certain tension the mechanical work of a single contraction of a more excitable muscle is greater than that of a muscle of normal excitability. But the number of successive works the former can do till exhaustion, and their sum total, is less; and the length of time it can perform a series of works with a given load, till exhaustion, is less.—Végetation of wheat by M. Risler.—Conditions of production of Epinastia in leaves, by M. Mer.

CONTENTS

PAGE
DISEASES OF MEMORY. By GEORGE J. ROMANES, F.R.S. . . . 169
EASTERN ASIA. By A H, KEANE 170
OUR BOOK SHELF:—
  Taschenberg's "Die Insekten nach ihren Schaden und Nutzen" . 172
  Potts's "Out in the Open" 172
  Anderson's "Catalogue of Mammalia in the Indian Museum, Calcutta" 172
  Houghton's Microscope and some of the Wonders it Reveals" . . 173
  Robinson's "Flora of Essex County, Massachusetts" 173
  Jones's "Catalogue of the Fossil Foraminifera in the British Museum (Natural History)" 173
LETTERS TO THE EDITOR:—
  The Aurora and its Spectrum.—Hon. RALPH ABERCROMBY . . . 173
  Swan Lamp Spectrum and the Aurora.—J. MUNRO 173
  The Meteor of November 17—W. M. F. P. 173
  Invertebrate Casts.—Dr. H. A. HAGEN (*With Diagram*) . . . 173
  The Scream of the Young Burrowing Owl sounds like the Warning of the Rattlesnake.—S. GARMAN 174
  Fertilisation of the Common Speedwell—A MACKENZIE STAPLEY . 174
  Complementary Colours at the Falls of Niagara.—H. G. MADAN . 174
M. DUMAS 174
THE METEOROLOGICAL OBSERVATORY ON BEN NEVIS 175
NOTES ON THE GEOLOGY OF HONGKONG. By F. WARRINGTON EASTLAKE 177
TRANSIT OF VENUS, 1882—BRITISH EXPEDITIONS. By E J. STONE, F.R,S.; Prof S P. LANGLEY; JOHN BIRMINGHAM 177
NOTES 180
OUR ASTRONOMICAL COLUMN:—
  Measures of Double Stars 182
PHYSICAL NOTES 182
CHEMICAL NOTES 183
M. MIKLUKHO-MACLAY ON NEW GUINEA 184
THE RECENT AND COMING TOTAL SOLAR ECLIPSES. By J. NORMAN LOCKYER, F.R S. (*With Illustrations*) 185
SCIENTIFIC SERIALS 189
SOCIETIES AND ACADEMIES 189

THURSDAY, DECEMBER 28, 1882

MATHEMATICS IN AMERICA

American Journal of Mathematics, Pure and Applied. Published under the Auspices of the Johns Hopkins University. Vols. III. and IV. (Baltimore: Isaac Friedenwald.) And other Mathematical Journals.

THE *American Journal of Mathematics* was established in 1878 under the auspices of the Johns Hopkins University at Baltimore, and four handsome quarto volumes of 400 pages each have now been published. Prof. Sylvester was editor-in-chief of the first three volumes, being assisted by Mr. Story as editor-in-charge, but the last volume bears Sylvester's name alone as editor.

A notice of the first two volumes of the Journal appeared in NATURE, vol. xxii. p. 73, and the hope was there expressed that it might have as great a future before it as awaited Crelle's Journal half a century before. A careful examination of the last two volumes shows that the promise of the earlier volumes has been so far maintained, and that the Journal has already acquired a distinctive character of its own. It almost invariably happens that mathematical journals exhibit marked characteristics, and that certain branches of the subject occupy a pre-eminent position. One paper leads to another relating to the same questions, and the original bias of a journal is generally due, both directly and indirectly, to its editor, as authors naturally prefer to send contributions where they are more likely to be understood and appreciated. That this is especially the case with the American Journal is what we should expect, as besides being the principal contributor, the editor is professor in the institution with which it is connected, and many of the papers are by his former pupils and colleagues. Although a very distinct tendency is thus evident in the direction of the large group of subjects (and more particularly Higher Algebra and Higher Arithmetic) with which the name of Prof. Sylvester is associated, it is not to be supposed that the Journal has become narrow in its scope. On the contrary, the whole range of mathematical subjects is very fairly represented, as will appear from the following paragraphs, which contain a list of the papers in vols. iii. and iv., an attempt being made to group them to some extent according to subjects.

The arithmological papers are numerous. Prof. Sylvester gives closer limits for a quantity which occurs in Tchebycheff's well-known investigation of the number of primes inferior to any given prime; he contributes also a note on the trisection and quartisection of the roots of unity, and an instantaneous proof of a theorem of Lagrange's on the divisors of a certain quadratic form. In a paper on a point in the theory of vulgar fractions he gives a method of developing any vulgar fraction as a sum of certain special fractions, each having unity as its numerator. This development he terms a sorites, and he remarks that it was suggested to him by the chapter in Cantor's *Geschichte der Mathematik,* which gives an account of the singular method in use among the ancient Egyptians for working with fractions: it was their curious custom to resolve every fraction into a sum of simple fractions according to a certain traditional method, which however only leads in a few simple cases to a sorites. There are two papers by Mr. O. H. Mitchell, both relating to the theory of congruences: one of them contains a generalisation of Fermat's and Wilson's theorems. There is also a short note by Prof. Newcomb on the relative frequency of the occurrence of the digits as leading figures in logarithmic tables.

The contributions to the higher algebra occupy a very conspicuous place. The important tables of the generating functions and ground forms of binary quantics which have been calculated by Prof. Sylvester and Mr. F. Franklin, with the aid of a grant from the British Association, are continued. Mr. Franklin is also the author of a separate paper, in which he gives a consecutive account of the methods, due to Cayley and Sylvester, of calculating the generating functions for binary quantics and thence determining the number of fundamental invariants and covariants of any order and degree. Prof. Sylvester gives a determination of the impossibility of the binary octavic possessing any ground form of degree 10 and order 4. There is a paper on the 34 concomitants of the ternary cubic by Prof. Cayley, who also gives a specimen of a literal table for binary quantics and certain tables for the binary sextic; and there are some notes on Modern Algebra by M. Faà de Bruno, of Turin. Mr. Mitchell and Mr. T. Muir, of Glasgow, give theorems relating to determinants.

Prof. Wm. Woolsey Johnson is the author of a paper on strophoids. The term strophoid has been applied by French writers to a cubic curve, the symmetrical form of which Dr. Booth discussed under the name of the Logocyclic curve. The author gives to the term a more extended signification, and defines a strophoid as the locus of the intersection of two straight lines which rotate uniformly about two fixed points in a plane. Dr. Booth's curve is included as a particular case of the class of curves which Prof. Johnson terms right strophoids. Prof. Sylvester considers the theory of rational derivation on a cubic, and Mr. Story is thus led to discuss the subject more fully in a separate memoir: the points on the curve which are considered are those whose coordinates can be expressed as rational functions of an arbitrary initial point on the curve. Mr. Samuel Roberts contributes a paper on the generalisation of local theorems, in which the generating point divides a variable linear segment in a constant ratio; and there is a note by Miss Christine Ladd on segments made on lines by curves.

There are three papers on solid geometry, all by Mr. T. Craig: they relate to the orthomorphic projection of an ellipsoid upon a sphere, to certain metrical properties of surfaces (in n dimensions as well as in three dimensions), and to the counter-pedal surface of an ellipsoid. The surface which the author designates the counter-pedal is the locus of the intersections of central planes parallel to the tangent planes of the ellipsoid with the normals at the corresponding points of contact; its equation is worked out, and is found to be of the tenth order. Mr. E. W. Hyde contributes a note on the centre of gravity of a solid of revolution, and there is a discussion by Prof. Stringham on the regular figures in n-dimensional space.

Prof. Cayley gives a note on the analytical forms or ramifications which he has termed trees. This is a subject which has applications to the theory of chemical combinations, and it is one to which Prof. Cayley has already devoted attention, a long memoir of his upon it having appeared in the Report of the British Association for 1875. Prof. Cayley also is the author of a short paper on certain imaginaries connected with the product of two sums of eight squares. Among papers on general analysis should also be mentioned a determination by Prof. Stringham, of the number of possible finite groups of quaternions, a group being defined as in the theory of substitutions; and a note by Mr. Story on non-Euclidean geometry.

A number of papers relate to the differential calculus. There are two by Prof. Sylvester on the solution of classes of difference and differential equations No less than four relate to development in series by Taylor's and other expansion theorems and the forms of the remainder: these are by Mr. J. C. Glashan of Ottawa, Mr. McClintock, and Mr. A. W. Whitcom. Prof. Crofton gives some remarkable theorems involving symbols of operation, and Mr. J. Hammond considers the theory of general differentiation, a subject that received attention from Liouville, Peacock, and others half a century ago, but which has attracted but little notice in recent times. Mr. Franklin contributes a short note on Newton's method of approximating to the roots of equations, and Mr. Glashan gives certain formulæ relating to the change of the independent variable in differentiations.

More than one whole number is devoted to a reprint of the late Prof. Benjamin Peirce's valuable memoir on Linear Associative Algebra, of which only a small number of copies in lithograph were made in the author's lifetime for circulation among his friends. This well-known paper was read before the National Academy of Sciences at Washington in 1870: it is here reproduced with notes and addenda by Mr. C. S. Peirce, the son of the author, and occupies 133 pages. Mr. C. S. Peirce himself, who is known not only by his logical writings but by his stellar photometric researches, is also the author of two papers, the one on the algebra of logic and the other on the logic of number. In connection with this subject a paper by Miss Ladd on De Morgan's extension of the algebraical processes should be noticed.

There is a short bibliographical paper relating to Alhazen's problem by Mr. Marcus Baker. The problem is from two points in the plane of a circle to draw lines meeting at a point on the circumference, and making equal angles with the tangent at that point. The author also gives an extension of the problem to the sphere.

Only three papers belong to mathematical physics. One, by Prof. H. A. Rowland, relates to the general equations of electromagnetic action, with application to a new theory of magnetic attractions and to the theory of the magnetic relation of the plane of polarisation of light. The other two are on hydrodynamics, they are by Prof. Rowland and Mr. Craig, and relate respectively to the motion of a perfect incompressible fluid and to certain possible cases of steady motion in a viscous fluid. There are some notes on moving axes by Prof. Loudon of Toronto; and Prof. Sylvester considers the theory of mechanical involution, which is the name he has given to the relation between six lines in space so situated that forces may be made to act along them whose statical sum is zero. Linkages form the subject of a paper by Mr. F. T. Freeland.

Astronomy is represented by two papers: one by Prof. Newcomb, on a method of developing the perturbative function of planetary motion; and the other by Mr. G. W. Hill, on Hansen's general formulæ for perturbations. The object of the former paper is to exhibit a method of effecting the development in powers of the eccentricities. The author remarks that in consequence of the complex character of the series this development has been but little used even in the cases of nearly circular orbits, when its application would be most convenient, but that, as the disturbing force is given as an explicit function of all the elements, it is of more interest to the mathematician than any other. In his method of development Prof. Newcomb directs especial attention to the expression of the coefficient of each power of the eccentricity in terms of the coefficients of lower powers, and to the expression of the coefficient in each term involving the perihelia of two planets as the symbolic product of coefficients involving the perihelion of one only. Mr. Hill's paper contains a transformed form of Hansen's expression for the perturbation of the mean anomaly, which is more simple and more convenient for computation. There is also a short note by Mr. Ormond Stone relating to formulæ in elliptic motion.

The titles of the papers speak for themselves, and but little comment is needed. It will be seen that the two volumes represent a considerable amount of mathematical work, a fair proportion of which may have real influence on the advancement of the science. Some of the papers, as must evidently be the case, are needlessly pretentious in form, and the new matter they contain might be advantageously stated in less space. The effect of Professor Cayley's visit to Baltimore is apparent in the papers which occur in the last number issued, and we believe that the lectures which he gave at the Johns Hopkins University will shortly appear in a future number.

The dates which the numbers of the Journal bear are the dates when they ought to have appeared, assuming it to be published quarterly in March, June, September, and December, and not the dates when they did actually appear. Thus the last number issued bears date December, 1881, but in the case of this number the inconvenience attending so great a discrepancy between the nominal date and the date of publication is partially remedied by the words "Issued July 18, 1882," at the foot of the last page. It seems a pity to retain the nominal dates on the wrappers, as they may be misleading. It will be difficult to regain the lost time, and there is but little advantage in stating the time when the number should have appeared. The volume and number and date of publication are all that need to be given.

On the wrappers of the numbers of vol. iv. appears an announcement in which a prize of 1500 francs and a perpetual free subscription to the journal from its commencement are offered to the first person who, before January 1, 1883, discovers and transmits to the Editor a valid proof or disproof of the proposition that a ground form and a syzygant of the same degree and order cannot appertain to the same binary quantic, "provided that the Editor shall not himself have previously discovered the same, and given public notice thereof." The truth of this proposition has been assumed as a fundamental postulate in the calculation of ground forms, and its importance cannot be over-estimated. It is, however, somewhat of an anachronism to draw attention to it by the offer of a prize. Such prizes exist in Universities and in the older academies, but by many they are not regarded with much favour. It seems unlikely that any competent person would be tempted to investigate the subject by hope of the reward. Pure mathematics offers no mercenary inducements to its followers, who are attracted to it by the importance and beauty of the truths it contains; and the complete absence of any material advantage to be gained by means of it, adds perhaps even another charm to its study.

The late Prof. Benjamin Peirce denoted the base of the Napierian logarithms and the ratio of the circumference to the diameter of a circle by two special symbols turned opposite ways, somewhat resembling a 6 and a 6 reversed. The forms of these symbols would seem to imply that 2·71828... and 3·14159... were regarded as allied to one another, and in some reciprocal or inverse manner too, though it is not easy to see what the author's point of view was. Two writers in the *American Journal* use Prof. Peirce's symbols in place of π and e, and this is to be regretted, as any departure from the recognised notation in elementary matters is always unfortunate. Even if the symbols were happily chosen, which does not appear to be the case, they would require the cutting of new type, and it is absolutely certain that there is not the least chance of their general adoption. If now used by a few prominent writers in America, they may spread to such an extent as to make it very difficult for their successors to get rid of them. The preservation of the international character of mathematical notation is of paramount importance, and the existence of local notations, especially when they find their way into text-books, is a calamity. In England the Cambridge notations, $\sin^{-1}x$, due we believe to Herschel and Babbage, and the factorial notation due to the late Prof. Jarrett, are still retained by many English writers, although it has long been evident that there is no chance of their adoption by continental mathematicians. It is always desirable to adhere to an established notation, if it is generally understood and accepted, even if it is unsatisfactory, rather than attempt to replace it by a better one, unless there seems very good reason to suppose that the attempt will be successful.

In the previous article in NATURE reference was made to the services which Dr. J. E. Hendricks, of Des Moines, Iowa, has rendered to mathematics in America by the publication of the *Analyst*, which he established in 1874, and has continued to the present time. This journal is published every two months, and has now completed its ninth volume. In spite of typographical and other difficulties the editor has published it regularly, and it shows no signs of diminished vitality or interest on his part. It has been self-supporting, and its success is due to the genuine love of their subject felt by the editor and the contributors. A great part of each number is unfortunately devoted to problems—the lowest form of mathematics—and the space available for more valuable matter is thus considerably diminished. One also is tempted to wish that the editor would show greater strictness in curtailing or excluding the writings of certain contributors, but nevertheless the *Analyst* contains not a few useful papers. It is easy to see the blemishes in such a journal by merely turning over the pages, but it is not so easy to estimate the services which it confers upon science by inducing teachers to look beyond the text-books and interest themselves in a subject for which a genuine taste can only be acquired by attempting to do work for oneself. The large quarto page of the *American Journal* and the elaborate nature of some of its papers render it unsuitable for the short notes and the more unpretentious class of papers in which the author lays but little claim to originality. For these the *Analyst* is available; but, after all, the chief value of such a publication consists in the interest in mathematics it excites and fosters in those who could be reached in no other way, and the inducement it affords for those who are unable to devote their whole time to the subject to nevertheless undertake useful and profitable work. No previous American mathematical journal has ever been published regularly for nine years, and Dr. Hendrick has reason to feel proud of the success of his efforts.

In 1877 Mr. Artemas Martin, of Erie, Pennsylvania, issued the first number of the *Mathematical Visitor*, a quarto journal which was published annually until Jan., 1880, and since then has appeared semi-annually. The first volume ended with the number published in January, 1881. The journal consists entirely of problems and solutions, there being a senior and a junior department. Several of the problems relate to probability questions and involve very complicated and elaborate integrations. As the solution of a prize question, Mr. E. B. Seitz gives, in the number for January, 1879, the values of the coefficients obtained by reverting a general series proceeding by ascending powers of the variable, as far as the sixteenth order. The journal is beautifully printed, and is set up by the editor himself. In the number for January, 1880, he says: "This number of the *Visitor* has been delayed some months, in consequence of the sickness of the editor, who has done all the type-setting with his own hands. He is not a practical printer, and never had set up a stickful of type till last May or June."

At the beginning of the present year Mr. Martin issued, besides the *Visitor*, a new publication, entitled *The Mathematical Magazine: a Journal of Elementary Mathematics*, of which four numbers have now appeared. It is of large quarto size, and, like the *Visitor*, is printed by the editor's own hands. No mathematical journal, if it is to contain anything of real value, can be elementary. Mathematics is an old science, and the really elementary parts of it must be acquired from text-books and by means of the examples which the student works out for himself as exercises. Elementary mathematics is a subject for the school-room, but is unsuitable for a journal, and such a publication as Mr. Martin's, if it continues elementary, is educational rather than scientific. In no instance, we believe, has it been found possible to restrict a mathematical journal really to the elements of the subject alone, though of course elementary articles, and articles which are of interest to junior readers, form a considerable portion of each number of some mathematical publications. Mathematical investigations that are really valuable can never be made elementary, and the questions that can be treated by elementary mathematics are too trivial to deserve recognition in a scientific journal.

We may notice a demonstration of Euclid i. 47, by the late General Garfield, which appears in the first number. If the figure is completed it is in fact an intuitive geometrical proof that $(a+b)^2 = c^2 + 2ab$, where a and b are the sides and c the hypotenuse of a right-angled triangle. The construction is to divide the four sides of a square each into two parts, a and b, in the same order, and to join the points of division. Each of the joining lines is thus equal to the hypotenuse c, and the whole square, $(a+b)^2$, is evidently equal to the inside square, c^2, and the four triangles in the corners, each of which is equal to $\frac{1}{2}ab$. The figure is practically the same as in the well-known proof in which the squares a^2 and b^2 are placed side by side and divided by only two lines in such a manner that the parts may be moved by mere translation (without rotation) so as to form the square c^2, but the special features which give this proof its remarkable elegance are absent. Garfield's proof is Indian in its character, and must have been known to Bhascara, but in the rather more elegant one given in the Vija Ganita (1150) the lines are drawn from the angles of the square c^2 parallel to the sides of the triangle, and include a square $(a-b)^2$, each of the triangles in the corners being $\frac{1}{2}ab$ as before, so that the theorem proved is $c^2 = (a-b)^2 + 2ab$. If the points of division in the figure in § 150 of the Vija Ganita, in which it is shown that $(a+b)^2 - 4ab = (a-b)^2$ are joined, the figure includes both Garfield's and the Hindoo constructions. The construction given by Garfield must have been of course discovered over and over again, and, on its own account, it is so self-evident as only to be interesting historically in connection with the Indian proofs.

If therefore we include among journals one published at such long intervals as half a year there are now no less than four journals, devoted exclusively to mathematics, published in the United States.

With reference to the list of mathematical journals given in the previous article in NATURE, it may be mentioned that the Belgian journal, the *Nouvelle Correspondance Mathématique*, which was edited by M. Catalan, with the co-operation of MM. Mansion, Brocard, Neuberg, and others, was discontinued at the end of 1880. It has been replaced by a new journal, *Mathesis*, which has since been published monthly under the editorship of MM. Mansion and Neuberg, and has now completed its second volume. In this journal the titles of elementary articles are marked by a cross; there are not on the average more than one or two so marked in each number.

A new Scandinavian mathematical journal is shortly to appear under the editorship of Prof. H. G. Zeuthen, of Copenhagen, and Prof. Mittiag-Leffler, of Stockholm. It is to be hoped that it has a great scientific career before it, and assuredly no journal will bear on its title-page the names of more illustrious mathematicians, or will have started under more favourable auspices.

J. W. L. GLAISHER

QUAIN'S "ANATOMY"

Quain's Elements of Anatomy. Edited by Allen Thomson, E. A. Schäfer, and G. D. Thane. Two volumes. Ninth edition. (London: Longmans, Green and Co., 1882.)

Lehrbuch der Neurologie. Fortsetzung von Hoffmann's "Lehrbuch der Anatomie." Von Dr. G. Schwalbe. (Erlangen: Eduard Besold, 1880 and 1881.)

THE appearance of a new edition of Quain's Anatomy is always regarded with attention and interest by teachers of anatomy. The high reputation of its successive editors, Richard Quain, William Sharpey, G. V. Ellis, Allen Thomson, and John Cleland, and the care which has been taken to revise each edition and to incorporate with it the latest additions to anatomical knowledge, have caused this work to be universally regarded as an authority, and have gained for it the position of a standard treatise on Human Anatomy.

The new edition, the ninth, which has just appeared, has been prepared under the editorial supervision of Professors Schäfer and Thane, and Dr. Allen Thomson. The first volume, which has been revised by Prof. Thane, contains the descriptive anatomy of the bones, joints, muscles, blood-vessels, but not the heart; cerebro-spinal and sympathetic nerves, but not the brain and spinal cord; with a chapter on superficial and topographical anatomy, in which the editor has been assisted by Mr. R. J. Godlee. The second volume has been for the most part revised by Mr. Schäfer, and contains the histology, and the anatomy of the viscera, including the heart and central organs of the nervous system; whilst a special chapter on embryology has been written by Dr. Thomson.

The separation of the anatomy of the heart from that of the other parts of the vascular system, as well as of the anatomy of the brain and spinal cord from the nerves which arise from them, and from the sympathetic system, both of which are so intimately connected both anatomically and physiologically with both brain and cord, was first made in the eighth edition; for prior to that time they had always been described along with, and as parts of their respective systems. This arrangement, which is also carried out in the present edition, is, in our judgment, most unphilosophical, for it both destroys the continuity of description, and leads the student to dissociate in his mind the origin of the nerves and blood-vessels from their distribution. Such a dissociation might indeed, as regards the nervous system, have been excusable at the time when both the distributory portions of the cranial and spinal nerves and the sympathetic system were believed to be developed quite independently of the cerebro-spinal axis, and only to become connected with it secondarily. But now-a-days, since through the researches, more especially of the much-lamented F. M Balfour, both the cranial and spinal nerves and the sympathetic have been shown to be true offshoots of the cerebro-spinal axis, and like it of epiblastic origin, to dissociate them, even for descriptive purposes, in a systematic text-book, is, we believe, injurious to real progress. The editors of "Quain" would, we suppose, scarcely think of describing in one volume the gangliated cord of the sympathetic, and in another the nerves which arise from it, and yet to do so would not be more illogical than the course they have pursued of separating the description of the cranio-spinal nerves from that of the central nervous axis. If the anatomical description of the human body is ever to be put on a scientific basis, it must be founded on the facts of comparative anatomy and of development, and the great aim of descriptive writers should be to accommodate their descriptions to these facts.

After this protest against some features in the general arrangement of the book, we may now glance at the manner in which the process of revision has been performed by the different editors. The first volume bears throughout the mark of careful revision by Prof. Thane. We have compared many of the descriptions with those of the corresponding structures in the immediately preceding edition, and we notice many changes both in the matter, and in the mode of expression. Various redundancies have been expunged, errors have been corrected, new facts have been introduced, and to some extent the descriptions generally have been re-arranged. Several new woodcuts have been inserted, and those illustrating the vascular system have been made more diagrammatic by colouring the arteries red and the veins blue. We notice, however, that Prof. Thane, as is unfortunately only too common with some English human anatomists, does not properly discriminate between the meaning of the terms mesial line and mesial plane. In his description, for example, of the recto-vesical portion of the pelvic fascia, he speaks of its being "continuous from side to side across the middle line in front of the bladder," forgetful apparently of the fact that the descriptive term "middle line" expresses a line on the surface, either anteriorly or posteriorly, as the case may be; whilst the imaginary plane between these anterior and posterior mesial lines is the mesial plane of the organ or region.

The greatest amount of change, however, as was naturally to be expected from the subjects discussed in it, has been made in the second volume, and more especially in the chapters edited by Mr. Schäfer. The important section on General Anatomy, or Histology, has been in some measure re-arranged, and many of its chapters rewritten. The latest investigations into the structure of the nucleus, the part which it plays in the multiplication of cells, and the process of maturation of the ovum, have been explained and illustrated by woodcuts. The description of the structure of the individual tissues has obviously been carefully revised, and various changes both in the way of addition and omission have been made. Not the least important is the addition to each chapter of the titles of the most recent papers on the subject-matter of the chapter.

If we were disposed to be very critical, we could undoubtedly lay our fingers upon more than one statement to which objection could be taken. And there is indeed one point that we cannot pass over without notice, as it illustrates that in the comparatively small matter of editing a work on anatomy, as in the much larger subject of administering the finances of Egypt, a Dual Control has many disadvantages. Mr. Schafer, for example, in his references to the layers of the embryo, in which the several tissues take their rise, employs the terms ectoderm, mesoderm, endoderm, to express the three layers of the blastoderm, and in this respect adopts the nomenclature most commonly used by German anatomists; whilst Dr. Thomson, in his references to the same layers, almost invariably speaks of them as epiblast, mesoblast, and hypoblast, which, indeed, are the terms commonly in use amongst British anatomists. The employment in different sections of the same work of two distinct words to express the same structure, is an error in judgment, and certainly not to the edification of the student.

The chapter on the spinal cord and brain has also been greatly modified, and in re-arranging it Mr. Schäfer has largely availed himself of the book, the title of which stands second at the head of this article. The history of this book is somewhat curious. Originally it appeared as a German translation of an earlier edition of "Quain," edited by Prof. Hoffmann. Then, somehow or other, the name of Quain dropped out of the title in the later German editions, and now, as regards the chapters on the Nervous System, written by Prof. Schwalbe, it is essentially a new book, and in our opinion contains the best account of the anatomy of the nervous system in man which has yet been published.

Of the section on Embryology, prepared by Dr. Thomson, we have not space to say more than that it narrates in a convenient compass the successive series of changes which result in the formation of the adult human body; that the descriptions are clear, well arranged, and embrace the latest investigations; and that on points under dispute the author expresses himself with reservation and caution, in a manner characteristic of the writings of this anatomist.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Transit of Venus, December 6

In the morning here the sky was clear, and the sun remarkably free from spots. I noticed only 4 small ones on the disk: quite a contrast to the monstrous appearance a month ago.

Being neither equipped nor qualified for technical astronomical observations, I did not attempt to do more than to give a popular demonstration of the transit of Venus to between 30 and 40 friends interested in the phenomenon. My experience of stargazing was chiefly obtained upwards of 50 years ago, before I became otherwise occupied; and then I found for myself, that the best way of studying solar phenomena, whether eclipses or spots, was by projecting on white paper or cardboard, the image of the sun from the telescope, focussed a little beyond the point for direct vision through the dark eye-glass; extraneous sunlight being shut out by a napkin suspended around the telescope. I presume that this method is well known, and it seems to have been adopted, (with the additions of a dark chamber) by Mr. Campbell of Islay, in the exhibition of the transit at Cannes, so well described by the Duke of Argyll in NATURE (p. 156). In this way with a small achromatic of 32 inches focal length, and 2¼ inches aperture we saw well all the chief features of the transit, (which I need not describe, as this has already been done by more competent observers), and this without fatigue to the eyes, and the unnatural colouring, inseparable from looking through the telescope with darkened glasses. Further, as the time of sunset approached, at about 3.30 p.m., we had in our camera view the additional charm of the colours of the objects in view. As, in the Italian sky the golden orb sank with the dark planet spot on its disk, under brightly tinted clouds, shaded off in streaks of tender grey into the azure above, with the blue rose-tipped mountains of the Esterets beneath,—the scene was one as fascinating in beauty as it was interesting in science. All these tints appeared distinctly, albeit faintly, in the telescopic image on the card.

One point was remarkable,—that whilst the shades of the mountains were all *blue*, the dark round spot of the planet on the sun was almost *black*. It was the darkest object in the field of view. Partly, but I hardly think entirely, this may be explained by its being higher, and less subjected to the decomposing power of the lower atmospheric layers. I have endeavoured to represent in water-colours this view of the transit of Venus. The result, of course, cannot be reproduced in print, but any of your readers who may be visitors at Cannes will be welcome to see it, as an original kind of reminiscence of a very rare event. C. J. B. WILLIAMS

Cannes, December 21

The Comet during the Last Month

SINCE my last communication (see NATURE, vol. xxvii. p. 110) the weather and the presence of moonshine has been unfavourable for views of the comet; but I have seen it, more or less distinctly, on seven nights, from November 22 to December 21. I will not take up your space with details, but mention, as the general result of these observations, that the comet has become smaller in dimensions, and much fainter in its light. With moonlight, no trace of a tail is visible; and the nucleus can only be discerned by telescope as a nebulous star of third magnitude. In absence of moonlight, as on December 6, 8, and 12, between 2 and 3.30 a.m., the tail was visible to a length of about 10°, with a breadth expanding from the head, with no distinguishable outline. My last view of it was on the 20th, at 3 a.m., when, with a brilliant starlight after moonset, the comet was in the south-south-east, about 20° above the horizon, with a tail about 8° long, and a nucleus, a nebulous star of third or second magnitude. Its position was about as far to the east-north-east of Procyon as that star is east-south-east of Sirius. It seems likely to be visible in clear moonless nights for two or three weeks longer. C. J. B. WILLIAMS

Cannes, December 21

The Heights of Auroras

THE observations described in your last number as having been made long since in Siberia, of lunar halos projected on auroras, have not, I believe, been confirmed by other observers; but if correct, possibly this phenomenon may be a peculiarity of auroras in Siberia, or in the Arctic regions. There seems reason to think (see Capron's "Auroræ," pp. 37-40) that auroras may be lower when near the magnetic pole than further south. If this is the case, it is so far favourable to the theory (propounded, I think, by a German writer) described in NATURE (Vol. xxv. p. 320), that the auroral zone is a plane, and not part of a sphere concentric with the earth's surface. The majority of the observations in lower latitudes cited in Capron's "Auroræ," place the phenomenon at a height of 100 miles or upwards.

The height of the spindle-shaped object seen in the aurora of November 17 is thus no argument against its auroral character, which I see no reason to doubt. It is true that in my experience (which, in this northern part of the country is probably much greater than that of your correspondents), I have never seen anything resembling it, judging from the descriptions of it; but I do not think this is a reason for supposing such an auroral phenomenon could not take place. The fact that it moved along a parallel of magnetic latitude is a very strong argument for its auroral character. Besides, its spectrum is stated to have exhibited the characteristic auroral line. I hope some one will collect all possible observations of this beam, especially from the continent, and undertake a careful investigation into its path and height. T. W. BACKHOUSE

Sunderland, December 23

The Aurora and its Spectrum

IN reference to Mr. Ralph Abercrombie's letter (NATURE, vol. xxvii. p. 173), I may mention that his remarks quite accord with an opinion expressed to me by my friend, H. R. Procter, that the "aurora is generally formed in some imperfect mist or vapour." I am intending some experiments on discharges *in vacuo* under such conditions and reduced temperatures, also on phosphorescence, in connection with which M. Lecoq de Boisbaudran has shown in his "Spectres lumineux," that we get a line in the red, brightening as the temperature is reduced. I do not read the result of my Swan lamp experiment, as Mr. Munro (same number and page) does. The lamp, when perfect, gave quite a bright white glow, with a strong carbon spectrum. I should therefore attribute the absence of the nitrogen spectrum at this time not so much to a high spectrum as to the probability that the lamp had been, as far as possible, exhausted of air, and filled with some form of carbon gas. I am not aware of any air-vacuum point at which the nitrogen bands or lines disappear, except for want of light in the discharge. With regard to the letter of W. M. F. P. on the "Meteor of November 17th," I only assumed the correctness of the figures and heights quoted in mine for the purpose of showing the complex nature of the auroral questions. I am not the less perfectly satisfied that the "beam" was a true aurora, and not a meteor, my spectroscopic observation of it putting this beyond a doubt. J. RAND CAPRON

Guildown, December 23

The Weather

IT is curious how the recent auroræ have been followed not only by a cold wave, but by a subsequent warm one, and these respectively of such extremes, that 21° at 9 a.m. on the 11th is this day replaced by 48° or 27° of difference. Equally strange have been the effects on animal and vegetable life. During the cold, an almost Arctic season in its ice-bound stillness prevailed, and a flock of wild geese crossing in front of the house (the forerunners, in public opinion, of a hard winter) represented external creature life. Now all is changed almost to spring. Roses, though somewhat nipped by the frost, seem ready to blow; flies and gnats are unthawing, and last night, in going up to the observatory, I noticed the phosphorescent glimmer of a luminous centipede under one of the shrubs, a sight I do not remember ever to have met with in winter before. J. RAND CAPRON

Guildown, December 19

A Common Defect of Lenses

A CHANCE observation a few weeks since led me to the discovery of a serious defect in the object-glass of the collimator of a spectroscope by Grubb, of Dublin, which I have been using for some time. As further investigation has shown me that the defect is very common, while at the same time it is a source of considerable error in all experiments on the plane of polarisation of polarised light, it seems worth while to call the attention of readers of Nature to it. The object-glass in question has been imperfectly annealed. As a consequence, a plane polarised incident beam is elliptically polarised on emergence from it.

If it be looked at between crossed Nicols in a pencil of parallel rays, the field of view becomes bright, and is crossed by two brushes hyperbolic in form, which for two positions of the lens became two straight lines. If again plane polarised light be allowed to pass through the lens while it is turned round its own axis, there are four positions of the lens for which the central portion of the emergent beam is plane polarised, and can be quenched by an analysing Nicol; for all other positions of the lens, the emergent beam is elliptically polarised, and the light cannot be quenched, but reduced to a minimum. Moreover, as the lens is turned, the position of the axes of the ellipse varies by nearly half a degree. I have since examined a large number of lenses, without finding one quite free from the defect. One well-known London optician declines to attempt to supply me with a two-inch object-glass which shall not show it, while another states he has never known any lens absolutely free from it.

The important bearing of the point on all investigations into the polarisation of light is obvious. The consequences it produces in modifying the results of some recently-published experiments of mine (*Phil. Trans.*, Part ii., 1882) formed the subject of a paper read at the last meeting of the Royal Society. R. T. GLAZEBROOK

Trinity College, Cambridge, December 20

New Deep-Sea Fish from the Mediterranean

MY letter in NATURE, vol. xxv. p. 535, called forth two important notes from such competent ichthyologists as Mr. J. Y.

Johnson and Dr. Th. Gill (NATURE, vol. xxvi. pp. 453, 574), to both of whom a reply is due, and should have been given sooner had I not been absent from Florence and otherwise engaged.

Firstly, I must correct my assertion as to the occurrence of *Malacocephalus lævis* in the Mediterranean; after having examined the type specimen and that mentioned by Mr. Johnson, both in the British Museum and after a further examination of my specimens, which I had considered as young *Malacocephali*, I have now not the slightest doubt that they are quite distinct. They are an undescribed and most interesting form of Macrurids allied to *Coryphænoides*, which I propose calling *Hymenocephalus italicus*. I have in my possession six specimens, both adult and young; in two of the former I have found the ovaries fully developed with mature ova.

As to the "singular fish of a deep black colour with small eyes, a naked skin, and a most abyssal physiognomy," which I got at Messina, it has no connection whatever with *Chiasmodon niger*, but is, as I before assserted, a Stomiatid, very different from all the known forms, including Dr. Gunther's *Bathyophis*. It stands apart in many respects, and is the type of a new genus and perhaps of a new section of that singular family. I intend shortly to describe and figure it under the name of *Bathophilus nigerrimus*, along with other strange fish collected during my deep-sea and ichthyological researches in the Mediterranean.

HENRY HILLYER GIGLIOLI

R. Zoological Museum, Florence, December 17

Electrical Phenomenon

ON retiring to bed shortly after midnight on the 13th inst., I experienced a phenomenon which, though not of itself uncommon, was, I think, unusually developed. On pulling off a flannel vest which I wear next my skin, over my head, I became conscious of a strange sensation in the latter, accompanied by a distinct crackling noise, and bright sparks which were plainly visible in the dimly lighted room. To make sure that I was not the subject of a delusion, I repeated the operation many times, in each case rubbing the flannel half-a-dozen times—not more—against my hair. Not only were the same phenomena observable every time, but also if, after removing the flannel I then approached my knuckles to that part of it which had been in contact with the hair, a whole volley of sparks passed between the flannel and each knuckle at a distance of not less than *two inches*. As often as I repeated the experiment, so often did the phenomena repeat themselves, until I at length retired to bed not altogether without apprehension, that I might awake in the night with the bed-clothes on fire, by reason of the discharge of some extra big spark between my hair and a convenient blanket. No such catastrophe, however, occurred, and on repeating the operations the next morning, I could not reproduce the phenomena. The next evening I again repeated the experiment, and this time by very violent rubbing could just get a faint discharge between the flannel and knuckles when almost in contact. On other nights since these I have not succeeded in getting any such effect, or at most a very feeble one. To what, then, am I to attribute the marked difference of the first night? Was it due to something peculiar in the condition of the hair, the air, or the flannel? Perhaps some of your readers can suggest. As regards the first of these I ought to state that it had, on the afternoon of the 13th, been subjected to the operations of cutting, shampooing, and brushing "by machinery," at the hands of the barber. That was, however, seven hours earlier in the day, and any electricity developed by the friction of the last operation ought to have been dissipated long before twelve o'clock—especially as the night was damp and misty.

A. J. K.

29, Victoria Road, Finsbury Park, December 19

PHOTOGRAPHING THE CORONA[1]

PROBLEMS of the highest interest in the physics of our sun are connected, doubtless, with the varying forms which the coronal light is known to assume, but these would seem to admit of solution only on the condition of its being possible to study the corona continuously, and so to be able to confront its changes with the other variable phenomena which the sun presents. "Unless some means be found," says Prof. C. A. Young, "for bringing out the structures round the sun which are hidden by the glare of our atmosphere, the progress of our knowledge must be very slow, for the corona is visible only about eight days in a century, in the aggregate, and then only over narrow stripes on the earth's surface, and but from one to five minutes at a time by any one observer" ("The Sun," p. 239).

The spectroscopic method of viewing the solar prominences fails, because a large part of the coronal light gives a continuous spectrum. The successful photograph of the spectrum of corona taken in Egypt, with an instrument provided with a slit, under the superintendence of Prof. Schuster during the solar eclipse of May 17, 1882, shows that the coronal light as a whole, that is the part which gives a continuous spectrum, as well as the other part of the light which may be resolved into bright lines, is very strong in the region of the spectrum extending from about G to H. It appeared to me, therefore, very probable that by making exclusive use of this portion of the spectrum it might be possible under certain conditions, about to be described, to photograph the corona without an eclipse.

In the years 1866-68 I tried screens of coloured glasses and other absorptive media, by which I was able to insolate certain portions of the spectrum with the hope of seeing directly, without the use of the prism, the solar prominences (*Monthly Notices*, vol. xxviii. p. 88, and vol. xxix. p. 4). I was unsuccessful, for the reason that I was not able by any glasses or other media to isolate so very restricted a portion of the spectrum as is represented by a bright line. This cause of unsuitableness of this method for the prominences which give bright lines only, recommends it as very promising for the corona. If by screens of coloured glass or other absorptive media the region of the spectrum between G and H could be isolated, then the coronal light which is here very strong would have to contend only with a similar range of refrangibility of the light scattered from the terrestrial atmosphere. It appeared to me by no means improbable that under these conditions the corona would be able so far to hold its own against the atmospheric glare, that the parts of the sky immediately about the sun where the corona was present would be in a sensible degree brighter than the adjoining parts where the atmospheric light alone was present. It was obvious, however, that in our climate and low down on the earth's surface, even with the aid of suitable screens, the addition of the coronal light behind would be able to increase, but in a very small degree, the illumination of the sky at those places where it was present. There was also a serious drawback from the circumstance that although this region of the spectrum falls just within the range of vision, the sensitiveness of the eye for very small differences of illumination in this region near its limit of power is much less than in more favourable parts of the spectrum, at least such is the case with my own eyes. There was also another consideration of importance, the corona is an object of very complex form, and full of details depending on small differences of illumination, so that even if it could be glimpsed by the eye, it could scarcely be expected that observations of a sufficiently precise character could be made to permit of the detection of the more ordinary changes which are doubtlessly taking place in it.

These considerations induced me not to attempt eye-observations, but from the first to use photography, which possesses extreme sensitiveness in the discrimination of minute differences of illumination, and also the enormous advantage of furnishing a permanent record from an instantaneous exposure of the most complex forms I have satisfied myself by some laboratory experiments that under suitable conditions of exposure and development a photographic plate can be made to record minute differences of illumination existing in different parts of a bright object, such as a sheet of drawing paper, which are so subtle as to be at the very limit of the power of recognition of a trained eye, and even, as it appeared to me, those which surpass that limit.

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[1] "On a method of Photographing the Solar Corona without an Eclipse." Paper read at the Royal Society by William Huggins, D.C.L., LL.D., F.R.S., December 21.

My first attempts at photographing the corona were made with photographic lenses, but uncertainty as to the state of correction of their chromatic aberration for this part of the spectrum, as well as some other probable sources of error which I wished to avoid, led me to make use of a reflecting telescope of the Newtonian form. The telescope is by Short, with speculum of 6 inches diameter, and about 3½ feet focal length. A small photographic camera was fastened on the side of the telescope tube, and the image of the sun after reflection by the small plane speculum was brought to focus on the ground glass. The absorptive media were placed immediately in front of the sensitive film, as in that position they would produce the least optical disturbance. Before the end of the telescope was fixed a shutter of adjustable rapidity which reduced the aperture to 2 inches. This was connected with the telescope tube by a short tube of black velvet for the purpose of preventing vibrations from the moving shutter reaching the telescope. On account of the shortness of the exposures it was not necessary to give motion to the telescope.

It was now necessary to find an absorptive medium which would limit the light received by the plate to the portion of the spectrum from about G to H. There is a violet (pot) glass made, which practically does this. I had a number of pieces of this glass ground and polished on the surfaces. Three or four of these could be used together, castor-oil being placed between the pieces to diminish the reflection of light at their surfaces. Some inconvenience was found from small imperfections within the glass, and it would be desirable in any future experiments to have a larger supply of this glass, from which more perfect pieces might be selected.

In my later experiments I used a strong and newly made solution of potassic permanganate, in a glass cell with carefully polished sides. This may be considered as restricting the light to the desired range of wave-length, since light transmitted by this substance in the less refrangible parts of the spectrum does not affect the photographic plates.

Different times of exposure were given, from so short an exposure that the sun itself was rightly exposed, to much more prolonged exposures, in which not only the sun itself was photographically reversed, but also the part of the plates extending for a little distance from the sun's limb.

Gelatine plates were used, which were backed with a solution of asphaltum in benzole.

After some trials I satisfied myself that an appearance peculiarly coronal in its outline and character was to be seen in all the plates. I was, however, very desirous of trying some modifications of the methods described, with the hope of obtaining a photographic image of the corona of greater distinctness, in consequence of being in more marked contrast with the atmospheric illumination.

Our climate is very unpropitious for such observations, as very few intervals, even of short duration, occur in which the atmospheric glare immediately about the sun is not very great. Under these circumstances I think it is advisable to describe the results I have obtained, without further delay.

The investigation was commenced at the end of May of this year, and the photographs were obtained between June and September 28.

The plates which were successful are twenty in number. In all these the coronal form appears to be present. This appearance does not consist simply of increased photographic action immediately about the sun, but of distinct coronal forms and rays admitting in the best plates of measurement and drawing from them. This agreement in plates taken on different days with different absorptive media interposed, and with the sun in different parts of the field, together with other necessary precautions observed, makes it evident that we have not to do with any instrumental effect.

The plates taken with very short exposures show the inner corona only, but its outline can be distinctly traced when the plates are examined under suitable illumination. When the exposure was increased, the inner corona is lost in the outer corona, which shows the distinctly curved rays and rifts peculiar to it.

In the plates which were exposed for a longer time, not only the sun but the corona also is photographically reversed, and in these plates, having the appearance of a positive, the white reversed portion of the corona is more readily distinguished and followed in its irregularly sinuous outline than is the case in those plates where the sun only is reversed, and the corona appears, as in a negative, dark.

Prof. Stokes was kind enough to allow me to send the originals to Cambridge for his examination, and I have his permission to give the following words from a letter I received from him: "The appearance is certainly very corona-like, and I am disposed to think it probable that it is really due to the corona."

Prof. Stokes' opinion was formed from the appearance on the plates alone, without any knowledge of their orientation, and without the means of comparing them with the eclipse plates taken on May 17.

I have since been allowed, through the kindness of Capt. Abney, to compare my plates with those taken of the corona in Egypt during the eclipse of May last. Though the corona is undergoing doubtless continual changes, there is reason to believe that the main features would not have suffered much alteration between May 17 and September 28, when the last of my plates was taken. This comparison seems to leave no doubt that the object photographed on my plate is the corona. The more prominent features of the outer corona correspond in form and general orientation, and the inner corona, which is more uniform in height and definite in outline, is also very similar in my plates to its appearance in those taken during the eclipse.

Measures of the average height of the outer and of the inner corona in relation to the diameter of the sun's image are the same in the eclipse plates as they are in my plates taken here.

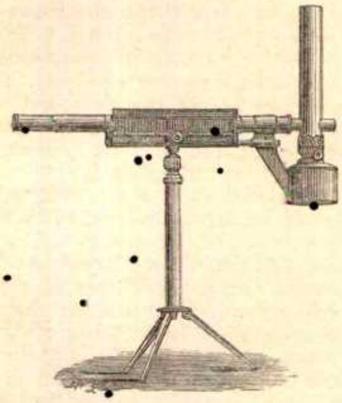
There remains little doubt that by the method described in this paper, under better conditions of climate, and especially at considerable elevations, the corona may be successfully photographed from day to day with a definiteness which would allow of the study of the changes which are doubtlessly always going on in it. By an adjustment of the times of exposure, the inner or the outer corona could be obtained as might be desired. It may be that by a somewhat greater restriction of the range of refrangibility of the light which is allowed to reach the plate, a still better result may be obtained.

Plates might be prepared sensitive to a limited range of light, but the rapid falling off of the coronal light about H would make it undesirable to endeavour to do without an absorptive screen. Lenses properly corrected might be employed, but my experience shows that excessive caution would have to be taken in respect of absolute cleanness of the surfaces and of some other points. There might be some advantage in intercepting the direct light of the sun itself by placing an opaque disk of the size of the sun's image upon the front surface of the absorptive screen. Though for the reasons I have already stated I did not attempt eye-observations, there seems no reason why, with suitable screens and under suitable atmospheric conditions, the corona should not be studied directly by the eye. There might be some advantages in supplementing the photographic records by direct eye-observations. I regret that the very few occasions on which it has been possible to observe the sun has put it out of my power to make further experiments in these and some other obvious directions.

P.S.—[I have Capt. Abney's permission to add the following letter this day received from him;—"A careful examination of your series of sun-photographs, taken with absorbing media, convinces me that your claim to having secured photographs of the corona with an uneclipsed sun is fully established. A comparison of your photographs with those obtained during the eclipse which took place in May last, shows not only that the general features are the same, but also that details, such as rifts and streamers, have the same position and form. If in your case, the coronal appearances be due to instrumental causes, I take it that the eclipse photographs are equally untrustworthy, and that my lens and your reflector have the same optical defects. I think that evidence by means of photography of the existence of a corona at all is as clearly shown in the one case as in the other."—*December* 15, 1882.]

A WEDGE AND DIAPHRAGM PHOTOMETER

A NEW photometer, shown in perspective in the figure, has lately been constructed by Mr. Sabine. The stand supports a straight horizontal tube, at one end of which is a paraffin lamp, and at the other an eyepiece. The middle portion of the tube is cut away, and has, slipped over it, a collar to which a frame is attached, carrying a wedge of neutral-tinted glass, adjustable by means of a rack and pinion. Inside the collar is fixed a transverse disc of ground opal glass, which the paraffin lamp illuminates to a definite degree. This disc constitutes the field of comparison, the illumination of which is adjustable by means of a series of diaphragms of known aperture at the end near to the paraffin lamp. At the side, between the wedge and the collar which carries it, is a narrow pane of ground opal glass, just behind which a small mirror is fixed at an angle of 45° to the axis of the tube. This mirror is supported from the centre of the transverse opal disc in such a way, that the support is hidden from the observer by the mirror itself, an arrangement which insures the apparent juxtaposition of the illuminated surfaces which have to be compared. The light to be measured is placed on the right-hand side of the photometer; and the collar is turned so that the light falls normally upon the face of the wedge, passes through the wedge, through the pane of opal glass, and is incident upon the mirror, which reflects a portion of it to the eye of the observer. The wedge is then shifted, if necessary, to interpose a greater or less thickness of absorbing medium, until a balance is obtained, that is until the apparent illumination of the mirror is equal to that of the field of comparison, in the middle of which it is seen. If the range of the wedge is insufficient to admit of this, the degree of illumination of the field is altered, by means of the diaphragms, and the wedge is then adjusted.



The employment of glass wedges for photometric comparisons is not new, having been already used by both Xavier de Maistre and Quetelet; but no practical photometer based upon this method has hitherto been constructed. The employment of diaphragms for extending the range of the wedge is found to work well and to enable the operator to adjust the illumination of the field with exactitude, the bright part of the paraffin flame being of course, kept opposite to, and so as to well cover the diaphragm aperture. A table is constructed giving for each position of the wedge and for each diaphragm, the value, in standard candles, of any light placed at a distance of one metre from the instrument; and if the light be placed at any other distance, the number in the table has simply to be multiplied by the square of the actual distance in metres. For ascertaining approximately the amount of light which passes through any given coloured glass, for example, orange glass, the eyepiece is furnished with a rotary disc containing small panes of white and different coloured glasses, either of which can be interposed at pleasure.

This photometer is being made by Messrs. Elliott Bros., in two forms, one for use as a portable photometer, as shown in the figure, and the other on a more solid stand, for laboratory purposes.

ON THE OCCURRENCE OF GREAT TIDES SINCE THE COMMENCEMENT OF THE GEOLOGICAL EPOCH[1]

IT will I daresay be within the recollection of many of those who are now present that I was honoured by the invitation to deliver the opening lecture in this hall last year. In response to that invitation I addressed to you a discourse which I ventured to call "A Glimpse through the Corridors of Time." Accounts of it have appeared in very many quarters, both at home and abroad. I am myself responsible for the account which appeared in the columns of NATURE, as well as for the pamphlet form in which the lecture has since been issued. The chief reason why I now recur to the subject remains to be stated. Among the various comments which have been made upon that address, some are by no means favourable to the views I ventured to put forward, and they have been the theme of considerable discussion. Up to the present I have not made any reply to the criticisms which have appeared; I postponed doing so until a suitable opportunity should have arisen for a review of the whole subject. Your kindness in inviting me once again to address this great Institute has afforded such an opportunity, and with your permission I propose to preface the subject of my lecture this evening by a reply to those critics who have honoured me with their attention.

Let me recall to you very briefly the subject of that lecture, so as to enunciate clearly the point as to which an issue has been raised. You will perhaps recollect that the lecture treated principally of the tidal relations between the earth and the moon, of the influence of the tides during ages past, and of the future which awaits the earth-moon system during ages to come. I pointed out that at the present moment the orbit of the moon must be gradually growing in size, that this gradual increase of the distance from the earth to the moon is essentially non-periodic, and thus is totally different to the ordinary lunar irregularities which are recognised in rigid-body astronomy. As a consequence of this incessant growth in the moon's distance we see that in past ages the moon must have been appreciably nearer to the earth than it is at present, and that if we look far enough back we must inevitably come to an epoch when the moon would seem to have been quite close to the earth; indeed looking earlier still we are not without reasons for believing that in primeval times the earth and the moon formed but a single body.

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[1] Extract from a lecture delivered at the Midland Institute, Birmingham, on November 20, 1882, by Prof. Robert S. Ball, LL.D., F.R.S. Communicated by the Author.

Wondrous as this narrative may seem, yet on a due consideration of the mathematical evidence in its favour we are constrained to admit that it must be substantially correct. Unless some notable agency at present unknown to us has intervened in past time, the course of events must have run along the lines we have indicated. We make but little pretence to give the date when the moon seems to have commenced its independent existence, nor to indicate the chronology of the epochs when its distance increased by one thousand miles after another. All that seems certain is that the events we are at present discussing must have occurred millions, many millions of years before man placed his foot on this planet.

The cause of these mighty series of changes is still in hourly operation. The ebb and flow of the tides around our coasts is only the survival of greater tides with which in earlier days the ocean must have throbbed. The earlier we look back the mightier must have been the daily ebb and flow. I even invited you to look back to an excessively remote epoch when the moon was only at a fraction of its present distance, and when the daily rise and fall, instead of being counted in tens of feet, must have been reckoned by hundreds. Even up to this point there has been little or no controversy, there can be none. I do not know that any one has attempted to deny that the earth must once have experienced these mighty tides either in the actual body of the earth, or in the ocean on its surface. The controversy has arisen on the question as to whether these great tides had subsided before the commencement of the geological epoch, or whether they were contemporaneous therewith.

In my lecture in this hall last year, I made the suggestion that the reign of the mighty tides did perhaps extend into the commencement of the geological epoch. I further ventured to suggest that these great tides had left their traces on the solid crust of the earth. I quoted eminent geological authority to show that the rocks at the base of our stratified system are of the most stupendous volume and thickness. It has always been a difficulty to determine how the present geological agents could have manufactured so mighty a mass, and I appealed to the great tides as a grinding engine competent to aid in this work. At this point issue has been taken, and it is now my duty to review the arguments which have been adduced as bearing on this question. But let me here make a single remark which disposes of many of the objections that have been raised. Several of my critics do not seem to have observed that I postulated the mighty tides for the manufacture of the earliest primæval rocks, and *for these alone*. "Take, for example," I said (p. 25), "that earliest and most interesting epoch when life perhaps commenced on the earth, and when stratified rocks were deposited five or ten miles thick, which seem to have contained no living form higher than the eozoon, if even that were an organised being." Again and again I stated that I merely referred to these primitive strata. Yet this is a point that many of my critics have ignored. They have been at pains to prove that colossal tides did not exist in the comparatively modern geological epochs, and many interesting facts have been adduced. But such considerations have only an infinitesimal bearing on the position I adopted. Even the coal-measures are a modern formation when compared with the primeval rocks, for the manufacture of which I suggested the mighty tides.

The controversy as to the great tides has principally ranged around the question as to whether the primitive rocks present any indications of great tidal action. I am not a practical geologist, and am most anxious to obtain the views of those that are. Now these opinions are to be had. They are to be found in the correspondence in NATURE at the commencement of this year; yet there is, as might have been anticipated, considerable differences of opinion. I first refer to Prof. Hull's letter (NATURE, vol. xxv. p. 177), and find that this most competent authority adduces direct evidence of tremendous denudation in the Palæozoic ages, such as might have been produced by mighty tides. On the other hand, Prof. Newberry (vol. xxv. p. 357) says that there is no direct evidence whatever to show that the denuding agencies were greater in former times than now. In the following number of NATURE we have letters from Dr. Callaway and Mr. Hale (p. 385) to show that Prof. Newberry's conclusions are not necessarily valid. Mr. S. V. Wood and Mr. J. Vincent Elsden bring forward facts which go to support Prof. Newberry's view, while Dr. Callaway, though carefully declining to commit himself to the high tides, controverts Mr. Wood and Mr. Elsden (p. 409). Now all these gentlemen speak with special knowledge, but it is not easy to deduce from this correspondence as to which side the balance of skilled opinion really inclines. It would almost seem as if a very fundamental point had escaped attention. Prof. Geikie, in his great work just published, tells us that these great tides could not have existed in geological times, because, if they had done so they must have left certain traces, and we do not find these traces. The fundamental question is, What traces of great tides ought we to expect to find if those great tides had really existed? It would seem that unless this question be first answered, it is impossible to dispose of the question with the brevity which Prof. Geikie has adopted. I apprehend it cannot be doubted that by the great tides the materials of stratified rocks would be rapidly formed, and that in suitable localities these materials would be deposited to form rocks. I can see no necessary difference between a ton of mud ground up by colossal agents in former days, and a ton of mud ground up by the more prosaic agents of modern days. But for each ton of mud now made there would then have been a great many tons. The strata would thus have grown more rapidly in early times, and thus the exceptional thickness of the earliest stratified rocks could be accounted for. It seems useless to assert that vestiges of the great tides do not exist, unless we can form some idea of the sort of vestiges that should be expected if the great tides had existed. I believed at the time I gave my lecture, and I believe still, that we do see vestiges of vast primæval tides. I can even count these vestiges. They are five, or perhaps ten in number; they are the five or ten miles of vertical thickness of stratified rocks which were deposited at the bottom of the ocean during the earliest stages of the geological epoch.

I have derived so much pleasure and so much instruction from the study of Mr. G. H. Darwin's writings, that on this ground alone I would be reluctant to have any difference of opinion with him. Indeed, seeing that the earth-moon history is one which he has made peculiarly his own, and illuminated by discoveries which I believe to be the most important contributions made to physical astronomy in modern times it would seem presumption in me to venture to differ from him. Mr. Darwin, writing in NATURE, vol xxv. p. 213, has asked me to reconsider the views I had set forth as to the probability of the great tides being contemporaneous with geological phenomena. Mr. Darwin shows that at the time when the great tides supposed in my calculation existed, the earth must have been spinning round once in seven hours, and that this would involve trade-winds of $3\frac{2}{3}$ times their present velocity and vertical storms of prodigious violence, and then he adds:—

"Now if this state of things existed in geological history, we should expect to find the earlier sedimentary rocks of much coarser grain than the modern ones. But I am not aware that this is the case. Again, to understand such blasts, the earliest trees should have trunks of enormous thickness and their leaves must have been very tough, or they would have been torn to shreds. There seems to be no reason to suppose that the trees of the Carboniferous period present marked peculiarities in these respects."

"It is on these grounds that I venture to dissent from Mr. Ball in the geological interpretation to be placed on the tidal theory, and I think we must put these violent phenomena in pre-geological periods."

But is it necessarily true that the prodigious tides must have produced a coarser material as the result of their grinding than is found in the later rocks? I can imagine it to be contended that the more powerful mill would produce the finer flour, but in truth I really do not see that we have any *a priori* grounds for deciding whether the *débris* produced by mighty tides should be fine or coarse. Have we not illustrious authority for invoking our "Domestic Productions" to throw light on obscure questions removed from actual observation. Let us look at the biggest tides we know of, and see whether they are associated with fine mud or with coarse. I appeal to every one who has stood on the Clifton Suspension Bridge or walked on the Beach at Weston-super-Mare to answer this question. In both cases they will see mud of a fineness and a stickiness that is proverbial; yet that mud is washed twice every day by the mightiest tides in the British Islands. I do not say, nor do I believe, that the fineness of the mud in the Avon is the consequence of the great tides; but I think the illustration is a fair reply to an argument which says the tides in ancient days cannot have been of great size, because the mud with which those tides are associated is not coarse.

In the second place, Mr. Darwin urges against me the trees of the Carboniferous epoch, and his inference that the tremendous tides cannot have existed in the Carboniferous epoch is probably well founded. But I have not said that these tides did exist in the Carboniferous epoch. I can only again repeat that my argument supposed that the mighty tides may have existed in the times when the very earliest stratified rocks were deposited. In the course of ages, as the moon receded, so the tides gradually dwindled down until in the comparatively modern time indicated by the Carboniferous epoch, they may have been small enough to be connected with the wonderful coal vegetation.

I had, as I was bound to do, most carefully weighed the words in which I addressed you from this place last year. I was aware that the opinion I advanced would meet with opposition. This was a reason why I should consider the subject most carefully before I spoke, but it was not a reason why I should withhold the views at which I had arrived. I have again considered the matter with the results now set forth, and I have seen no reason to depart in the slightest degree from the position which I had previously adopted.

MARS[1]

THE similarity which has long been thought to exist between our own globe and the planet Mars would naturally commend itself to careful examination at the hands of such observers as possess instruments adequate to the inquiry. The shadowing of large portions of its surface with patches which easily lend themselves to the supposition of being collections of water, the occasional indistinctness of their outlines, so strongly indicative of atmospheric obscuration, the clothing of either pole with the semblance of a snowy mantle obedient in its extent to solar action, all this would bespeak of itself a critical investigation. And the challenge has been taken up from an early period, and to an extent which would probably surprise those who are unfamiliar with the subject. Already in 1873 the number of drawings collected by Dr. Terby of Louvain, than whom no man is more intimately conversant with areography, amounted to 1092, and the nine subsequent years, which have included among others the celebrated representations of Green and Schiaparelli, have greatly augmented that imposing number. We should be mistaken, however, if we were to estimate the progress of our knowledge by the multiplication of designs. In this case the ancient saying *πλέον ἥμισυ παντός* would probably express too large a proportion. The increase, if in some respects not to be regretted, brings with it additional elements of uncertainty, if not of error. Many representations might be discarded with positive advantage to the final conclusion: like numerical observations whose unworthiness is detected by their wide deviation from the mean of the rest, the result is all the surer for their exclusion. An unpleasant experience proves that the most careful observer is not always the most successful draughtsman, nor in such matters is zeal any pledge of excellence. Comparison of the results obtained by different astronomers leads to the conclusion that, after due allowance has been made for instrumental and atmospheric differences, all men do not see alike, or interpret in the same way what they see, or transfer the image to paper with equal success. Here it is that photography, though not exempt from defects and hindrances of its own, is now beginning to render invaluable aid. But such an object as the disc of Mars would not lend itself very readily at present to the camera, and the pencil and the brush must do the best they can till some further advance is made to supersede them.

But however improved may be our future representations, and whatever may be the result—on every supposition most interesting—of the keen scrutiny that is in store for the next opposition of the planet, it would undoubtedly be an injudicious course to discard as unworthy of study and comparison the delineations of earlier days. Less valuable, if standing alone, they may attain considerable importance in the elucidation of some otherwise unexplained difficulty; and evidence which, unsupported, might be of little weight, may acquire especial consequence from its collateral bearing on more direct testimony. The comparatively rude and defective sketches of a long-passed era, contained in the publication before us, executed in a spirit of unwearied industry and unimpeachable fidelity, but under the influence of a mistaken impression, form a striking illustration of the previous remarks.

The history of the "Areographische Beiträge" is connected with a very lamentable occurrence in the life of the worthy old Hanoverian observer, Dr. Johann Hieronymus Schröter. He had long been settled in a Government office at Lilienthal, not far from Bremen, where his almost innumerable observations on sun, moon, and planets (with stars he did little) had been carried on with reflectors of various sizes—two by Sir W. Herschel of 4 and 7 feet focal length, others by Schrader, of Kiel, of 7, 11, 15, and 27 (26 English) feet, and a 4-inch object-glass by Dollond, equatorially mounted. His passion for observation would never have allowed so interesting an object as Mars to escape him, and accordingly we find that between the years 1785 and 1803 he had accumulated 217 designs, with a corresponding description marked by all the minute preciseness of detail and inference which characterise his other labours. The work had been promised for publication at Easter, 1812, but had been somehow delayed, when an event occurred on the night of April 20, 1813, in connection with the occupation of Bremen by the French, under the rapacious and unscrupulous Vandamme, the story of which we may allow the sufferer to relate in his own words.

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[1] "Areographische Beiträge zur genauern Kenntniss und Beurtheilung des Planeten Mars." Von Dr. J. H. Schrœter: herausgegeben von H. G. van de Sande Bakhuyzen, Director der Leidener Sternwarte. 8vo, 447 pp., with Atlas. Leiden: E. J. Brill.

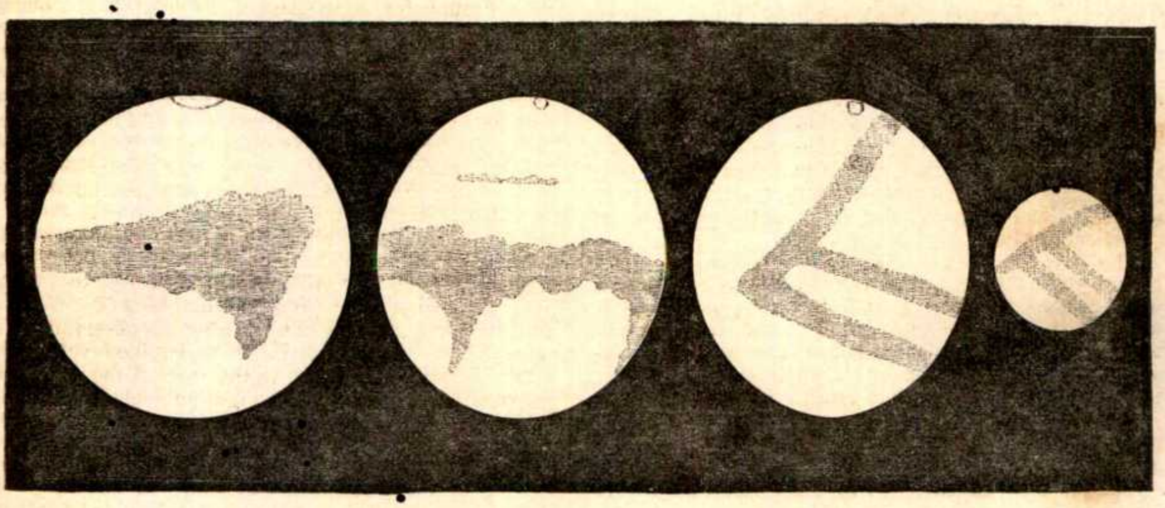
"Through the most barbarous fury, in consequence of an equally barbarous sentence, the whole unoffending soft "vale of lilies" (Lilienthal) was, without previous inquiry, destroyed by fire. Without possibility of succour, they burnt down also the Royal Government offices; I lost the whole of my furniture, and what was most distressing of all, with a considerable damage also to the bookshops of Europe, the sole stock of my collected works and writings laid up in the government buildings. Even my observatory, preserved by Providence from the conflagration, was a few days afterwards broken into, plundered, and through the destruction of the clocks, breaking off of the finders, and robbery of the smaller instruments, scandalously ruined. Having been displaced from my post, my income had been previously by degrees so very much reduced, that I was compelled to forego everything but absolutely necessary expenditure, and to be laid aside in a scientific slumber." Elsewhere he says that even his journals had perished; and at the date of writing the introduction to his "Observations and Remarks upon the Great Comet of 1811" (January 22, 1815), from which the foregoing passage is taken, he complains that his circumstances were still so reduced, that his observatory, for want of time and money, remained for the most part in a state of confusion. So great are even the minor miseries of those "wars and fightings," of which many speak with such apathetic unconcern. It is painful to add to these sad details that this seems to have been Schröter's final effort, for after a twelvemonth of bodily and intellectual decay, he expired August 29, 1816, leaving behind him a worthy memory, to which, till of late years, our own country has done but inadequate justice.

The "Areographische Beiträge" remained in MS. at his death, having escaped the calamitous fire, but so narrowly, that two out of the sixteen copper plates of figures had to be engraved again; no idea seems to have been entertained of publication, but they were safely preserved by the author's family. Their existence having been ascertained many years ago, by the present writer, through the kindness of Dr. Peters of Altona, a negotiation was set on foot for their acquisition by the Royal Astronomical Society; this proved ineffectual; but, in consequence of the attention directed to them, they were allowed to be inspected by Dr. Terby, to whose able and comprehensive analysis of the MS. as published in the *Memoires* of the Belgian Royal Academy of Sciences in 1873, the present notice is deeply indebted; and astronomers will be glad to learn that they have now been purchased by the University of Leiden for the library of that observatory, and that, after an obscurity protracted through seventy years, they have at last been published in a complete and handsome form, under the able and accurate editorship of the director of that institution.

The work, though characterised, like other productions of the same author, by a needless amount of prolixity, is well deserving of careful study, as indicating or confirming some valuable conclusions, and affording material for suggestive thought. The whole observations are pervaded by an impression that the obscurer portions of the disc are condensations in a vaporous atmosphere. The author, with a singular misconception of terrestrial analogy, supposes throughout that such cloudy masses viewed on their upper or enlightened side would appear darker rather than lighter than the surface beneath them; admitting at the same time that the configuration of that surface may so modify the superjacent atmosphere, as to cause a permanence, or, at any rate, recurrence of vaporous formation, from which the rotation may be, and has been determined. The occasional invisibility of dark spots, which has been recorded by too many observers to be brought in question, would be explained by Schröter in accordance with this theory, and may possibly be due to atmospheric causes; though, as Terby has pointed out, it may often have arisen from the difficulty of tracing any markings in the neighbourhood of the limb. It is more difficult to account satisfactorily for the movements ascribed by Schröter to the action of winds, of which he has specified in an elaborate table no less than forty-six instances, not much differing in velocity from those on earth, and in the great majority of cases conspiring with the direction of the rotation. Error may have crept in with regard to the identification of some of the spots; and both from the designs and descriptions a suspicion arises that some of the minuter details, which now serve for the recognition of distinct but similar regions, escaped the observer's notice. There must have been some cause for this unfortunate defect which detracts materially from the value of his work, and the removal of which would have been the one step in advance which, as Terby remarks, would have put him in possession of the true interpretation of what he saw. It would be a ready explanation to refer it to the imperfect defining power of his instruments; and I have somewhere read of a comparison instituted after his time between his much-prized speculum of 9½ inches aperture and 12⅔ feet focal length, and a Fraunhofer object-glass of, I think, about 4 inches, to the disadvantage of the former. Yet on the other hand he began his work with telescopes by the elder Herschel; and his 7-foot instrument by this great maker does not seem to have been superior to that of the same dimensions by Schrader, the manufacturer of all his larger ones. This point is therefore not quite clear. We might have attached some importance to his own admission, in his work on the moon, that his vision was less microscopic than that of his assistant, Harding, but for the fact that the latter occasionally aided him in these observations on Mars. Whatever may have been the cause, the frequent absence of minuter detail must have served to confirm Schröter's misapprehension of what he saw. And it must be borne in mind, in estimating his observations in general, that it was his habit to undertake investigation with a preconceived idea. In the case of the moon, his prepossession in favour of changes no doubt occasionally misled his judgment; and on Mars he might be prepared to look out for atmospheric movement by the known phenomena of Jupiter. An anticipation of this kind may not be without incidental advantage in directing and sharpening attention: our search may be aided, and with perfect fairness and honesty, by the foresight of the result: but at other times such expectations may be equally or more prejudicial; and they probably were so in the instance before us. And it is at any rate a possible suggestion that, with regard to some of these supposed changes, Schröter's ideas may have been unconsciously biassed by his study of the surface of Jupiter. The perspective foreshortening of that great globe having no effect on the aspect of its more conspicuous and familiar markings, from their equatorial direction, and being non-apparent in the transits of the shadows of the satellites, the eye may come to regard it too much as a flat disc, and to appreciate too little the extensive changes which mere foreshortening produces among markings arranged in any other direction.

But whatever explanation may be attempted of Schröter's illusion, which, as his editor remarks, increases the value of his figures by securing their perfect independence, or however we may regret an apparent want in some cases of more distinctive detail, there can be no doubt that the work is worthy of attentive study. Dr. Terby has pointed out one curious inference—that at that epoch certain dark markings bore a relative proportion to each other, too different from that which obtains at present to be easily explained away. Nor does this result by any means stand alone: and it has considerable value as affording collateral testimony to subsequent observations pointing in the same direction. To say nothing of other authorities, the accurate designs of the deeply-regretted Burton, and the latest delineations of Schiaparelli (independent of the wonderful duplication of the narrow streaks) concur with the drawings of Schröter in indicating one of two suppositions as regards the dark patches of Mars; either they must be liable to long-persistent and very deceptive alteration of visible outline from atmospheric causes, or their own extent must be so variable as to awaken a doubt whether the right key of the mystery is after all in our hands. We have long believed that we hold it, and terrestrial analogy has been thought sufficient to account for all we can see. The result of the next opposition in 1884 may be found to confirm the old hypothesis; but it is not beyond possibility that it may shake it, even past recovery.

Besides the conclusion thus briefly indicated, several other points of varying degrees of interest are touched upon in this comparatively bulky treatise, some of which we may refer to, though only with a passing notice. Schröter paid considerable attention to the polar whiteness; but while he admits the probable analogy of terrestrial snow, he is less confident than some other observers as to any marked influence of solar radiation. Terby, however, has pointed out the cause of his misapprehension, and his substantial agreement with his compeers. He was aware of the irregular outline of the snowy regions, and thought them slightly different in colour, the south pole verging towards yellow, the north blue. On the question of rotation he could obtain no satisfactory result, as might have been expected from his idea as to the instability of the markings; his values being discordant at different periods: the mean, 24h. 39m. 50·2s., was only about 29s. less than that of Sir W. Herschel, but very wide of Proctor's elaborately deduced value, 24h. 37m. 22·735s.—a fact pointing probably to the same conclusion as before, that from some as yet imperfectly explained cause, the exact position in longitude of some of the features of the planet is not fully ascertained. The amount of the polar flattening of Mars is, as is well known, matter of much uncertainty. Herschel made it as much 1-16; Dawes, nothing, or even negative. Our observer, nearest to the great English authority, found it less than 1-81, a quantity fairly evanescent. The method of measurement which he adopted throughout all his researches was that of the apparatus which he calls the "projection machine." In this simple contrivance both eyes are employed simultaneously, the one in viewing the telescopic image, the other in bringing to coincidence with it, a squared-out area in the case of the moon, a series of discs for the planets, in either instance with provision for varying distance and illumination. This binocular mode of measurement, if open to some sources of error not incidental to the ordinary apparatus, appears hardly deserving of the censure so freely bestowed upon it by Beer and Mädler, who were not always fair towards the labours of Schröter; and notwithstanding the perfection to which the wire micrometer has been brought, might perhaps be revived for some purposes with advantage. The diameter of Mars obtained in this way by Schröter, 9″·84, does not differ much from the 9″ 8‴ (a curious instance, by the way, of notation by *thirds*), of Sir W. Herschel, or from more modern values—some proof, it may be thought, of the competency of the apparatus to obtain a close approximation.



Observations included in this volume of a partial flattening of the limb of Mars and of the abnormal breadth and want of symmetry in the phasis, however improbable they may at first appear, are not without parallel in the case of other planets, or the experience of other observers. If, as it must be assumed, these are nothing more than illusions, the record of them is still valuable in the probable event of their occasional recurrence.

To this brief and imperfect notice are appended three sketches from the Atlas—the two first as specimens of Schröter's mode of delineation—the third as bearing so striking a resemblance to one of my own, shown on a smaller scale beside it, that it might, in the absence of more accurate data, serve as the basis of an approximate value of rotation.

The dates respectively corresponding to these designs are 1798, Sept. 9d. 8h. 4m.; Oct. 17d. 7h. 39m.; Nov. 13d. 7h. 10m.; 1862, Dec. 10d. 9d. 30m.

T. W. WEBB

DESTRUCTION OF LIFE IN INDIA BY POISONOUS SNAKES

IN January, 1870, being then in Calcutta, I collected statistical information which afforded proof that the loss of human as well as animal life in India from the bite of venomous snakes was very great; and as it seemed to me that this ought to be, to a great extent, preventible, I extended my investigations with the view of obtaining accurate information as to the characters and peculiarities of the venomous snakes themselves, the localities in which they most abound; the *modus operandi* of the poison; the circumstances under which the bites are inflicted; the value of any known remedies in the treatment of those bitten, and what measures might possibly be devised for diminishing this serious evil.

After a long and careful investigation of the whole subject, I drew up a detailed report, containing the results of my inquiry, and presented it to the Government of India, with a request that, when published, it should be distributed throughout India, among civil and medical officers, with a view of enabling them to take measures for the protection of human life, and the destruction of the creatures which caused such frightful mortality. I also endeavoured to point out the mode in which the poison destroys life, and to indicate such rational measures as might be of service in the treatment of those bitten.

I am not aware how far the advice I then tendered has been acted on, but I am glad to find, by a recent resolution published in the *Gazette of India*, that some progress is being made, and that the mortality of 1881 has been somewhat less than that of 1880, from this cause, and that this desirable result is due to the measures that have been taken by Government to procure the destruction of the poisonous snakes.

From the returns furnished to me at the instance of Government in 1870, for the year 1869, I made out that the human deaths from snake-bite were as follows in—

Bengal, including Assam and Orissa	6645
North-West Provinces	1995
Punjab	755
Oude	1205
Central Provinces	606
Central India	90
British Burmah	120
Total	11,416

These were the only returns received, and represent not much more than half of the whole area, but the total, large as it is, cannot be regarded as the real mortality in these provinces, as the information from which the records were framed being probably only partial and imperfect, it rather under-rates than exaggerates the mortality. I expressed a belief that if systematic registration were adopted, the number recorded would prove to be larger, whilst, if information were gathered from the whole of Hindostan, it would be found that not less than 20,000 persons are destroyed annually by snakes.

Certain suggestions were made as to measures for identification, destruction of venomous snakes, and for registration of deaths. These would appear, from the terms of the resolution above referred to, to have been partially adopted, with the result of causing some diminution of the evil. I pointed out that the snakes which are so destructive to life are the cobra, the bungarus or krait, the echis, and the daboia or Russells' viper, all of which are most conspicuous snakes, and easily identified. There are others, such as Bungarus fasciatus, Ophiophagus elaps, which are dangerous, but comparatively rare, and seldom bite men, whilst the hydrophidæ being confined to the sea or estuaries, are, though very poisonous, not so dangerous to man, and the trimeresuri, which are both uncommon, and at the same time are not so deadly as to endanger life. All these are depicted in coloured figures taken from life, which renders their identification simple and easy.

I further remarked that, "meanwhile there exists the obvious necessity of endeavouring to prevent the numerous fatal accidents by making generally known the appearance and habits of the poisonous snakes, and by instituting rewards for their destruction. With a plain description and a faithful representation of each species in colours, every district, medical or police officer, would be able at once to distinguish the venomous from the innocent snakes, and thus knowledge enough, at least for all practical purposes, might be imparted to intelligent native subordinates, to enable them to recognise the poisonous snakes. By offering a larger reward for these only, their numbers would soon diminish, and the people would be made acquainted with the characters that distinguish the venomous from the harmless snakes, and would learn to avoid them. Thus only, I believe, can the evil be remedied, so long, at all events, as the mode of life among the lower and agricultural classes remain what it now is. I would suggest that magistrates, district and police officers, and civil surgeons be authorised to give the following rewards for poisonous snakes:—

	Annas[1]
Cobra	8
Bungarus cæruleus	6
Bungarus fasciatus	4
Ophiophagus	8
Russell's Viper	8
Echis	4
Trimeresurus	2

The sum disbursed would no doubt be large, but the results in the saving of life and destruction of snakes would compensate for the expenditure."

Such was the state of things when I left India in 1872. The Government of India then, at my instance, appointed a commission to continue the inquiry which I had commenced three or four years previously. This resulted in several valuable reports by Drs. J. Ewart, A. Wall, and Mr. Vincent Richards, whilst, in conjunction with Dr. Lauder Brunton, F.R.S., an investigation into the nature of the physiological action of the virus was continued here by me, the results of which have been published in the *Proceedings* of the Royal Society in 1873, 1874, and 1875. Meanwhile the evil continues, and it is probably within the mark to say that, since the subject came under consideration in 1870, 150,000 to 200,000 human beings, to say nothing of domestic animals, have been destroyed by snake bites.

The subject has often received the most anxious consideration of the Indian Government, and a variety of measures have been resorted to, not without a certain measure of success; but it is my belief that not until a system of organised, determined, and sustained efforts for the destruction of the snakes is adopted and carried out on the lines suggested in my report, will the evil be fairly grappled with and overcome. The present resolution shows that the matter is again receiving some consideration, and there is good reason to believe that if the measures be prosecuted with energy and determination throughout India, good results will follow. But I repeat it is only by the *destruction of the snakes* that the evil can be mitigated. Something may be expected from the people themselves as their knowledge of the subject increases, as they become more familiar with the appearance or character of the venomous as distinguished from the harmless snakes, and as they gradually become convinced of the futility of all antidotes charms or spells to protect them; or should they ever alter their present mode of living in huts which have the floor on the ground surface, to huts with raised floors—a consummation devoutly to be wished, not only on account of snakes, but of malaria—but hardly likely to be realised.

For the purpose of hunting out and destroying the evil it is absolutely necessary that a fixed system of rewards should be established, and that in every district there should be an organised body of men whose duty it would be, under proper supervision, to seek out and destroy the snakes, receiving a recompense according to the importance and number of the snakes killed. Such men are to be found among certain castes, and with the aid of descriptions and coloured drawings, such as now are available, there need be no great difficulty in carrying out this much-to-be-desired object. That such a project would be costly is true, but can that cost be considered excessive if it save thousands of lives of men and valuable animals? There can be little doubt that wherever such a system has been even partially carried out, it has been effective; it needs but combined effort to make universal, that which hitherto would appear to have been but partial success.

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[1] Eight annas represent one shilling.

From the tenor of the Government resolution referred to, it seems as though an organised scheme for the destruction of venomous snakes, as well as dangerous wild animals, is now likely to be generally adopted in India, and should it be so, there is good ground for hope that the great mortality will decrease—to quote from a former paper on this subject, I would repeat: "Rewards should be offered freely for venomous snakes only. This, if steadily carried out under some responsible official, would soon diminish snakes and deaths from snake-bite; and I earnestly protest against the opinion expressed by some Indian authorities, that such rewards are useless—useless they may have been, and will continue to be, if distributed without discretion for snakes not poisonous. If this method of dealing with the matter—and who can deny its importance—be adopted (but it must be done willingly, and not with the foregone conclusion that it will fail), I am certain that, as part of a comprehensive scheme for the destruction of noxious animals generally, it will succeed."

The following is the purport of the resolution of November 8, 1882, which shows that in 1881 the number of deaths caused by snake-bite, of men and animals, contrasted favourably with that of the previous year, 1880.

The statement appended to this resolution shows in detail for each province the number of persons and cattle killed by wild animals and snakes, and the number of wild animals and snakes destroyed, with the rewards paid for their destruction during the year 1881, as compared with the previous year. The figures are summarised in the following tables:—

Number of Human Beings and Cattle Killed by Snakes

	Persons killed 1880.	Persons killed 1881.	Cattle killed. 1880	Cattle killed. 1881.
Madras	1,182	1,064	227	273
Bombay	972	1,024	89	191
Bengal	10,064	9,208	1,248	154
North-Western Provinces and Oudh	4,723	5,010	221	317
Punjab	681	744	78	69
Central Provinces	901	985	39	26
British Burma	149	135	194	150
Coorg	3	Nil	Nil	Nil
Assam	211	189	57	16
Hyderabad Assigned Districts	125	197	383	836
Ajmere-Merwara	49	54	Nil	Nil
Total	19,060	18,610	2,536	2,032

Snakes killed and Rewards Paid

	1880 Destroyed	1880 Rewards Rs	a	p	1881. Destroyed.	1881. Rewards. Rs	a	p.
Madras	Nil	Nil			Nil	Nil		
Bombay	177,078	6,922	3	6	207,113	6,214	0	0
Bengal	23,201	3,733	3	6	19,282	3,430	5	0
N.-W. Provinces and Oudh	1,029	10	2	0	1,142	56	5	3
Punjab	9,126	635	5	0	22,279	1,587	4	0
Cent. Provinces	866	336	6	0	1,493	562	8	0
British Burma.	997	2	0	0	2,990	27	0	0
Coorg	58	Nil			16	4	0	0
Assam	202	Nil			300	34	0	0
Hyderabad Assigned districts	158	23	14	0	332	45	8	0
Ajmere-Merwara	61	Nil			21	Nil		
Total	212,776	11,663	2	0	254,968	11,960	14	3

The deaths of human beings from snake-bite were, in 1880, 19,060; while in 1881 they were 18,610.

In 1880, 212,776 snakes were destroyed at a cost of Rs 11,663.

In 1881, 254,968 snakes were destroyed at a cost of Rs 11,961.

Thus with an increased expenditure of Rs 298 in 1881, 42,192 more snakes were destroyed and 450 lives were preserved, above the expenditure of the previous years.

With regard to the measures adopted for the destruction of venomous snakes, the following remarks are made by the Governor-General in Council:—

"As regards the destruction of venomous snakes, special measures were adopted in some provinces, of which it appears desirable to give a brief account in case they may be considered suitable for adoption elsewhere. In Bengal a scheme has been sanctioned by the local Government in the case of the Patna Division, under which persons destroying snakes can obtain certificates from certain selected planters vouching for the poisonous nature of the snakes destroyed. The production of such a certificate entitles the holder to secure from the local authorities the reward offered whenever he finds an opportunity of applying for it. As observed by the Government of Bengal, this concession will probably be found to add much to the convenience of persons claiming rewards, and to act as an inducement towards the destruction of poisonous snakes. The expediency of extending the scheme will be considered by the Local Government when the result of the current year's operations are known. In the North-Western Provinces and Oudh the Lieutenant-Governor and Chief Commissioner has sanctioned the entertainment tentatively in each district of those provinces of a staff of Kanjars, or men of similar caste, who trap and kill reptiles, for the systematic destruction of venomous snakes. These men will receive pay at the rate of Rs. 2 per mensem, together with an additional reward of two annas for every venomous snake in excess of twenty destroyed by each man during any month. A gang of snake-hunters is also to be employed at each tahsili, and, if the measure proves successful, it is proposed that similar gangs should be eventually appointed to each police circle of other local area. It appears to the Governor General in Council that a plan for the destruction of snakes such as that initiated in the North-Western Provinces and Oudh, is likely to prove far more efficacious than the mere offer of rewards, although it is true that unless such operations are confined to towns and villages and their neighbourhood, where it is believed that the largest number of deaths occur from snake-bite, they will probably be very costly. His Honour the Lieutenant-Governor of the Punjab has issued a circular to commissioners and superintendents in the Punjab, drawing attention to the matter with a view to the adoption of measures for destroying snakes by system of rewards to be granted by district committees and municipalities. Casts and lithographed pictures of the more common species of deadly snakes have already been supplied to the police stations in some districts, and deputy commissioners have been requested to suggest to municipal and district committees the desirability of procuring similar means of reference for the purpose of testing applications for rewards. In British Burma the Chief Commissioner, with a view to encourage village snake-hunts in the rice plains, has arranged to grant sums varying from Rs 10 to Rs 20, according to the number of houses, in aid of a feast or *pweh* at the end of the annual hunt to every village which successfully carries out such an undertaking.

"On the whole, the results recorded during the year under review appear to the Government of India to be more satisfactory than those of the previous year. The Governor General in Council is glad to notice that the question of taking measures to reduce the lamentable loss of life which is at present caused by wild animals and venomous snakes is receiving the earnest consideration of Local Governments and Administrations, and His

Excellency in Council will await with interest the reports showing the results of the special measures which have been adopted in some provinces. It is clear that much still remains to be done; but if sustained efforts are made and well-considered plans adopted for the extermination of wild beasts and deadly snakes, His Excellency in Council believes that the number of deaths from these causes will in course of time be materially reduced.—Simla, November 8, 1882."

From the above it appears that more vigorous measures than any hitherto adopted have been taken for the destruction of venomous snakes, and the contrast of the results of 1881 with those of 1880, warrant the anticipation of further benefit if these measures are only carried out with a sustained determination to succeed. It is mainly a question of perseverance and the expenditure of money, and one can hardly imagine a more desirable object on which to expend both energy and rupees. But it is essential that the system be laid down on some general principles for the whole of India, to be worked out in detail, according to the needs or peculiarities of each district. There should, in short, be a department with a responsible chief and subordinate agents, for whom certain rules should be laid down to be carried out steadily and without hindrance throughout the country, leaving much of the detail to the discretion of local authorities. I would insist on the importance of carrying it out on broad principles everywhere. When such a department is constituted under a proper head—and there are many persons well fitted for such a duty—then, I believe, venomous snakes and other noxious animals will decrease in numbers, and people will cease to be startled by these appalling losses of life.

J. FAYRER

SIR J. WHITWORTH'S MECHANICAL PAPERS[1]

THE fact that, by an order in Council of August 26, 1881, some 300 Whitworth gauges of various dimensions have been adopted as standards by the Board of Trade, is so important a recognition of the value of the labours of Sir J. Whitworth in improving mechanical measurement, that the occasion has been selected for republishing certain papers which have been long well known among engineers, but which have not hitherto been accessible to the public generally.

The first paper in the series is on plane metallic surfaces, and the proper mode of preparing them, and contains an account of an invention of great simplicity, but of the highest practical importance. Such plates, when worked up to an extreme degree of accuracy and finish, form an approximation to a plane surface which would surprise and delight any geometrician who had an opportunity of critically examining and testing their qualities. They consist of an assemblage of minute bright surfaces very evenly distributed over a plate of cast iron, and very near together.

As to their qualities, there is not space here to describe them, but they have formed the subject-matter of an excellent lecture by Prof. Tyndall, at an evening meeting of the Royal Institution in the year 1875.

Passing from these so-called true planes, we refer to a step involving an original conception which has led to the construction of the new standard gauges. The production of an approximately true plane surface gave an increased value and importance to the feeling of contact between prepared metallic surfaces, and resulted in the invention of a measuring machine which was made to depend on the sense of touch instead of upon optical contrivances, and was founded entirely on truth of surface.

The improvement consisted in the substitution of end for line measure, and inasmuch as these are technical terms, it may be well to explain them.

As stated in the last paper of the series, the English standard yard is an example of line measure, being represented by the interval between two lines drawn across two gold studs sunk in a bronze bar about 38 inches long, the temperature being at 62° Fahrenheit.

The standard yard, from the subdivisions of which the standard inch has been obtained on the Whitworth system, is a rectangular metal bar with plane sides capable of resting along its whole length in rectangular V grooves, which are plane surfaces, while the ends of the bar are planes lying perpendicular to its axis. The bar is exactly 36 inches long, and the measurement is complete when the degree of contact between its ends and two small true planes abutting against them is ascertained. Such a measurement is, of course, end measure, and its accuracy depends throughout upon truth of surface, and also upon truth of position of surface. The ends of the bar must be perpendicular to its axis, and the planes which feel those ends must be truly parallel to each other, and one at least must be movable to and fro without deviating at all from the position of parallelism to its fixed neighbour.

Then comes the question of the amount of shifting of the movable plane. That is done by a micrometer screw, the linear motion for one graduation of the micrometer head, which can be easily read without a lens, being in some cases 1-10,000th of an inch, and in other cases 1-1,000,000th of an inch.

There is not space to discuss the measuring machine, whether as capable of producing cylindrical gauges varying by 1-10,000th of an inch, or as capable of reproducing a standard inch or a standard yard to a degree of accuracy which leaves the microscope far behind in the contest.

It must suffice to point out that the reprinted papers are full of interest, as showing the manner in which Sir J. Whitworth has thought out and accomplished the work of improving the construction of machinery, and it is matter of regret that those who are occupied in teaching mechanics have not better opportunities than now exist of becoming practically conversant with the subject-matter of the collected papers.

NOTES

FROM Punta Arenas, near the extremity of South America, intelligence has been received that the fourth section of the German expedition sent out to observe the transit of Venus has been particularly successful, Professor Auvers having managed to take exceedingly good photographs and numerous measurements.

A TELEGRAM received from Monte Video states that the *Volage* has anchored in these roads from Santa Cruz in Patagonia. Capt. Fleuriais and observers of the transit of Venus were on board, returning to France with their instruments, photographs, and other documents.

M. TRÉPIED, in a communication to the Paris Academy on his observation of the transit in Algiers, states that clouds rendered the ordinary observations of little value, but that some good results were obtained with the spectroscope on the borders of the planet in the region from A to E; while some photographs were obtained in the green, the blue, and the violet. The examination of the spectral lines in the groups A, B, *a*, in the regions comprised between *a*, D, E, did not show, M. Trépied states, anything which could be attributed to a selective absorption produced by an atmosphere on the planet. The same inference is deduced from the photographs.

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[1] "Papers on Mechanical Subjects." By Sir Joseph Whitworth, Bart., F.R.S., D.C L, Vol. I. True Planes, Screw Threads, and Standard Measures. (London: Spon.)

AT a recent conference of members of the British Association, held in the rooms of the Geographical Society, a protest was drawn up against the proposed meeting of the Association in Canada in 1884.

AT the Conference of Head Masters the other day one of the subjects discussed was the teaching of geography in schools, in which many varied and vague opinions were expressed. There is in this country too great a tendency to treat geography as mere topography, the mere dry bones of the subject, which can only be clothed with flesh and endowed with life through the medium of the physical and natural sciences. We advise those teachers who desire to make the subject of geography both interesting and useful to make themselves acquainted with the programmes of German schools and universities under that head.

THE Société d'Encouragement has held its annual meeting for 1882, and awarded its gold medal to M. Gaston Planté for his work in the accumulation of electricity.

IN a recent number of the St.-Petersburg Academy's *Bulletin* (1882, t. xxviii. p. 163), Herr Kortazzi reports on his observations of Jupiter at Nikolajus from September, 1879, to December, 1881, giving, in four plates, forty-seven drawings of the planet. The time of rotation, calculated from the red spot, he finds to have continually diminished, but not according to an ascertained law; more recently an increase has appeared. The spot can hardly (he considers) be regarded as gaseous; it is more likely a liquid or even solid mass forming part of the planet's surface. In the former case it might be considered a large lake in an ocean of other liquid, which covers the southern hemisphere of Jupiter, and it might be expected that this lake, owing to currents flowing over the surface of the planet, would gradually be diffused and spread over the whole surface, or at least over the whole parallel. If the spot were a solid projection from the solid body of the planet (if such a body there be), it would be impossible to account for the observed changes of position. The most plausible view is, that it is a solid floating mass on the surface of an ocean; but even this hypothesis the author considers bold, since we are not entitled to infer by analogy from terrestrial phenomena the nature of forms on Jupiter which may be very different in internal nature from the earth.

A DOE having horns, which gave it the appearance of the male animal, was recently killed in the woods of Herr Pönsgen, near Aix-la-Chapelle. The longer horn was about 19 centimetres in length. Such a case is rare, though small rudiments of horns are sometimes met with in old does. A picture of the animal is given in the *Revista Scientifico-Industriale*, October 31.

To illustrate the effect of expansion of the bulbs of liquid thermometers on the indications of those instruments, Prof. Govi connects a capillary tube with a bulb of ebonite, and partly fills it with mercury. Such a thermometer does not indicate gradual variations of temperature (within certain limits). With rapid variations, the mercury shows *inverse* movement (descending with heat and rising with cold), but after some time the original level is restored, while the excess of heat or cold is lost. The phenomena are due to the almost perfect equality of the coefficients of cubical dilatation of ebonite and mercury, at least between zero and 50° or 60° C.; and to the fact, that with sudden changes of temperature, the bulb responds first, and being a bar conductor, transmits slowly to the mercury.

A RECENT report by M. de Bezerédy, Government Commissioner for cultivation of silk in Hungary, shows that the industry is making considerable progress in that country. In 1881 there were 2976 producers, who obtained 41,537 kilogrammes of cocoons in 426 communes, and the produce was sold for 41,816 florins. The corresponding figures for 1880 are: 1059 producers, 10,132 kgr., 109 communes, and 11,062 florins. The Commissioner sold in Italy the produce of 1881 for 63,000 florins, and the profit so realised allowed of the institution of a model school for silk-cultivation, without exceeding the credit voted by the Chamber. This school has received three primary teachers sent by the Minister of Public Education, and three from the Minister of Commerce; three more are maintained at private expense. These nine will acquire knowledge to be afterwards utilised in their place of residence. Further, a professor in the Model School of Graz has given public lectures on the rearing of silkworms in several villages, and more than 80 kgr. of cocoons have been distributed continuously to cultivators. Lastly, 28,956 mulberry trees have been planted at Government expense. The report recommends the establishment of spinning mills in the country, and the plantation of mulberry trees on land belonging to the communes, and on the Government roads. The climate of certain regions of Hungary is highly favourable to the production of silk.

THE *Times* Geneva correspondent states that the recent heavy rains, which recommenced on Friday, with, if possible, greater violence than before, are producing disastrous consequences in various parts of Switzerland. A considerable extent of ground, covered with vines, at Espesses, in Canton Vaud, is slipping rapidly towards Lake Leman, and, unless the measures taken by the engineers succeed in arresting its progress, must soon be engulfed. An earthslip has also taken place near Troistorrents, and another at Pully, in the same neighbourhood. Up to the end of November there had been 200 rainy days in that part of Switzerland since the beginning of the year, and only 50 days of sunshine.

AN international exhibition will be held in Calcutta next December. There will be nine principal sections: (1) fine arts; (2) apparatus and application of the liberal arts; (3) furniture and objects used in dwellings; (4) clothing, including fabrics; (5) products of mining industry, forestry, &c.; (6) apparatus and processes in the common arts; (7) food; (8) artisans' workmanship; and (9) children's work. An attempt will also be made to hold an exhibition of live stock, agricultural and horticultural products, and of a loan collection of paintings, sculpture, and works of art generally. The usual gold, silver, and bronze medals will be awarded by special juries of experts. The exhibition will be opened on December 4, 1883, and will close on February 29, 1884.

M. GERMER BALLIÈRE has published an edition of Father Secchi's "Les Étoiles" in two volumes, as a part of the Bibliothèque Scientifique Internationale.

THE Académy of Moral and Political Sciences has announced the conditions of the competition opened every year for the prize of 200*l*. to be devoted to the author of the work, which is to "faire aimer," morality and virtue, and "faire repousser," vice and egotism.

M. DE CHANGY, the first electrician who attempted to manufacture incandescent lamps *in vacuo* about twenty years ago, has constructed a small model for demonstration. The carbon is rectilinear, which permits a very small length to be given to it. It is to be lighted with bichromate of potassium elements. In his former attempts M. de Changy advocated very small carbons cut in the graphite from the retorts. Now his fibres are carbonised according to the common practice.

THE introduction of western improvements into China by Europeans is evidently a work beset with many difficulties. Some years ago the only railway in the country was purchased by the Government from the proprietors and promptly torn up; but now the officials themselves are laying down railroads from the mines in North China to the nearest canal. The telegraph also had to encounter a vigorous opposition from the authorities and people for many years; at present, however, the capital is connected by wire with the coast. The electric light is the latest improvement which has excited the suspicion and dislike of the Mandarins. The foreign settlement at Shanghai has for some time been lighted on the Brush system, apparently much to the comfort and jubilation of the denizens of the "model settlement," as the foreign portion of the city is generally called. The promoters appear, however, to have reckoned without the Chinese officials. They probably thought that where gas was permitted, there could be no objection to electricity. The Chinese Governor of the district appears to be of a different opinion. He has addressed a letter to the senior Foreign Consul requesting the removal of all the electric lamps. He has read, he says, in translations from European papers, that terrible accidents have arisen from electricity, and flatly refuses to permit the residents of Shanghai to be exposed to such dreadful risks. Hundreds of thousands of houses might be destroyed, millions of lives might be lost; even the walls of the city might be blown down if anything went wrong with the machines. He has strictly forbidden his own countrymen to use it, and has peremptorily ordered those who have already adopted it to discontinue it forthwith. Whether this ukase will be immediately obeyed or not it is impossible to say; but past experience leads us to the conclusion that if the Chinese have determined to set their face against the electric light, no power on earth can get them to permit it in their territory. Their leading principle in these matters seems to be a dislike of all innovation until its necessity is clearly demonstrated by *their own* experience, and a determination that new inventions or appliances shall not be foisted or forced on them from outside. The late difficulty with Russia showed them the imperative necessity of being prepared for war, and of having their capital in direct communication with the outer world. Ironclad ships and rifled guns are accordingly being purchased with extraordinary rapidity; forts are being erected at various points on the seaboard, and a telegraph line about 800 miles in length was constructed in the course of a few months. Perhaps, after all, the Chinese policy in this respect is not so wrong-headed as it sometimes appears. It certainly saves them from the wiles of speculators and promoters of all sorts.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus radiatus* ♂ ♀) from India, presented by Mr. Nathaniel Cotton; two Slender Loris (*Loris gracilis*) from India, presented by Dr. H. W. Lentaigne; a Leopard (*Felis pardus*) from India, presented by Capt. Park; a Crimson-crowned Weaver Bird (*Euplectes flammiceps*) from Madeira, presented by Mr. E. W. Gain; a Common Heron (*Ardea cinerea*) from Scotland, presented by Mr. W. H. Henderson; eleven Muscovy Ducks (*Cairina moschata*) from South America, presented by Major Finlay; a Hoary Snake (*Coronella cana*), a Crossed Snake (*Psammophis crucifer*), a Rhomb-marked Snake *Psammophylax rhombeatus*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Golden-winged Woodpeckers (*Colaptes auratus*) from North America, purchased; a Golden-Eye (*Clangula glaucion* ♂), British, on approval; a Molucca Deer *Cervus moluccensis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

STELLAR PARALLAX.—The results of a series of observations with the filar micromerer on the Washington refractor for the determination of the annual parallax of α Lyræ and 61 Cygni have been printed in advance of the publication of the yearly volume of observations. The measures were made by Prof. Asaph Hall, those of α Lyræ extending from May 24, 1880, to July 2, 1881, on seventy-seven nights, and those of 61 Cygni from October 24, 1880, to December 7, 1881, on sixty-six nights. The magnifying power employed was 383. Prof. Hall remarks that since observations of the angle of position made with the micrometer-circle are less accurate for distances that enter into the determination of parallax, he observed simply the difference of declination of α Lyræ and the companion of the tenth magnitude, and in the case of 61 Cygni the difference of declination of the smaller component and a star of 9·5 magnitude about 3′·3 south of the double star, which is D.M. + 38°, No. 4345. α Lyræ was observed both with bright and dark wires, for 61 Cygni only the dark wires were used. It may be noted that the star measured is the following component of the double star. The course of observation pursued for each night's set of measures is described, and except on one occasion the same programme was followed throughout.

The resulting parallax for α Lyræ is, 0″·1797 ± 0″·00561; the time required for light to pass from the star to our sun is thus found to be 18·11 Julian years.

For 61 Cygni the parallax is 0″.4783 ± 0″·01381, and light requires 6·803 Julian years to traverse the space that separates this star from the sun.

The parallaxes it will be seen, are obtained by the differential method, and are thus relative, or they are the differences of the parallaxes of the two stars. To get the absolute parallax of the bright star it is necessary to add the parallax of the small star. Prof. Hall says that he might have effected this by means of the parallaxes for stars of different magnitudes given in Struve's table in his "Études d'Astronomie Stellaire," but as the whole matter is uncertain, he has omitted this reduction.

Dr. Ball, Astronomer Royal for Ireland, continues his researches on stellar parallax, at the Observatory of Dunsink, Dublin, and has lately published a determination of the parallax of 6 (Bode) Cygni, which is the well-known double star No. 2486 of Struve. The components are of 6 and 6·5 magnitudes. The existence of a parallax to a very measurable amount was suggested during the course of a series of preliminary observations in 1879 and 1880 for the detection of such proximate objects, and a systematic course of observation was commenced on October 3, 1880, and continued to December 22, 1881. 6 B. Cygni is No. 196 of Argelander's list of 250 stars having large proper motion, given in vol. vii. of the Bonn Observations, where it has attributed to it an annual motion of 0″·636 on an angle of 346° 27′; Argelander's positions belong to the preceding component. Dr. Ball has employed the following one in his investigation. Measures were obtained on twenty-six nights, the mean date being 1881·5207; they were made from a star *n.f.* of the 10·5 magnitude, the adopted mean distance of which is 170″·692, and position angle 78° 18′ 61″. If this small star is assumed to be at rest, and Argelander's proper motion attributed to the double star, the annual increase of distance is + 0″·02, and that of angle + 12′·796; thus almost the whole proper motion applies to change in the position angle. The observations show that there is no regular increase of distance, and hence, Dr. Ball observes, there is *primâ facie* evidence that the comparison star does not participate in the proper motion.

The resulting parallax of Σ 2486 is—

From the distances … … … … 0″·5039 ± 0″·060
From the angles … … … … 0·383 ± 0·13

Combining these two values, we have for the parallax 0″·482 ± 0″·054. It is intended to make another series of observations of this star, the present result being regarded by Dr. Ball as merely provisional, though he thinks it can hardly be doubted that a parallax of very considerable amount really exists. The place of the star is in right ascension 19h. 8m. 20s., with 49° 35′·3 north declination for 1855·0.

COMET 1882 *b*.—The following positions of this comet for midnight at Greenwich, though liable to an error of several minutes of arc, may serve for finding it in the telescope without difficulty.

	Right Ascension.		Declination.		Log. distance from	
	h.	m	°	′	Earth.	Sun
Dec. 31	7	14·9	−29	28	0·2365	0·3883
Jan. 2	7	9·2	29	10		
4	7	3·7	28	49	0·2489	0·3991
6	6	58·4	28	26		
8	6	53·3	28	2	0·2622	0·4096
10	6	48·4	27	36		
12	6	43·8	27	8	0·2763	0·4196
14	6	39·4	26	40		
16	6	35·2	26	10	0·2912	0·4294
18	6	31·2	25	40		
20	6	27·4	−25	9	0·3067	0·4388

AMERICAN RESEARCHES ON WATER-ANALYSIS

AT the instance of the U.S. National Board of Health, a comprehensive investigation relating to analysis of water has been recently conducted by Prof. J. W. Mallet, F.R.S., of Virginia, assisted by several chemists and others. It was proposed to examine carefully the chief processes in use for chemically determining the organic matter or its constituents, in drinking water, to test the absolute and relative accuracy of the results these processes are capable of yielding, and, as far as possible, to ascertain the nature and scope of the practical conclusions available for sanitary purposes, which might thence be secured.

A preliminary report of this inquiry has appeared in a recent supplement (No. 19) of the National Board of Health *Bulletin*, and we propose here to give our readers some idea of the nature of it.

Three chemists took charge severally of the so-called "combustion process" of Frankland and Armstrong, the "albuminoid ammonia process" of Wanklyn, Chapman and Smith, and the permanganate process as advocated by Tidy; they were, Mr. W. A. Noyes, who worked at Baltimore, Dr. Ch. Smart, U.S.A., at Washington, and Dr. J. A. Tanner, U.S.N., at Virginia. The water-samples were collected and distributed to these three places by Prof. Mallet, and all three chemists were required to examine their several samples on the same day. Prof. H. Newell Martin undertook a simultaneous microscopic examination, and pathological observations on the effect of injecting the waters, concentrated by evaporation at a very low temperature, under the skin of rabbits.

After a large amount of preliminary and special work, nine classes of waters were obtained or prepared, for the main series of test analyses, as follows :—

Class I.—Natural waters, believed to be wholesome (including the water-supply of some of the principal cities).

Class II.—Natural waters which were believed to have actually caused disease in those who drank them.

Class III.—Natural waters of doubtful, but more or less suspected character.

Class IV.—Artificially prepared waters, made by adding to wholesome water, certain amounts of various infusions of *vegetable* organic matter, such as drinking water is liable to be contaminated with.

Class V.—Waters prepared with various forms of *vegetable* refuse, from manufacturing or industrial operations.

Class VI.—Waters prepared with *animal* (or partly animal) organic matter of natural origin.

Class VII.—Water prepared with *animal* refuse from manufacturing or industrial operations.

Class VIII.—Water prepared by adding morbid products of human disease.

Class IX.—Solutions, in distilled water, of carefully determined amounts of pure substances of definite chemical compositions.

The report first deals with the *degree of accuracy* of the three processes examined, a matter which may be looked at in two ways: first, as to the concordance of the results of each process in duplicate or triplicate experiments on the same water; and secondly, as to the agreement of the results with the actual, quantitatively known, contents of a particular water.

In the case of multiplied experiments on the same water, then, the author first shows in three successive tables (for the three processes), the divergence of individual results from the mean, as *percentage on the mean*. It appears that, on the whole, the most closely concordant results were furnished by the permanganate process, and the least so by the combustion process, the albuminoid-ammonia process holding the intermediate position.

Next, a table is given showing the extent of agreement of results obtained by the different processes with quantities of organic constituents known to be actually present. The figures strikingly indicate certain important defects of the several processes, though (it is pointed out) they must be looked at in a broad, general way, remembering the small number of organic substances treated and their special characters.

The Report proceeds to deal with the effects on the results by the different processes of varying the extent of dilution of the same organic substances in water. Here, as regards the combustion process, distinct confirmation is had of the existence of two forms of constant error affecting evaporation. The weaker the solution the less is the amount of organic carbon obtained, and the larger the figure for organic nitrogen. If the usual interpretation of Frankland's C : N ratio be applied, the curious and important result of these sources of error follows, that the more dilute an organically-polluted water is, the more animal-like in origin will its polluting material seem to be; while the stronger it is, the greater will be the tendency to refer the contamination to a vegetable source—a distortion of conclusions manifestly in the opposite direction to that commonly assumed to be safe.

As to the albuminoid-ammonia process, the weaker the solutions, the higher are the results obtained, both for free and albuminoid ammonia, the influence common to both being probably that of imperfect condensation in the distillation having a less effect as the quantity of ammonia is stronger. The lower results from the stronger solutions for albuminoid ammonia, may be partly due to the fixed charge of alkaline permanganate to the quantity of organic matter to be acted upon.

The results of the permanganate process are shown to be much less influenced by varying dilution within the limits of those experiments than those of the other processes.

The following, briefly stated, are the author's

Special Conclusions as to the Combustion Process

1. The combustion itself, carried out according to Frankland's directions, is a process of great delicacy, and quite satisfactory in its details, with proper precautions, and in trained hands. Gaseous volumetric analysis with the aid of the Sprengel vacuum, is sufficient.

2. The Frankland process is quite within reach of the manipulative skill of any fairly-trained chemist, but it requires practice and probably pretty constant practice. It is better adapted for large public laboratories, where many samples of water are examined, than for occasional use by a private individual.

3. The defective point is the failure of the evaporation to leave a residue representing the original organic matter; this (suspected hitherto) is regarded as now established. There are two constantly present errors (greatest with little organic matter), viz., loss of carbon, and gain of nitrogen from the atmosphere, the latter probably partly balanced by loss of that originally present in the water.

4. These errors, affecting unequally the carbon and nitrogen, are liable to alter the statement of the C : N ratio, and so distort sanitary conclusions.

5. The result for organic nitrogen, by the combustion process, is also affected indirectly by the errors connected with determination of the ammonia ("free ammonia"), as the nitrogen belonging to this has to be subtracted from the gross result.

6. A further, indirectly operative, cause of error as to the nitrogen arises from the varying loss of ammonia by dissociation of its salts during evaporation. The time occupied in evaporation, as well as the amount of ammoniacal salts present, will influence the amount of loss. Such error will mostly be very small, but may affect considerably the result for *organic* nitrogen.

7. The presence of nitrates presents a special difficulty in the combustion method. Mr. W. Williams's application of the copper-zinc couple deserves more precise quantitative examination than it has yet had. The simultaneous presence of urea with nitrites and nitrates, presents a case of peculiar difficulty, with several sources of error.

8. The formation of sulphuric acid during evaporation with sulphurous acid seems to occur oftener than has been recognised. It is much to be deprecated, though its effect, at any rate on the carbon determinations, has not seemed so great as might have been expected.

9. The combustion process, in its present form, cannot be considered as determining the carbon and nitrogen in a sense to justify the claim of "absolute" value for its results, which has been denied to those of other methods. It is but a method of approximation, involving sundry errors, and in part, a balance of errors.

10. There is, however, good ground for believing, that in many, perhaps most cases, its results for organic carbon may, with proper precautions, be made more valuable than the indications of the permanganate process, and its results for organic nitrogen more valuable than the indications of the albuminoid ammonia process.

Special conclusions as to the Albuminoid-ammonia Process

1. In the determination of both "free" and "albuminoid" ammonia there is a loss, which may be quite considerable, from imperfect condensation of the ammonia during distillation. This varies especially with the efficiency of the cooling apparatus and the time occupied.

2. Where waters contain urea and other amidated bodies, such as the leucine and tyrosine of putrefactive decay, some ammonia is so easily formed from these substances by boiling with sodium carbonate, or even without this addition, that it is impossible to distinguish sharply between pre-existing "free" ammonia (of ammoniacal salts), and that formed by the action of alkaline permanganate, the so-called "albuminoid" ammonia. This source of error as to free ammonia reacts (as noticed above), on the result for organic nitrogen by the combustion process.

3. There is no satisfactory evidence for Wanklyn's view, that, in distilling with alkaline permanganate, definite and simple fractions of the nitrogen of organic matter are given off as "albuminoid" ammonia. Such results may be varied at pleasure for most substances, by modifying the conditions of distillation.

4. If the distillation with alkaline permanganate be carried out according to Wanklyn, the nitrogenous organic matter is often so gradually acted upon, as to make the ending of the process indefinite; ammonia is still coming off when the distillation has to be stopped, the contents of the retort being nearly dry. Here an unknown fraction of the possible amount of albuminoid ammonia fails to be collected, and is but vaguely indicated by + after the figures recorded.

5. There is evidence that in some cases nitrogenous matter is volatilised during the distillation for free ammonia, which, if it had been retained, would have yielded up its nitrogen as albuminoid ammonia. Not affecting the Nessler reagent, such matter escapes detection.

6. The albuminoid-ammonia process proper, *i.e.* distillation with alkaline permanganate and determination of the ammonia evolved, is admittedly simple, and easily carried out with very little preparatory training.

7. The value of the results by this process depends more on watching the *progress* and *rate* of evolution of the ammonia than upon determining its total amount.

8. The recorded results by this process show a good deal of similarity between the figures for albuminoid ammonia and those for organic nitrogen (by the combustion process), but with frequent discrepancies of varying extent, such as prevent the one being taken as the accurate measure of the other.

Special Conclusions as to the Permanganate Process

1. The results by the Tidy method (the acidified permanganate at common temperature) and that of Kubel (operating at boiling point) differ irregularly from each other, the latter usually giving much higher figures, as was to be expected, but the ratio between the results by the two methods varies much in different cases.

2. On the whole, there seems to be a nearer approach to proportionality with the quantities of organic carbon found by the combustion process on the part of the Kubel process than on that of the Tidy process, but to this there are some very notable exceptions.

3. In a good many cases, the Kubel results are, contrary to the general rule, lower than those by the Tidy method. This seems to be due to loss of organic matter by volatilisation with the escaping steam from the boiling liquid before time has been afforded for action on the permanganate. Of course, a similar loss may have occurred in other cases, but not to the extent of reversing the general rule; and this may in part explain the absence of any uniform ratio between the figures yielded by the two methods.

4. The results by the Tidy process are liable to variation with the atmospheric temperature at the time of operation.

5. The amount of oxygen consumed by a specimen of water is probably in all ordinary cases much below that required for complete oxidation of the organic matter present, and does not stand in any fixed ratio thereto; it cannot be taken either as a measure of the organic carbon or of the total organic matter. Still a distinct general resemblance may be traced between strongly marked results, high or low as the case may be, for the consumption of oxygen on the one hand, and organic carbon (by the combustion process) on the other, and closer agreement is observable regarding waters of generally similar character.

6. The permanganate process is capable of giving more valuable information in regard to a water by watching the *progress* and *rate* of the oxidation of organic matter present than by any single determination of the actual amount of oxygen consumed in a given time.

7. For such observation of the progress of oxidation, the two determinations prescribed by Tidy, viz. of oxygen consumed in one hour and three hours respectively, are not sufficient, nor is the latter period of three hours long enough to indicate the general behaviour of the water with the acid permanganate.

As to other chemical determinations, the discrepancies in those of total solids left on evaporation, of the loss on ignition of this solid residue, and even of chlorine, are noted as illustrating the comparative roughness of the methods with which those results were obtained when very small quantities have to be dealt with. A coincidence is often presented between alkaline reaction of a water and the occurrence of nitrites and nitrates in considerable quantity, suggesting a recollection of the conditions under which nitrates are produced on a large scale in the decay of nitrogenous organic matter. Those salts, however, also occur pretty largely in some cases without alkaline reaction, and in other cases there was alkaline reaction and also much nitrogenous matter, but no nitrites nor nitrates. Ammonium nitrates seem to be rare, the basis-constituent being rather mostly non-volatile—no doubt calcium, magnesium, or one of the alkaline metals; this is noticed as in relation to possible reduction to nitrite, and consequent loss of nitrogen in the combustion process. Experiments with tannin showed the utter worthlessness of this group-reagent of Kämmerer for the purpose for which he has advocated its use. The general series of analyses of dissolved gases illustrate how the results are influenced by varying conditions of oxidisability of organic matter present, temperature, extent of exposure to the atmosphere, and interchange with it, in both direction, of gaseous constituents.

With regard to the *microscopic and pathological results*, Prof. Mallet, feeling himself incompetent to properly discuss these, prefers leaving them to speak for themselves, and merely remarks on some of the difficulties of such research, and, at the same time, on its importance and value when rightly conducted.

Passing now to sanitary conclusions and interpretation of results, the Report deals with

Chemical and Biological Results as contrasted with the actual Sanitary History of the Natural Waters Examined

Now on inspection of the tabulated results it appears that *no strongly marked generic difference is presented by the results from any of the processes for estimation of organic matter or its elements between the generally wholesome waters of Class I. and the waters of Class II. medically condemned and fairly assumed as pernicious.* This applies equally to the highest, the lowest, and the average results. No one could, with those figures to guide him, refer a water of unknown origin to one or other of the two classes, on the basis of chemical analysis by any or all of the three methods. Attention is called to the smallness of the amount of organic matter indicated as present in many of the most dangerous waters, giving important evidence against any chemical theory of the production of disease from this source (on the simple assumption that some of the chemical products of decomposition of organic matter are poisonous or noxious in their effect on the human system). Thus in the case of two waters of highly dangerous character, if the whole of the organic carbon and nitrogen present existed as strychnine, it would be necessary to drink about half a gallon of the water at once, in order to swallow an average *medicinal* dose of the alkaloid. It is not easy to believe that the ptomaines, or other chemical products of putrefactive change, can be so much more poisonous than the strongest of recognised poisons. While most of the mischief in drinking water is probably attributable to living organisms, the possibility is noted, that indirectly a large amount of organic matter in water may be more dangerous than a smaller quantity, as furnishing on a greater scale the suitable material and conditions for development of organisms. Whether variations in the mere quantity of organic matter within such limits as occur in water likely to be used for drinking are of much importance in this respect, is a question on which (in the author's opinion) depends largely the utility of all attempts to estimate the quantity of organic matter or its constituents as such.

A much more conspicuous difference between the waters of Classes I. and II. is presented by the results for nitrites and nitrates. These salts are either absent or present in but trifling amount in the wholesome waters of Class I., but almost universally present, and often in large quantity, in the pernicious water of Class II. They are very variable as to presence and amount in the doubtful waters of Class III. This result is worthy of special attention in view of the different opinions which have been expressed (by Wanklyn, Angus Smith, Frankland, Griess, Ekin, Haines, &c.) as to the sanitary conditions of nitrites and nitrates in water.

Among the artificially polluted waters were a number of samples of such general character as to be under the gravest suspicion on sanitary grounds (suspicion corroborated in sundry cases by biological tests), in which, nevertheless, nitrites and nitrates were not found; but these waters had an extraordinarily large amount of organic matter, generally accompanied by very large amounts of ammonia.

Looking at the results for Classes I. and II., and bearing in mind the conclusions reached by Müller, Schloesing, and Muntz, Storer, Warrington, and others, as to the process of nitrification being due to presence of an organised ferment or ferments of bacterial character, "the idea suggests itself whether the noxious character of waters containing largely nitrates and nitrites—themselves presumed to be harmless—and but very little organic matter—which ought to be present, of some sort, to support the 'previous contamination view—*may not be in reality due to the presence of a special nitrifying ferment, itself to be classed among the lower organisms capable of propagating disease.*'"

Two points are noted as requiring caution in regard to the above conclusions: first, the samples may have undergone some chemical change in the interval from their collection to their reaching the analysts (but such changes could hardly have been great); secondly, it was necessary to take *exaggerated instances* of mischief; and the organic impurities present in the waters concerned may not be the same as those which would produce slighter, but, in time, serious ill effects. Slighter forms of disease, really attributable to drinking water, may perhaps be numerous, and possibly of various types, but generally the difficulty will be too great of securing, in view of the many factors concerned, any satisfactory evidence as to their cause.

In regard to determinations of chlorine, the results are in many cases of water from shallow wells, significant enough of contamination by fluid animal excreta. The amount of chlorine in the case of several wells near the sea, shows the need of thought as to the natural source of a water in drawing conclusions from the presence of chlorides. Even where chlorine has come in with organic matter, this impropriety in too hastily deciding, as is sometimes done, that a small quantity indicates vegetable, and a large quantity animal contamination is illustrated by several cases.

Prof. Martin and Dr. Hartwell were asked to independently mark waters as "dangerous" and "suspicious" on the basis of the biological observations. The results, as summarised in a table, prove that these methods will not afford the means of deciding between a wholesome and an unwholesome natural water. Several of the waters believed to be fairly wholesome, and certainly in use on a large scale, are marked "suspicious," while not one of the waters believed to have proved themselves pernicious when used by man, are set down as "dangerous." In many cases the waters which affected rabbits most, contained *very large* amounts of organic matter, so large as to probably invalidate comparison with natural waters or with the much more dilute specimens of prepared water. On the other hand, with three strengths of a solution of organic material, it was not the strongest that produced the most marked effects. The pernicious character of waters containing relatively but very little organic matter, seemed to be proved by several cases; probably supporting the idea that it is not mainly the amount of organic matter, but the presence and nature of low organisms that render drinking-water unwholesome. Much difficulty in interpretation of the biological results seems to have arisen from too great differences of absolute strength in the solutions of organic matter used.

SCIENCE AT KHARKOFF[1]

THE Society of Naturalists at the Kharkoff University is one of those which were founded a few years ago for the advancement of the natural sciences generally, and especially for the study of the natural history of Russia in the provinces that surround University towns, and which have already rendered most valuable services in both these directions. The Kharkoff Society of Naturalists, which numbers 117 members, has already published fifteen volumes of their Transactions (*Trudy*), which contain many valuable papers. Of those in the earlier volumes we will only mention, in geology: The chemical researches of rocks and coal of the Dnieper basin, by A. S. Brio; geological explorations in the government of Kharkoff and in the Coal-measures of the Don, by A. W. Guroff; and in the basins of the Dnieper and Kalmius, by M. F. Klemm; the explorations of the Delta of the Dnieper, and microscopical analyses of the Dnieper granites and of the fossil trees of Southern Russia, by M. E. Krendovsky; the very interesting researches into the formation and shapes of valleys in Southern Russia; on the crystalline rocks of the Dnieper; on black earth, on the Devonian formation of the Sosna and Tim river, and on the structure of the mountains of Taurida, by Prof. S. F. Levakovsky; and on the hydrography of the Northern Donets river, by J. T, Morozoff. The attention of the Kharkoff zoologists was especially attracted during recent years to the obnoxious insects which destroyed the crops, and we find in the *Transactions* of the Society several papers devoted to the subject, such as a complete description of the locusts and other insects inhabiting corn-fields, by P. W. Ivanoff; on the parasites of the locust and the corn-beetle, by P. T. Stepanoff; and on obnoxious insects of the province of Kharkoff, by W. A. Yaroshevsky. The same author has published also nearly complete lists of the Hemiptera, Heteroptera, Diptera, and Lepidoptera of the province of Kharkoff. Among many other contributions in zoology and physiology we notice physiological researches into the structure of the eyes of birds, on the movements of protoplasm, on the air-sacs of birds, and on the mechanism of their breathing; on the movements of *Unio*, and of *Anguis fragilis* (all with numerous plates), by R. F. Byeletzky; on water-acarides, by M. E. Krendovsky; on the *Bythotrephes* of the Sea of Azov, and on a new *Polyphemida*, by N. P. Pengo; on Infusoriæ, Turbellariæ, and Lepidoptera of the province of Kharkoff, by Madame S. M. Pereyaslavtreff; on the development of Nematodes, and on *macrobiotus macronyx*, Duj., by the late G. M. Radkevitch; and on the Araneæ fauna of the province of Kharkoff, by W. W. Reinhard. There are but few papers on botany in the *Transactions*. K. S. Gornitsky contributes a "Conspectus plantarum" of the Walki district of the province of Kharkoff; E. M. Delarme has two contributions on the anatomy of Coniferæ and on the Kirkazon plants; N. F. Kransakoff publishes a list of plants of the neighbourhood of Taganrog, and Novocherkask; and L. W. Reinhard, on the conjugation of zoospores, and on the Characeæ of Middle and Southern Russia. All these papers are profusely illustrated, and sold each separately at very low prices.

The recent (fifteenth) volume of the *Transactions* (*Trudy*), contains the work done by the Society in 1881. M. Stepanoff contributes a paper on the very unsettled question as to the metamorphosis of Bombylides. He has found larvæ of Bombylides in cocoons of *Stauronotus vastator*, Stev.; they support very well temperatures as low as –20° Cels., and can remain at life for more than one year. The opinion of M. Zetterstedt as to the larvæ of Bombylides living also freely, non-parasitically, in the soil, seemed to be confirmed by M. Stepanoff, who found them in the autumn and in the spring in the soil, but they might have already abandoned their former dwellings. M. Stepanoff gives also a complete description (with coloured drawings) of the larvæ of *Systoechus leucophagus*, Mg.—M. Kulchitzky contributes two papers; on the endings and ramifications of the motor nerves of the lower vertebrata (the author doubts that the motor nerves necessarily end in small lamellæ under the sarcolemma, as it was observed by Herr Kühne); and on the origin of the coloured globules of the blood of Mammalia; these last—the author says—arise, not out of protoplasm, but from globules of lymphoid elements which undergo a whole series of very complicated metamorphoses.—M. Yaroshevsky gives a list of Neuroptera and Hymenoptera of the province of Kharkoff. The Neuroptera of the close neighbourhoods of the Kharkoff city number no less than sixty-one species. The Hymenoptera number 400 species, of which no less than 235 are known in the neighbourhood of the Kharkoff city. The same author, in company with M. Sokoloff, contributes a paper on the state of larvæ of the corn-beetle (*Anisoplia*) during the winter. The recent ravages of the corn-beetle in Southern Russia had provoked new researches on this subject, and a controversy had arisen among Russian entomologists, some of them being of the opinion that the larvæ remain during the winter in the upper frozen sheet of the soil, and are in a state of sleep, while others affirmed that they go deeper into the unfrozen soil, and eat there the roots of plants, but die completely if exposed to temperatures below the freezing point. The researches of MM. Yaroshevsky and Sokoloff proved that these larvæ fell asleep when exposed to temperatures below zero, but immediately returned to life as soon as exposed to a warmer temperature. In the frozen soil, whose temperature was one degree below zero, they found plenty of larvæ of *Asilus*, *Elateridæ*, *Heliothis dipsaceus*, and whole nests of ants with their larvæ. All returned to life when warmed. M. Byeletsky contributes a paper on the respiration of the gigantic Salamander, *Cryptobranchus japonicus*, Hœv., one metre long, and weighing four kilograms (several individuals of the same species measure, as is known, four feet, and weigh nine kilograms). Siebold had already observed the very long pauses between the breathings of this Salamander, sometimes lasting for half an hour. M. Byeletsky found that at a temperature of water about 15° Celsius, his Salamander remained without breathing sometimes for an hour and a half. In the air it breathed more often. M. W. Reinhard contributes an elaborate paper on the structure and development of freshwater Bryozoa. After a sketch of our present knowledge of the subject—up to the last works of Messrs. Nitsche, Hatschek, Hyatt, and Allmann—the author describes at length the structure of *Crystatella mucedo*, giving special attention to the development of the statoblasts, and the sexual multiplication of *Alcyonella fungosa*. The paper is accompanied by seven well-engraved plates.

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[1] *Trudy Obshestva Estestvoispytatelei pri Kharkovskom Universitete* (Transactions of the Society of Naturalists at the Kharkoff University), vol. xv. 1882.

THE HIBERNATION OF ALETIA XYLINA, SAY., IN THE UNITED STATES, A SETTLED FACT[1]

I HAVE already shown in previous remarks before the Association that there were various theories held by competent men, both entomologists and planters, as to the hibernation of this *Aletia* (the common Cotton Worm of the South), some believing that it hibernated in the chrysalis state, some that it survived in the moth state, while still others contended that it did not hibernate at all in the United States. I have always contended that the moth survives within the limits of the United States, and in this paper the fact of its hibernation, principally under the shelter of rank wire-grass, is established from observations and experiments made during the past winter and spring. The moth has been taken at Archer, Pla., during every winter month until the early part of March, when it began to disappear, but not until eggs were found deposited. The first brood of worms was found of all sizes during the latter part of the same month on rattoon cotton, while chrysalides and fresh moths were obtained during the early part of April.

The fact thus established has this important practical bearing:

"Whereas upon the theory of animal invasion from some exotic country, there was no incentive to winter or spring work looking to the destruction of the moths, there is now every incentive to such action as will destroy it either by attracting it during mild winter weather by sweets, or by burning the grasses in which it shelters. It should also be a warning to cotton-growers to abandon the slovenly method of cultivation which leaves the old cotton-stalks standing either until the next crop is planted, or long after that event; for many planters have the habit of planting the seed in a furrow between the old row of stalks. The most careful recent researches all tend to confirm the belief that Gossypium is the only plant upon which the worm can feed, so that, in the light of the facts presented, there is all the greater incentive to that mode of culture which will prevent the growth of rattoon cotton, since it is very questionable whether the moth would survive long enough to perpetuate itself upon newly-sown cotton, except for the intervention of rattoon cotton."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following Boards of Electors have been thus constituted:—

Professorship of Anatomy: Professors Flower, P.Z.S., Allen Thomson, Paget, Huxley, A. Newton, Liveing, Dr. Michael Foster, and Mr. J. W. Clark.

Downing Professorship of Medicine: Sir G. Burrows, Bart., Drs. Farre, Lauder Brunton, R. Quain, Professors Paget, Liveing, Humphry, and Mr. Main.

Professorship of Pathology: Professors Burdon Sanderson, Latham, Humphry, Paget, Sir James Paget, Drs. Michael Foster, J. F. Payne, and W. H. Gaskell.

Professorship of Political Economy: Messrs. L. H. Courtney, M.P., A. Marshall, H. S. Foxwell, R. H. Inglis Palgrave, H. Sidgwick, V. H. Stanton, H. J. Roby, and Prof. James Stuart.

Dr. Michael Foster is appointed an additional member of the Special Boards for Medicine, and for Biology and Geology.

Candidates for the Plumian Professorship must send in their names to the Vice-Chancellor on January 6; the election will take place on January 16.

A report has been issued recommending various modifications in the Previous and General Examinations; it, however, contains no indication of any approaching relief from examination in Greek, or of the introduction of French or German into the ordinary curriculum, or of any natural science subject. As the syndicate contains several names of scientific weight, this appears rather surprising.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, tome v. fasc. iii., 1882, contain the concluding part of M. G. D'Hercourt's "Ile de Sardaigne." In this paper the author considers at length the nature and presumed purpose of the massive conical structures known as *nur-aghes*, of which there are upwards of 3000 on the island of Sardinia, generally on, or near the coast. Since Diodorus of Sicily, who ascribed their origin to Dedalus, they have been a puzzle to the learned. The author's remarks on the intelligence of the modern Sardi, notwithstanding that craniometrically they rank among the lowest European races, gave occasion to various discussions at subsequent meetings.—A communication from M. Beauregard in regard to a discovery, made last January by M. Crevaux, of an ancient city of the Incas, at 10 kilom. from Salta, in the Argentine Republic, whose geographical position the latter was engaged in determining at the time.—On the various races inhabiting French Cochin China, by M. G. de Clanbry, who confirms the general view of the moral and social degradation of the Annamites. He draws attention to the slight distinctions perceptible between the men and women of these tribes in voice, length of hair, gait, features, &c., and supplies interesting details in regard to the local flora.—M. Topinard, in presenting to the Society Hölder's craniometer, based on geometric methods, described the craniometric and anthropometric instruments in use from Camper's time to our own.—On the merits of M. Beaumanoir's system of comparing the facial and cranial areas, by M. Corre.—A report by M. Deniker of the result of the official examinations for the Society, of an adult ourang-outang, and a young female chimpanzee, recently brought to Paris. The latter, as in the case observed by Darwin, showed its temper like a petulant child, by pouting, kicking, grinding its teeth, and *shedding tears*.—A paper by M. Corre, on the craniometric relations of certain anthropomorphous apes.—Report by M. de Mortillet of the labours of the Commission appointed to examine and protect the megalithic monuments of France. By the efforts of the Commissioners the remains at Carnac have been secured from further demolition, and the Locmariaquer group, including in the so-called "Roi des Menhirs" the largest known monolith, has passed by purchase under the control of the State.—On the abnormal development of the teeth in a child's jaw, belonging to the Stone Age, and found at Erlen, near Colmar, by Dr. Collignon. The general dental system shows a low racial character, while the large permanent molars had come up before the milk teeth had been shed.—A communication by M. Hovelacque, on certain ethnographic survivals in Marne and Berry. In the former province it is deemed specially unlucky to use the horses of a deceased person till after his funeral; in the latter the hives must have a black ribbon attached to them while the family wears mourning, and to avert evil fortune from the house of a departed master, one of his nearest relatives must proclaim to the bees that their former owner is dead.—M. Chervin, on the census of the French people in 1881. The author shows that the augmentation since 1876 has been only 20 per 1000 in France, while in England it was 145, and in Germany as much as 574 per 1000! Maine and Normandy, notwithstanding their natural productiveness, are conspicuous for the regular diminution of their populations.—On a new form of sclerosis of the cerebral convolutions, by M. Pozzi, with special reference to the cerebral lesions common in insanity.—M. Duval's demand, in the name of a large number of his *confrères*, for the foundation by the Society of an annual Darwinian Conference, was opposed by M. Mortillet in as far as the term Darwinian was concerned, which he proposes to replace by that of *transformist*, arguing that the adoption of the word "Darwinism" is an act of injustice to Lamarck, whose researches entitle him to be regarded as the father of transformism. The question has been referred to the Central Committee.—M. Topinard's explanation of the funereal objects collected in the Philippines by M. Marché's mission.—A discussion on the project for a general manual of ethnographic questions, as drawn up by M. Letourneau for the Society. The plan followed, which is that adopted by the Florentine Society of Anthropology, is criticised at great length by M. Dally, who strongly objects to the phraseology and definitions employed in the questions, and in consequence of his objections M. Letourneau's proposed "Questionnaire" has been referred to a special commission for further consideration.

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[1] Abstract of a paper read at the Montreal meeting of the Am. Ass. Adv. Sc., by Dr. C. V. Riley.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 14.—Note on a discovery, as yet unpublished, by the late Prof. F. M. Balfour, concerning the existence of a Blastopore, and on the origin of the Mesoblast in the embryo of *Peripatus capensis*, by Prof. Moseley, F.R.S., and Adam Sedgwick, M.A., Fellow of Trinity College, Cambridge.

The late Professor Balfour was just before his death engaged on the preparation of a monograph on the anatomy and development of the members of the genus Peripatus, together with an account of all known species. He left a series of notes, completed MSS., and drawings, which will be edited by the above authors, and issued shortly in the *Quarterly Journal of Microscopical Science*. His discoveries, however, concerning the early embryology of *P. capensis* are so remarkable that the above preliminary note has been communicated at once to the Royal Society.

The discovery is shortly as follows:—That a widely open slit-like blastopore is formed in the early oval embryo of Peripatus, which blastopore, occupying the median ventral line, becomes closed in its centre an anterior portion remaining open as the mouth, whilst a posterior portion apparently becomes the anus. The mesoblast is formed from the hypoblast at the lips of the blastotopre, and makes its appearance as a series of paired hollow outgrowths from the cavity of the archenteron. This most primitive method of the the formation of the mesoblastic somites closely similar to that occurring in Amphioxus and other ancestral forms, is of the greatest morphological significance, and it is especially interesting to find that it survives in an entirely unmodified condition in Peripatus, the adult organisation of which proves that it is a representative of an animal stock of the most remote antiquity.

Mr. Sedgwick, by examining some embryos in Prof. Balfour's collection of material as yet uninvestigated, has been able to conform his results, and also by finding earlier stages to verify certain points in the developmental history which rested at the stage at which Prof. Balfour's inquiry ceased, mainly on inference. A discussion took place, in which Prof. Huxley, Prof. Lankester, and Mr. A. Sedgwick took part. The latter pointed out the close resemblance of the early embryo Peripatus with open blastopore to an actinia, the mesoblastic pouches corresponding to intermesenterial cavities, and the blastopore to the mouth, and urged that the discovery tended to confirm Prof. Balfour's published theory as to the origin of the bilateralia from the elongation transversely of a disc-like ancestor, the ventral nerve-cords having been formed by the pulling out into long loops of a circum-oral ring.

Prof. Lankester expressed his opinion that the view that the blastopore represented a structure, which in an ancestral form acted as a mouth, must be abandoned. The blastopore is very probably merely an aperture necessarily formed in the process of production of the hypoblast by invagination, and has never had any special function. Prof. Huxley pointed out the essential difference between the peripheral nerve ring of Hydromedusæ and a true circumoral nerve ring.

Geological Society, December 6.—J. W. Hulke, F.R.S., president, in the chair.—Charles Bird, Enoch Cartwright, Henry Eunson, William Johnstone, Henry Liversidge, Henry George Lyons, Joseph Mawson, Horace W. Monckton, Henry Alexander Miers, John Postlethwaite, and Thomas Viccars, were elected Fellows of the Society.—The following communications were read:—Note on a Wealden fern, *Oleandridium* (*Tæniopteris*) *Beyrichii*, Schenk, new to Britain, by John E. H. Peyton, F.G.S.—On the mechanics of glaciers, more especially with relation to their supposed power of excavation, by the Rev. A. Irving, F.G.S. Generally, the author concluded, from mechanical and physical considerations, that far too much *erosive* power has been attributed by some writers to glaciers, and that it is doubtful if the work of actual *excavation* has been accomplished by them at all. The *differential movement* of glaciers he attributed to three causes: (1) cracking and regelation (Tyndall and Helmholtz); (2) generation of heat by friction within the glacier (Helmholtz); (3) the penetration of the glacier by *luminous solar energy*, the absorption of this by opaque bodies contained in the ice (stones, earth, organic germs, &c.), and the transformation of it in this way into *heat*. To this last he attributed the greater differential movement of the glacier (*a*) by day than by night, (*b*) in summer than in winter.

Physical Society, December 9.—Prof. Clifton, president, in the chair.—New members: Mr. H. E. Harrison, B.Sc., Mr. S T. H. Saunders, M.A.—Prof. G. Forbes read a paper on the velocity of light of different colours. The author concluded from his experiments described to the Society a year ago, that blue rays travel quicker than red rays. M. Cornu had endeavoured to explain this result by peculiarities of the apparatus employed; but this explanation seemed doubtful. It was suggested that the experiments might be repeated with such modifications of the apparatus as would set the question at rest.—Professors Ayrton and Perry read a paper on the resistance of the voltaic arc, or the opposition electromotive forces set up. The electromotive force was measured by a voltmeter connected between the terminals of the lamp. Keeping the width of arc constant the E.M.F. was found to diminish as the current increased. Keeping the current constant, the E.M.F. increased rapidly, at first with an increasing width of arc, and afterwards more slowly. The authors gave a curve representing the change. About 80 volts are required to produce an arc of one-third of an inch. For further increase of arc E.M.F. is therefore proportional to increase of length of arc. The authors also read a paper on the relative intensities of the magnetic field produced by electromagnets when the current, iron core, and length of wire, &c., are constant, but the wire differently distributed. In *a* case the wire was wound uniformly from end to end; in *b* case it was wound from the middle to one end; in *c* case it was wound only at both ends; in *d* case it was wound only at one end. The field was measured along a line running through the axis of the poles beyond the magnet of the above plans; *a* gave the strongest field, except at short distances, when *b* was best.—Professors Ayrton and Perry also exhibited a set of three Faure accumulators in series feeding twenty Swan lamps, each lamp giving over 1 candle power. The electromotive force of each cell was about 2 volts.

Anthropological Institute, December 12.—Mr. M. J. Walhouse, F.R.A.S., in the chair.—Mr. A. L. Lewis exhibited some Neolithic flint implements and flakes found by him at Cape Blanc Nez, near Calais.—A paper by Mr. A. W. Howitt, F.G.S., on the Australian class systems, was read, in which the author discussed and explained the various rules with respect to marriage adopted by several of the native tribes.

SYDNEY

Linnean Society of New South Wales, September 27.—Dr. James C. Cox, F.L.S., &c., in the chair.—The following papers were read:—On a resinous plant from the interior, by K. H. Bennett. Specimens of the gum or resin of this plant, which Mr. Bennett described as *Myoporum platycarpum*, R. Br., were exhibited.—On three new fishes from Queensland, by Charles W. De Vis, B.A. This paper was a description of a new genus of the family Berycidæ, and a species of *Homalogrystes* and *Scolopsis*.—Contribution to a knowledge of the fishes of New Guinea, No. 2, by William Macleay, F.L.S., &c. This is a continuation of a list of the fishes found at Port Moresby by

Mr. Andrew Goldie.—Description of two fishes lately taken in or near Port Jackson, by William Macleay, F.L.S., &c.—On the physical structure and geology of Australia, by the Rev. J. E. Tenison-Woods, F.L.S., &c. This paper dealt at length with all the physical features of the Continent, viz.:—its mountain systems, its inland plains, and the portions intervening between the tableland and the sea, and its river-systems. Secondly the author enumerated the formations which had been recognised in Australia from the fundamental granite up to the recent alluvial. Showing that none of the large groups of rocks which are known in other parts of the world are absent from this continent. References were made to the character of the fossils found, and the soils resulting from the rocks.—On a large cretaceous *Mytilus*, from the Barcoo, by the Rev. J. E. Tenison-Woods, F.G.S., &c. This paper was descriptive of a very large fossil *Mytilus* (*M. ingens.* sp. nov.), which was found in some mesozoic strata in Queensland, of probably Oolitic age. The paper also contained a brief reference to the collections of Mesozoic fossils made in Australia.—Notes on the inflorescence and habits of plants indigenous in the immediate neighbourhood of Sydney, by E. Haviland The author gives an account of his observations on the mode of fertilisation of two species of rutaceous plants common in the neighbourhood of Sydney—*Philotheca australis* and *Boronia pinnata*. In the former species the arrangement of the parts of the flower is such as apparently to specially favour self-fertilisation, but a closer observation shows that this is rendered physiologically impossible by the maturing and discharge of the pollen of each flower before the stigma comes to maturity. A similar phenomenon was observed in *B. pinnata*, and the author suggests that the close opposition of the anthers to the stigma in these species until the pollen is almost ripe, may be designed in order to prevent, to some extent, the access of light and heat, and thus retard the maturing of the stigma until the pollen of its own flower has become discharged.—Note on some seaweeds from Port Jackson and adjacent coast, by E. P. Ramsay, F.L.S.—Mr. W. A. Haswell read a note on some points in the anatomy of the pigeons referred to by Dr. Hans Gadow in a recent paper on the anatomy of Pterocles.—Prof. Stephens exhibited a collection of rocks and fossils illustrating the structure of the Western coal-fields, as explained by Mr. Wilkinson in his map of Wallerawang (1877).

BERLIN

Physical Society, December 1.—Prof. Kirchhoff in the chair.—Dr. Hertz described and exhibited an apparatus he had constructed for demonstration of such weak electric currents as change their direction very often, several thousand times in a second. He called attention to the defects of the electro-dynamometers previously employed for the purpose, and showed that the electric heat-effect could most fitly be used in this case. The new dynamometer consists of an extremely thin horizontally stretched silver wire, the extension of which by heat, produced by the alternating currents, is observed. To this end the wire is, at its middle, wound round a vertical cylinder of steel capable of rotation about its axis, by turning of which the wire is stretched. Each extension of the wire through electric heating turns the cylinder the opposite way to his torsion, and its rotation is observed by means of a mirror and telescope. This dynamometer, as Herr Hertz showed, is only applicable when the currents are weak, and the current reversals are very frequent; that is, precisely in cases where other measuring instruments fail.—Prof. Helmholtz then spoke on his thermodynamic investigations of chemical processes, and their relation to the electromotive force of galvanic batteries, and fully explained his views both on the reversibility of chemical processes and the electromotive forces in batteries; also the experimental verification of these views in a "Calomel battery" composed of zinc, chloride of zinc solution, mercurous chloride, and mercury. The results hitherto obtained in these experiments and considerations, were brought by the author before the Berlin Academy of Sciences in July, and he is at present still engaged with the inquiry.

Physiological Society, December 8.—Prof. du Bois Reymond in the chair.—Prof. Munk read a paper upon two investigations which had been carried out in his laboratory. The first of these was by Mr. M. Preusse, on the Tapetum in the retina of some mammals. It appeared from this chiefly anatomical investigation that a tapetum is always present in the eyes of dogs, horses, and cats; and further that this tapetum is of an irregularly triangular shape and that the greater part of it is situated in the outer and upper quadrant of the retinal surface; so that it is specially impinged upon by the rays that enter the eye from beneath; over the median line and the equator of the retina the tapetum extends only a little, and this inwards and under. The point of entrance of the optic nerve always lies to the inside of the tapetum, which attains its greatest height above the nerve-papilla. In the case of all the animals that were examined, the situation of the tapetum corresponds with the region of most distinct vision. Hence is seen the correctness of Mr. Brücke's view that the tapetum acts as a mirror at a plane behind the cones and rods that are sensitive to light, which sends back a second time through the axes of these cones and rods the rays of light that have already passed through them. This arrangement is of particular service to animals when the illumination is feeble, and it explains how the above-mentioned animals can distinctly see objects lying on the ground even when slightly illuminated, and consequently also at night-time. The second investigation on which Prof. Munk reported was that made by Dr. Karlin on the vaso-motor nerves. It is well known that Prof. Goltz has, from experimental evidence, laid down the doctrine that the blood-vessels have ganglion-cells on or in their walls, which cause the blood-vessels to contract, and which are connected with the central organs by means of vaso-motor nerves which generally dilate but also occasionally contract the blood-vessels. The well-established fact that a section of a nerve, *e.g.* of the sciatic nerve, is followed by an expansion of the vessels in its tract was regarded by Prof. Goltz as the result of the action of the vaso-dilator nerves stimulated by the section, and the after occurring contraction of the vessels as the result of the action of the peripheral vaso-motor centres which in course of time attain the preponderance. This doctrine had received support from Prof. Bernstein's experiments, in which a great dilatation of the vessels was observed to occur, on stimulation of the divided sciatic nerve, in extremities in which contraction of the vessels had been induced by a great lowering of the temperature, and consequently a strong dilatation of the vessels was caused by direct electrical stimulation of the nerve. Dr. Karlin repeated the above experiment, and found its results confirmed only when very strong currents were employed; when, however, weak or moderate stimulation was applied, a contraction instead of a dilatation of the vessels took place. Accordingly the dilatation of the vessels on powerful stimulation is to be regarded as due to a paralysis, and the experimental evidence for the existence of vaso-dilator nerves as inconclusive.

CONTENTS

PAGE
MATHEMATICS IN AMERICA By J. W. L. GLAISHER, F.R.S. 193
QUAIN'S "ANATOMY" 196
LETTERS TO THE EDITOR:—
  Transit of Venus, December 6.—C. J. B. WILLIAMS, F.R.S. . . 197
  The Comet during the Last Month.—C. J. B. WILLIAMS, F.R.S. . 198
  The Heights of Auroras.—T. W. BACKHOUSE 198
  The Aurora and its Spectrum.—J. RAND CAPRON 198
  The Weather.—J. RAND CAPRON 198
  A Common Defect of Lenses.—R. T. GLAZEBROOK 198
  New Deep-Sea Fish from the Mediterranean.—Dr. HENRY HILLYER GIGLIOLI 198
  Electrical Phenomenon.—A. J. K. 199
PHOTOGRAPHING THE CORONA. By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. 199
A WEDGE AND DIAPHRAGM PHOTOMETER (*With Illustration*). . . 201
ON THE OCCURRENCE OF GREAT TIDES SINCE THE COMMENCEMENT OF THE GEOLOGICAL EPOCH. By Prof. ROBERT S. BALL, LL.D., F.R.S. 201
MARS By Rev. T. W. WEBB (*With Illustration*) 203
DESTRUCTION OF LIFE IN INDIA BY POISONOUS SNAKES. By Sir J. FAYRER, F.R.S. 205
SIR J. WHITWORTH'S MECHANICAL PAPERS 208
NOTES 208
OUR ASTRONOMICAL COLUMN:—
  Stellar Parallax 210
  Comet 1882 *b* 210
AMERICAN RESEARCHES ON WATER-ANALYSIS 211
SCIENCE AT KHARKOFF 213
THE HIBERNATION OF ALETIA XYLINA, SAY, IN THE UNITED STATES, A SETTLED FACT By Dr C. V. RILEY 214
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 214
SCIENTIFIC SERIALS 214
SOCIETIES AND ACADEMIES 215

THURSDAY, JANUARY 4, 1883

AUGUSTUS DE MORGAN

Memoir of Augustus de Morgan. By his Wife, Sophia Elizabeth de Morgan. With Selections from his Letters. (London: Longmans, 1882.)

"DE MORGAN is certainly no commonplace man." Whenever we read this sentence in Crabb Robinson's Diary we wonder how so acute an observer could have penned it. No one who has read the shortest article by De Morgan, or who has been in his company for however short a time, but would say that he was the very opposite of commonplace. Indeed the Diarist himself elsewhere records "De Morgan called. He is the only man whose calls, even when interruptions, are always acceptable. He has such luminous qualities, even in his small talk." This last testimony all who knew De Morgan will accept as true. Though nearly twelve years have passed away since the death of this eminent mathematician and logician, no account, so far as we know, has been given of his life and writings, save the appreciative notice by the late Prof. W. Stanley Jevons—whose writings so amply testify to the influence De Morgan's teaching exercised over him—in the present issue of the *Encyclopædia Britannica* (vol. vii. pp. 64-67; 1877), and the interesting sketch by Mr. Ranyard in the *Monthly Notices of the Royal Astronomical Society* for February 9, 1872, vol. xxxii. (erroneously cited as vol. xxii. in Jevons's article). It was, however, well known that a "life" was being drawn up by Mrs. De Morgan. This is the work now before us, in the preface to which the writer says, "my object has been to supply that part of my husband's life, the material for which would not be within the reach of another biographer."

Augustus De Morgan was born in the year 1806 (Mrs. De Morgan is not more explicit, but we learn incidentally from a letter—p. 394—that the exact date was June 27) at Madura, in the Madras Presidency.[1] His father was Lieut.-Col. De Morgan, who had held staff, and other appointments at several stations in India. Other members of his father's family also distinguished themselves in the service of the East India Company. His mother was the granddaughter of James Dodson, F.R.S., author of the *Antilogarithmic Canon* and other mathematical works,[2] a friend also and pupil of De Moivre; from her he appears to have inherited his musical talent ("his delightful flute") and his mathematical power. From his mother too we are told that De Morgan inherited his love of a city life.[3]

When Augustus was seven months old[4] the family came to England, and first settled at Worcester, but subsequently took up their residence at Barnstaple, and other towns in the West of England. After two or three journeys backwards and forwards, the father left Madras in 1816, having been ordered home ill with liver complaint, and died off St. Helena, leaving his widow with four surviving children. De Morgan gives in a half serious, half humorous way the idea "the victim" retained of his early schooling. At four years he learnt "reading and numeration" from his father. He always spoke gratefully of his father, but doubtless what he has written in his paper "On Teaching Arithmetic," had its rise in this early experience, "it is a very common notion that this subject is easy; that is, a child is called stupid who does not receive his first notions of number with facility . . . the subsequent discoveries of the little arithmetician, such as that six and four make thirteen, eight, seven, anything but ten, far from giving visions of the Lucasian or Savilian chairs, are considered tiresome, and are frequently rewarded by charges of stupidity or inattention. . . . Irritated or wearied by this failure, little manifestations of temper often take the place of the gentle tone with which the lesson commenced, by which the child, whose perception of such a change is very acute, is thoroughly cowed and discouraged, and left to believe that the fault was his own, when it really was that of his instructor."

When about nine years of age the Rev. J. L. Fenner was for a short time his teacher; from him the boy "learnt his first—fortunately not his last—notions of Latin and Greek, with some writing, summing, how to mend a pen, and the first four verses of Gray's 'Elegy,' with a wonderful emphasis upon the 'moping owl.' He thinks, too, that 'he pitied the sorrows of a poor old man'; but on this his memory is not so clear."[1] At Taunton, under the Rev. H. Barker, he was taught Latin, Greek, Euclid, Algebra, and a little Hebrew. Of his last teacher, the Rev. J. Parsons, of Redland, near Bristol, De Morgan always spoke with respect. "It was strange that among so many teachers the germ of mathematical ability should have been so long unnoticed. It could not be quite latent or quite unformed in the brain of a boy of fourteen; it can only be supposed that the routine of school teaching smothered and hid it from observation." It was whilst at Taunton that a friend, seeing the boy very busy in making a neat diagram with ruler and compasses, asked him what was to be done. He said he was *drawing mathematics.* "That's not mathematics," said his friend; "come and I will show you what is." The lines and angles were rubbed out, and the future mathematician, greatly surprised by finding that he had missed the aim of Euclid, was soon intent on the first demonstration he ever knew the meaning of. De Morgan himself writes of this time, "On referring to my own experience I find that I have always had the image of '*length without breadth.*' I remember when I first opened Euclid, at thirteen years of age, I am sure I had no bias to admit any thing which should make mathematics 'exist as a science': for I should have been better pleased if it had not existed at all, science or no science. I thought I had studies enough; and Walkingame, who I understood was a cousin of Euclid, had given me no prejudice in favour of the family. But in the first glance at the book, when I came to 'a line is length without breadth,' I felt that I had gained expression for an idea which I distinctly possessed by image, but could not have put into words. And so, in a small way, I found that geometry *did* exist as a science."[1]

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[1] De Morgan was proud of his birth in the sacred city of Madura, and at one time longed to visit his native country . . . his doing so when young was prevented by his defect of sight. From his birth both eyes were affected with the "sore eye" of India, and the left was saved (pp 22, 5)

[2] He was also mathematical master at Christ's Hospital, and in connection with this "blot on the escutcheon," De Morgan writes that when quite a boy he asked one of his aunts "who James Dodson was," and received for answer, "we never cry stinking fish." He had to wait a few years to find out that his great-grandfather was the only one of his ancestors whose name would be held deserving of record

[3] In the "Budget of Paradoxes" (p 82) De Morgan applies to himself the lines:—

> "Ne'er out of town; 'tis such a horrid life;
> But duly sends his family and wife."

The memoir gives frequent illustrations of his dislike for even a short stay in the country (pp 79, 94, 234).

[4] In the *Monthly Notices* "three months old" would appear to be incorrect.

[1] "Recollections by Mr. De Morgan," Appendix to Crabb Robinson's Diary, vol. iii. p. 540

Before we leave these records of his school-days, we will cite some further remarks on the modes of instruction then in vogue—which, by his books, he more than any other writer helped to improve. When a boy arrives at school, "he is taught to *say* the table of numeration, and then proceeds through a number of rules . . . which, if he understand, it is well, but if not, nobody cares. . . . As to the reasons for the rules, the pupil cannot trouble his head (to use a common term for that much-avoided operation, thinking) about them, not knowing whether there are any at all, or whether the rules themselves came from the moon, or are a constituent part of that wisdom of our ancestors about which he sometimes hears. Should there be any natural defect in his mind, owing to which he finds it difficult to produce a correct result, knowing neither what he is to do, nor how to do it, there are several approved methods of proceeding. The best of these, unfortunately now somewhat exploded, is a flogging; which works on a principle recommended by physicians, of curing a disorder in a part which cannot be got at, by producing one in another which can. Next to this, comes the method of keeping the patient from all recreation until he has done what is required of him, it being considered the same thing in the end, whether he cannot work for want of means, or will not from want of application. It has been suggested to teach the principles involved in the rules, and thus to render the pupil their master instead of their slave; but to this plan, independently of its being an innovation, there are grave objections."[2]

At the age of sixteen years and a half he entered at Trinity College, Cambridge, and in 1825 gained a Trinity Scholarship. Devoting much of his time to music and to a rather wide range of reading (he had always "an insatiable appetite for novel-reading . . . let it be good or bad in a literary point of view, almost any work of fiction was welcome, provided it had plenty of incident and dialogue, and was not over-sentimental"[3]) he failed to attain the highest place in the Tripos, but came out Fourth Wrangler in 1827. Mrs. De Morgan notes that this failure, in a possibly fallacious test, was his own early, but unintentional, protest against competitive examinations, for which he felt excessive disapprobation even before his experience as a teacher showed him not only their mischievous effect upon mind and health, but their insufficiency to determine the real worth of a candidate for Honours (pp. 18, 56, 169). In connection with this subject we may mention that he had a great objection to marks in looking over examination papers. He said he could judge of the merits of the competitor from the whole work, but he could not reckon it up by marks, and he always refused to examine in this way.

Having conscientious scruples about the doctrines of the Established Church, he was prevented from proceeding to his M.A. degree and from sitting for a Fellowship, to which he would doubtless have been elected. "A strong repugnance to any sectarian restraints upon the freedom of opinion was one of De Morgan's characteristics throughout life." A further career at the University being thus closed against him, and having abandoned the study of medicine, he turned his thoughts to law, and entered at Lincoln's Inn. The establishment in 1828 of the London University—now University College—however gave him the opportunity of leaving the study of the Law, "which he did not like," for the teaching and pursuit of science. At the age of twenty-two, though much younger than any of the other thirty-one candidates[1] for the post, he was unanimously elected to the Chair of Mathematics. From this time, with the exception of an interval of five years,[2] he devoted himself with the greatest assiduity to the duties of the post until his final resignation in 1866.

It has been frequently remarked that De Morgan was unrivalled as a teacher of mathematics, and certainly no teaching in our University experience ever approached his in the faintest degree. Mr. Sedley Taylor writes:—

> De Morgan regularly delivered four courses of lectures, each of three hours a week, and lasting throughout the whole academical year. He thus lectured two hours every day to his College classes, besides giving a course addressed to schoolmasters in the evening, during a portion of the year . . . De Morgan was far from thinking the duties of his chair adequately performed by lecturing only. At the close of every lecture in each course he gave out a number of problems and examples illustrative of the subject which was then engaging the attention of the class. His students were expected to bring these to him worked out. He then looked them over, and returned them revised before the next lecture. Each example, if rightly done, was carefully marked with a tick, or if a mere inaccuracy occurred in the working it was crossed out, and the proper correction inserted. If, however, a mistake of *principle* was committed, the words 'show me' appeared on the exercise.[3] The student so summoned was expected to present himself on the platform at the close of the lecture, when De Morgan would carefully go over the point with him privately, and endeavour to clear up whatever difficulty he experienced. The amount of labour thus involved was very considerable, as the number of students in attendance frequently exceeded one hundred. . . . The claims which University or College examinations might be supposed to have on the studies of his pupils were never allowed to influence his programme in the slightest degree. He laboured to form sound scientific mathematicians, and, if he succeeded in this, cared little whether his pupils could reproduce more or less of their knowledge on paper in a given time . . . all *cram* he held in the most sovereign contempt. I remember, during the last week of his course which preceded an annual College examination, his abruptly addressing his class as follows: 'I notice that many of you have left off working my examples this week. I know perfectly well what you are doing; YOU ARE CRAMMING FOR THE EXAMINATION. But I will set you such a paper as shall make ALL YOUR CRAM of no use.' . . . De

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[1] "On Infinity; and on the Sign of Equality." *Camb. Phil. Soc Trans.*, vol xi Part 1.

[2] From a paper "On Mathematical Instruction," which, with four other papers by De Morgan, is reprinted (from the *Quarterly Journal of Education*) in the *Schoolmaster*, vol. ii., 1836. The five papers amply repay perusal even at the present date.

[3] In reference to this period of his life, he writes (1869, p. 393), "I read an enormous deal of fiction—all I could get hold of—so my amusement was not all philosophical."

[1] In a letter to Sir J. Herschel, August 9, 1862 (p. 312), De Morgan says, "I was picked out of fifty candidates."

[2] In consequence of a disagreement with the Council he resigned his Professorship, July 24, 1831. On the death of his successor, in October, 1836, he was requested to resume his office, and did so.

[3] The exercises were placed in a case of pigeon-holes hung on the wall near the entrance to the Mathematical Theatre.

Morgan's exposition combined excellences of the most varied kinds. It was clear, vivid, and succinct—rich too with abundance of illustration always at the command of enormously wide reading and an astonishingly retentive memory. A voice of sonorous sweetness, a grand forehead, and a profile of classic beauty, intensified the impression of commanding power which an almost equally complete mastery over mathematical truth, and over the forms of language in which he so attractively arrayed it, could not fail to make upon his auditors" (pp. 99, 100).

His pupils' affection, the memoir tells us, was not gained by any laxity of discipline, for he was strict, especially as to quietness and punctuality.[1]

Such arduous labours as these would amply suffice for the generality of teachers, but the remainder of his time was occupied with other work hardly less exhausting than these. In May, 1828, he was elected a Fellow of the Astronomical Society, and in February, 1830, he took his place on the Council. In 1831 he was elected Honorary Secretary; in which position he entered with zeal, we are told, into every question brought before the Society, and his place was not a sinecure. "It is not easy to say how much of the usefulness and prosperity of the Society . . . was due to his incessant energy and effort, and to his steady judgment at difficult junctures." Though his connection with the Society lasted for some thirty years, he would never undertake the office of President. "I will vote for and tolerate no President but a practical astronomer. . . . The President must be a man of brass—a micrometer-monger, a telescope-twiddler, a star-stringer, a planet-poker, and a nebula-nabber."[2] He was frequently employed as a consulting actuary,[3] and bestowed also much time and labour upon the subject of the decimal coinage.[4]

Passing from De Morgan's public labours we hurriedly glance at him as a writer, and here we cannot do better than quote Prof. Jevons: "From the above enumeration" of his mathematical and logical writings, "it will be apparent that the extent of De Morgan's literary and scientific labours was altogether extraordinary; nor was quality sacrificed to quantity. On the contrary every publication was finished with extreme care and accuracy, and no writer can be more safely trusted in everything which he wrote. It is possible that his continual efforts to attain completeness and absolute correctness injured his literary style, which is wanting in grace; but the estimation in which his books are held is shown by the fact that they are steadily rising in market price. Apart from his conspicuous position as a logical and mathematical discoverer, we may conclude that hardly any man of science in recent times has had a more extensive, though it may often be an unfelt influence, upon the progress of exact and sound knowledge."

His love of books was intense:[1] "the most worthless book of a bygone day is a record worthy of preservation." Evidences of his minute acquaintance with all sorts of out-of-the-way works present themselves in almost all his writings, but are especially conspicuous in that wondrous repertory of wit and wisdom, the "Budget of Paradoxes." De Morgan's peculiar dislike of conventional titles, "which are not what they seem to be," led him to decline the honorary degree of LL.D. of the University of Edinburgh, and accounts for his not allowing his name to be put up for the F.R.S.[2] "Whether *I* could have been a Fellow, I do not know; as the gentleman said, when asked whether he could play the violin, 'I never tried.'" In fact, as he writes in the "Budget," he was a man who could not *groove.*

The last occurrence connected with science which gave him pleasure was the foundation of the London Mathematical Society.[3] The idea of having such a society occurred to his son George and Mr. A. C. Ranyard, and on their mentioning the matter to Prof. De Morgan, he at once gave in his adhesion to their proposition, and with the countenance thus extended by himself and other leading mathematicians who were got together in reply to a circular issued by the two founders, the Society started into existence. Prof. De Morgan was the first president, and delivered at the first public meeting (January 16, 1865) an interesting and characteristic address. He continued to take a warm interest in the meetings (being a vice-president for the last time in the session 1869-70) until November 26, 1868, after which date severe illness prevented his further attendance.[4] The end came on March 18, 1871, "just after midnight he breathed his last."[5]

In the Vacation of 1837 De Morgan married Sophia Elizabeth, daughter of William Frend. This gentleman was a member of the old Mathematical, and subsequently of the Astronomical Society, had been Second Wrangler, and a Fellow of Jesus College, Cambridge. He sacrificed good prospects as a clergyman to his conscientious scruples about the doctrines of the Established Church, and was at this time Actuary of the Rock Life

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[1] A student, who joined the class in 1859, has put at our disposal some notes he wrote during the session 1859-60: "The class begins at 9 o'clock, but however early we go the Professor is sure to be there Only once or twice have I been early enough to see him coming. He has a large head, bald at the top, and with a tremendous halo of hair round the crown. He wears a black cloth suit and a parson's white neck-cloth. His coat is a swallow-tail, and his trousers, with fob pockets, scarcely reach his boots, of which the laces are often too long. As he shuffles along he seems to be counting the flagstones or rails, urged by a sort of centrifugal force to keep the outside kerb, as Dr Johnson used to do. In the lecture-room when the bell has rung, he always goes through the same routine at commencing. First of all he takes out a large red silk pocket handkerchief, with which he wipes his spectacles, he then readjusts them with the bridge upside down, and though he has only one eye he can see as keenly as another man with both. He then turns back the cuffs of his sleeves, and, after passing his fingers through his hair, takes his compasses in hand and looks round the room at his class. He has been talking all the time, and by this is fairly launched on his subject. . . . He often indulges in jokes with manifest gusto. The other morning he was illustrating a point, when he said, 'This reminds me of an anecdote told me once by my old Cambridge tutor, Prof. Peacock. He had been for some time striving to instil into the mind of a rather obtuse student the difference between $4x$ and x^4. At last he timidly ventured to remark, "'I think, now, Mr. A., you clearly see the difference.'" "'Yes, I think I do, but between us don't you think, Professor, it is a needless refinement?"' I think the part of his lectures I have most enjoyed has been his treatment of the Theory of Probabilities. In this he seemed to revel." Then follow remarks similar to Mr. Taylor's, and he concludes: "No Professor takes more pains with his class, and all through the session he deposits, in the Library, Tracts written by himself on the particular branch then in hand." We have similar testimony from other quarters.

[2] A list of the offices he held is given (p. 270).

[3] He was never connected with any office, but his advice was sought by professionals whenever there arose a "nodus vindice dignus."

[4] A full account of his work in this direction occupies pp. 235-255 of the *Memoir.*

[1] He loved to surround himself, as far as his means allowed, with curious and rare books He revelled in all the mysteries of watermarks, title-pages, colophons, catch-words, and the like; yet he treated bibliography as an important science.

[2] Why he did not care to "shine in the dignity of F.R.S"—*See* "Budget of Paradoxes," p. 18.

[3] There is a new Mathematical Society, and I am, at this present writing, its first president. We are very high in the newest developments, and bid fair to take a place among the scientific establishments." Then in contrast with the old Mathematical Society, "But not a drop of liquor is seen at our meetings, except a decanter of water: all our heavy is a fermentation of symbols; and we do not draw it mild,"—"Budget of Paradoxes," p. 236.

[4] In the recent Presidential Address it was announced that a "De Morgan Memorial Medal," of the value of £10 would be awarded triennially by the Society. The first award to be made in November, 1884.

[5] *Monthly Notices,* "at one o'clock in the afternoon."

Assurance Office. The marriage was a most happy one, and surrounded by a family of seven children, of whom three at least died before their father, De Morgan sought his happiness, as we have endeavoured to show, in his home, amongst his books, and in the earnest discharge of his professorial and other duties.

The Memoir is charmingly written, and abounds in graphic details, which bring clearly before the reader the picture of a simple, manly character that was unique in its idiosyncrasy. A prominent object in its production has been to tell the story of the Professor's connection with University College, and of the events which led to his leaving it. "After the lapse of sixteen years I trust that the narrative will provoke no revival of the somewhat acrimonious controversy which ensued."

Another feature of the work is the selection from De Morgan's extensive correspondence with contemporary scientific men; these letters are full of interest, and abound in utterances characteristic of the writer. Always effective and to the point, they are often very humorous: the humour, indeed, sometimes borders upon trifling.

Instead of thinking that Mrs. De Morgan has exceeded due limits in her selection, we would have welcomed a far larger number of specimens. On p. 333 De Morgan states that he had corresponded for thirty years with Sir W. Rowan Hamilton, but no specimens of the correspondence are given. In one or two cases Mrs. De Morgan has deviated from the general rule she laid down for herself, and to this deviation we are indebted for some very interesting letters from the late Sir Frederick Pollock, which enlighten one as to what was required to be read for the Senior Wranglership at the beginning of the present century. Is it too much to hope that another volume may be issued containing a further selection from the correspondence, and also a few of the more valuable of the early papers, such as those which appeared in the *Journal of Education*, and in the *Companion to the British Almanac?* We venture to give the following extract from a letter we received under date May 15, 1869, as an ordinary specimen of his writing to one who had no special claim upon the writer:—". . . I should decidedly object to the reference made to Barrow on the last leaf. 'Dr. Barrow with an orthodox dislike to give unnecessary credit to a Moslem author has misled. . . .' When was there an orthodox dislike to Mahometans being discoverers in science? And what possible reason is there for imputing any such feeling to Barrow, a man of most unimpeachable fairness, except in this, that when he had a congregation by the ears he would hold on for three hours until they prevailed on the organist to 'blow him down.' He had lived among the Turks at Smyrna and Constantinople, and was certainly not ill-inclined to a Mahometan, as such. But this much is enough: the imputation is quite new, or nearly so, and should not appear without proof in the *obiter dictum* of an historical writer who obviously makes it a theory to explain something he has found. . . . P.S.—I think that for one orthodox man who might be supposed likely to rob a Mahometan of geometry, I could find three who would have been more likely to toss it back again with the remark that such infidel stuff was only fit for Mahound and his slaves."

A list of writings is appended. In this we notice the following slips:—"Elements of Arithmetic," the dates should be "1st edition, 1830; 3rd, 1835"; on p. 403, in (18) for "No. 3" read § 3 of (15) supra"; p. 404, in (18) dele "1," and in (6) for "Trigonometry" read "Geometry"; p. 405, (5) should, of course, be "$P\,dx + Q\,dy + R\,dz$"; p. 406, in 1849, insert "Remarks" after "Supplementary"; in (1), (2), we think the dates are inaccurate; p. 407, read "Alfonsine."

We remark also that no account is taken of communications to the Mathematical Society. These were ten in number, not counting the Opening Address, which forms the first number of the Society's *Proceedings*. The only papers printed are "A Proof that every Function has a Root" (No. vi., a mere notelet); "Remark on paper by Mr. Woolhouse 'on General Numerical Solution'" (No. xiv.); and "On the Conic Octagram" (No. x. pp. 26-29). In this last paper occurs the characteristic note: "This presentation of the second hexagon was actually suggested to me by observing that *Bloise Pascal* has two hexagrams, and the jocose inference that there ought to be two hexagons in the theorem (given in the paper). My own names are both octagrams; but though I bow before the coincidence, I have no suggestion to acknowledge." There is a fairly full "Index of Names, &c.," but we do not grasp the principle upon which it is drawn up, as some names are inserted and others left out. We failed at first to identify "Prof. John Adams" with "Neptune" Adams. There are also the following corrections to be be made:—Read "J. Baldwin Brown," "Arthur Cayley" (both here and in the "Budget of Paradoxes" the famous mathematician is called "George"), Hanssen (for Haussen), Encke (for Hencke), Royal Society (insert the most important reference to p. 172), Sedgwick, Rev. C. Simeon (not J.), insert John Taylor, p. 122, and dele "p. 124" under Sedley Taylor. On p. 306 we presume x should be $\dot{x}$, and on p. 286, for 1866 read 1865; the present writer succeeded George De Morgan as teacher in the session 1865-66.

R. TUCKER

FISHES OF SWITZERLAND

Faune des Vertébrés de la Suisse. Par Victor Fatio, Dr. Phil. Vol. IV. Histoire naturelle des Poissons. 1re partie I. Anarthropterygiens. II. Physostomes-Cyprinidés. 8°. pp. xiv. et 786, avec 5 planches. (Génève et Bale: H. Georg, 1882.)

AFTER an interval of nearly ten years Dr. Fatio has issued another volume of the series of excellent monographs, in which he gives the results of his researches into the vertebrate fauna of Switzerland. The first volume, published in the year 1869, contained the Mammals; the third (1872) the Reptiles and Batrachians; the second, which will be devoted to Ornithology, being still in course of preparation. The one now published, which is the fourth of the series, treats of a part of the Fishes, which class will be concluded in the fifth.

No one who studies this volume will be surprised at the long lapse of time which intervened between its appearance and that of the preceding. The author had not the advantage of being assisted in his work by collections already formed and available for the purpose, but had to collect the materials himself; a labour which, even in a small country like Switzerland, takes years to accomplish; especially as, for comparison's sake, he extended his researches to the fauna of the neighbouring countries. His descriptions are well elaborated and compiled from numerous observations; they include all the variations of age, sex, season, locality, &c., and particular notice is taken of those modifications, by which Swiss examples seem to be distinguished from those of Germany, France, Italy, &c. Thus, this work rises far above the level of a local publication, and is of as great a value to the student of European freshwater fishes, as to the Swiss naturalist.

The present volume treats of the Acanthopterygians and Cyprinoids only, 5 species of the former and 21 of the latter being admitted as permanent inhabitants of the country. Besides, the author distinguishes 3 sub-species, many varieties, and 3 hybrids; he also refers in shorter chapters to 13 other species and 6 hybrids, which are extra-limital, or may sooner or later be found straying into Switzerland.[1] This number of the freshwater fishes of Switzerland must appear small, when we consider that it comprises representatives of four of the principal river-systems of Europe, viz. the Rhine, Rhone, Po, and Danube; and there is no doubt that this comparative poverty is due to the altitude of the country, freshwater Acanthopterygians and Cyprinoids being generally more developed in the less rapid waters of warmer low-lying countries. The Rhine contributes the majority; 20 out of the 24 species[2] which inhabit the middle and lower sections of the river, ascending beyond the Falls of Schaffhausen. The Rhone is inhabited by 24 species, but, singularly, of these 11 only have been able to establish themselves above the Perte du Rhone, although the others freely enter the Saone or penetrate even into the upper Doubs, a river not included in Swiss territory. Of the 23 species found in the lower part of the Po, 15 reach the Swiss frontier; and this southern portion of the fauna is in its character so distinct from the northern, that 9 only of these 15 species are identical with Rhine fishes. Finally the fish-fauna of the Danube, which is stated to consist of 30 species, is represented in Switzerland by 3 only, the great altitude of the river Inn proving a most effectual barrier to the dispersal of the remainder. Moreover, these three species are common Central European types, and not peculiar to the Danube.

The author has taken great pains to ascertain the extreme limits of altitude, to which the several species can attain in the Alps. Two only go beyond the height of 2000 metres, viz. the minnow and miller's thumb, which are still found at respectively 2400 and 2200 m. The perch, the next in order, reaches an altitude of 2000 m., all the remainder living at, or below, 800 m. However, several have been successfully imported to altitudes varying between 1000 and 1700 m., thus the carp, tench, rudd, roach, and chub.

Of the fishes described in this volume, we wish to draw particular attention to two which, belonging to marine genera, and evidently being of marine origin, have acclimatised themselves in the fresh waters of Southern Europe, and penetrated into, or close to, the confines of Switzerland, viz. a goby (*Gobius fluviatilis* or *martensii*), which has ascended the Po, and a blenny (*Blennius cagnata*), which occurs in abundance in Lago Maggiore as well as in the lake of Bourget in Savoy.

Hybrids are comparatively scarce in Switzerland. The author justly accounts for their scarcity by the physical peculiarities of his country; snow-fed, rapid rivers are less adapted for their production, than the slower and warmer waters of low countries, where a greater variety of species and a larger number of individuals are mixed together, sometimes within very narrow limits.

The volume is illustrated by five well executed plates, three of which are devoted to osteological, dental, and dermal details.

The author of a thoroughly original work like the present, cannot fail to differ from his predecessors in questions of specific distinctions and numerous other points of detail, but it is our duty to testify to the fair and calm spirit, in which such questions are discussed and treated by him; and we hope that, before many years, we shall have the pleasure of announcing to our readers the completion of so valuable a work as Dr. Fatio's Ichthyology of Switzerland.

OUR BOOK SHELF

China. By Robert K. Douglas. (Society for Promoting Christian Knowledge, 1882.)

It may be said at once respecting this book that it is without exception the very best elementary work on China with which we are acquainted in any European language. The author has resided for many years in China, and is in the forefront of the Chinese scholarship of our time; his work is, therefore, not only accurate, but it places the reader abreast of the latest researches. One of the most remarkable of these is fully explained at pp. 359-60. The *Yih King*, or Book of Changes, is the work for which the greatest antiquity is claimed by the Chinese. Some writers have placed it as far back as between 300 and 400 B.C. However this may be, the key to its interpretation has been entirely lost, although the best native scholars of all ages, including Confucius himself, have attempted to explain it. M. Terrien de la Couperie (assisted, we believe, by Prof. Douglas himself, though this fact is not mentioned), has succeeded within the last few years in showing that "instead of being a mysterious depository of deep divinatory lore, it turns out to be a collection of syllabaries such as are common in Accadian literature interspersed with chapters of astrological formulæ, ephemerides, and others dealing with ethnological facts relating to the Aboriginal tribes of the country; but all taking the form of vocabularies, and therefore as impossible to be translated in the sense in which every commentator, from Confucius downwards, has attempted to translate them as 'Johnson's Dictionary' would be." Although we possess innumerable volumes on subjects connected with China, we have not until now a thoroughly trustworthy book covering the whole ground in a simple elementary manner. Some volumes recently published for popular reading on the countries of the East exhibit such lamentable ignorance, that we can only "gasp and stare" at their contents. Notwithstanding his own intimate knowledge of the subject, Prof. Douglas has consulted almost all that we have in our literature relating in any way to China, from Davis's Chinese poetry and Oppert's Susian texts, down to recent numbers of the English journals published in China. The two last chapters—that on "Language" and "Literature"—are models of clear and simple exposition of complicated subjects. Another excellence of the book is what we may call its perspective. The writer does not thrust any particular branch of his subject into undue prominence, to the detriment of the rest. The first chapter gives a brief sketch of Chinese history, the second of the system of administration; various chapters are then devoted to popular customs, to education, medicine, music, dress and food, architecture, honours, names, superstitions, religions, &c. There is also an excellent map. To the general reader who desires some accurate information respecting a country which is coming nearer to us every day, or to the student who wants a *vade mecum*, no better volume can be recommended.

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[1] The second part of the Ichthyology is estimated to contain about 21 species.

[2] These and the following numbers refer to the Acanthopterygians and Cyprinoids only.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Occurrence of Great Tides since the Commencement of the Geological Epoch

MR. BALL says very truly that the fundamental question is, what traces of great tides ought we to expect to find if those great tides had really existed? Mr. Darwin says, coarse-grained rocks, and different forms of vegetation calculated to resist the action of the accompanying great winds. Mr. Ball, in reply, remarks that high tides in the Avon are accompanied by fine sediment. He thinks with others that the high tides would have produced a vastly greater amount of sediment than is being formed at present. I quite agree with Mr. Ball about the fine sediment, but I am not at all clear that high tides mean great marine denudation. By far the largest portion of the work done by the sea as a denuding agent is due not to the wearing action of currents, or to the pounding of materials on a beach at low water, but to the direct action of the sea on the cliffs. This force is estimated as about a ton per square foot on the average in winter, on the west coast of Great Britain. This undermines the cliff at the sea level, and then the top part falls partly by its own weight, but still more through the effect of the air compressed in the caves and cracks, which by its elasticity spread the blow over a very large surface through the crack and joints of the rock. Now to undermine cliffs with a given force of wind and wave, it seems clear that the maximum effect would be produced where the tides are very small, for there the force is constantly applied at the same spot. With a rise and fall of 100 feet, each portion of the cliff would be subjected to the force of the waves for so short a time that in all probability caves would never be formed at *all*, and the height of the tide would be an actual protection to the land. As a matter of fact, those places where the tides are highest show, as far as I know, no signs of excessive denudation. I spent two days last year on the Bay of Fundy, where the tides are higher than anywhere in the world, and I was very much struck with the absence of any evidence of great denudation due to the tide. The cliffs at the Joggins are about high-water mark, with a long beach which slopes very gradually. The force of the waves, such as they are, is spent in hammering this beach and grinding it into fine sand and mud; the mud is carried about in suspension by the tide, and the sand is shifted about, but the denuding effect is exceedingly small. The consequence is that the cliffs are pounded by the waves for such a short time each tide that they suffer mainly from atmospheric denudation, the sea doing little more than keeping their base clear, and in many places not even doing that.

Similar phenomena are presented by the highest tides in Great Britain—those on the Severn. Here at Clifton on the Avon the tide rises 30 feet; there are no waves; the banks are covered with a thick coating of mud and denudation is nil. At Aust Cliff, again, on the Severn, where the soft red marls are peculiarly liable to erosion, the height of the tide is again a protection. The cliff is about high-water mark, and the force of the waves is expended on the beach. The case is the same at Watchet, and a good many other places on the Severn. I do not know of any part of the Severn remarkable for excessive denudation, owing to the high tides. There is a strong resemblance between the Bay of Fundy and the Severn: there are the cliffs at high-water mark, the same long beaches, the same shifting sands and mud in suspension; similar causes have produced similar results. In narrow inlets like the Bay of Fundy and the estuary of the Severn, these high tides mean rapid currents and small waves; but along shores freely exposed to the ocean, the highest tides might be accompanied by very feeble currents. But if, as Mr. Darwin says, the high tides were accompanied by trade winds about 3¾ times as strong as the present ones, the battering power of the waves and the strength of the currents would be very greatly increased, and plains of marine denudation, it might be supposed, would be very rapidly formed. What would be likely to happen if such winds and waves began now to act on our shores? Should we have reason to expect that England would in a comparatively short time disappear beneath the waves? A very rapid destruction of our present cliffs would undoubtedly begin, though, as I pointed out before, this would be attributable mainly to the wind, and not to the tide; the cliffs would be driven back to about the ordinary high water mark, leaving a long shelving beach extending to a few feet below the low water mark. The cliffs would then for far the greater portion of each tide be entirely free from marine denudation, and their rate of wasting would depend on the power of the sea to tear up the long solid sloping beach, and restore comparatively deep water at the base of the cliffs. But this process is an exceedingly slow one, because there can be no undermining or assistance from compressed air, and I should anticipate that marine denudation would then be actually slower than it is at present. There would, of course, be abundance of very fine sediment formed during the first wearing back of the cliffs by the grinding of the materials between tide marks; but when once the cliffs had reached the high water line, the amount of sediment would depend chiefly on the amount of atmospheric denudation, supposing that the sea kept the base of the cliffs clear. We should, in fact, have a repetition of the phenomena presented to us now by the Bay of Fundy and the Severn. Thus far, then, it seems to me that no argument can be drawn from the fineness of the early sediments against the existence of high tides in the Geologic period; nor, on the other hand, does the quantity of sediment seem to me a strong argument in favour of it. But Mr Darwin's argument that the vegetation of the Carboniferous period could not possibly have held out against the violent winds which necessarily accompanied these high tides seems to me unanswerable. One has only to reflect on the effect produced by our present winds to feel convinced that if the winds and tides went together they were certainly Pre-Carboniferous, and almost equally certainly Pre-Devonian. J. G. GRENFELL

Clifton College, December 29, 1882

Sir George Airy on the Forth Bridge

As Sir George Airy's last letter may, like his first, provoke replies from distinguished American and continental engineers, it may save your correspondents' time and your own valuable space if I add a few final words in explanation.

1. Sir George says:—"The danger of buckling in a horizontal direction with a length of 340 feet, remains undiminished unless it is counteracted by bracing unknown to me." Now Sir George evidently has forgotten that some time ago he was furnished with photographs of a large model of the bridge taken with the view of showing the said bracing, and that his attention was specially directed to the point.

2. Sir George thinks "it desirable that attention should be called to the magnitude of the forces concerned," and speaks of a wind-pressure of 75 tons, and an end-pressure of 600 tons. Now he clearly has forgotten that before he wrote his first letter a "stress diagram" was sent to him, on which it was noted that the wind-pressure provided for was 2207 tons on each span, and that the estimated end-pressure on the strut referred to was 2380 tons.

3. Sir George holds the engraver responsible for some of the alarmist statements in his first letter. I must remark, therefore, that it was pointed out that the bridge would have been perfectly safe had the details of the design been as he assumed. For evidence that a 340 feet tubular strut of 12 feet diameter would not fail in the manner stated by him, he was referred to Hodgkinson's experiments as published in the *Philosophical Transactions*, and Clark's work on the Britannia bridge; and further, he was lent the *Transactions* of the American Society of Civil Engineers for last year, containing the most recent experiments on long wrought iron columns. Any or all of these documents would have shown him that the Forth bridge

struts, even if arranged as he first conceived them to be, could not have failed by flexure during the wildest hurricane.

4. Sir George finds fault with the connection of the brackets, and "can hardly imagine that trains could be run through at speed." I should have been pleased to have explained the connection to Sir George, but he has not sought to know anything about the details of the bridge, and, I am sure, would be much puzzled to give your readers even the vaguest possible description of the connection, which he nevertheless stigmatises as "not very perfect."

In conclusion, as Sir George has not done so himself, I would warn any young student who may have read the investigation contained in the appendix to the first letter, that the methods therein proposed would lead to an over-estimate of the strength of struts of ordinary proportions by from 200 to 300 per cent. This warning is the more necessary, as the general tenour of Sir George Airy's letter might make a student imagine that he erred, if anything, in the direction of excess of caution, whereas the application of the principles laid down by him would, in the case of the Forth bridge, result in the compression members being made only one-third of the strength considered expedient by Mr. Fowler and myself. B. Baker

2, Queen Square Place, Westminster, S.W.

P.S.—It may interest some of your readers to know that the maximum force recorded during recent storms by our wind pressure plates at the Forth has been 20 lbs. per square foot, upon the small and light plate having an area of 2 square feet, and 12½ lbs. upon the large and heavy one, with an area of 300 square feet. The same ratio holds good down to pressures of 2 lbs. per square foot, and it appears pretty certain that the higher blasts are of such momentary duration and of such unequal distribution, that even a small sized railway bridge could never experience ordinary anemometer pressures. Other reasons for a reduced pressure on a large surface have been advanced by Dr. Siemens in a recent number of the *Comptes Rendus*. Nevertheless, in this instance of the Forth bridge we have assumed that a 56 lbs. hurricane will act simultaneously over the whole width of the Forth, with a resultant lateral pressure of no less than 8000 tons upon the main spans. We have further assumed that the said hurricane might blow down one side of the Forth, whilst a dead calm prevailed on the other side, and have even provided for the twisting action upon the piers and superstructure due to a 56 lbs. hurricane blowing *up* the Forth on one side, and *down* it on the other. To ascertain what lateral pressure a 56 lbs. hurricane would cause, we tested, both in air and in water currents, a large model of the bridge, with cross-bracing complete, and ascertained its equivalent in square feet of flat surface. Under any of the conditions of wind pressure enumerated above, combined with any distribution of the rolling load, the resultant stresses upon superstructure, holding down bolts and piers will be far within the safe working limits as determined by our experiments upon the respective materials.—B. B.

Altitude and Weather

In Nature, vol. xxvii. p. 176, you notice the remarkable warm and dry weather September 21 last on Ben Nevis, during an anticyclone, and, as at the foot the air was relatively cold and humid, you see in the heat and dryness on the mountain an effect of descending air currents. In this you are quite right, but I do not think you are right in estimating that this air was saturated at a certain height above Ben Nevis. The fact is this: the increase of temperature from a certain height above sea-level to the latter being *de facto* much less than the dynamical increase of a stratum of air, due to compression sinking down, a downward current of air will be generally warm, and relatively dry. It does not matter if it sinks along the slopes of mountains (as the foehn), or vertically, as modern meteorology considers it to be the case in anticyclones. There is only one great difference: the air currents down a slope may be, and often are, very violent, and only when they are so, their relative heat and dryness are felt, while the downward currents in an anticyclone are so gentle that they are seldom felt or directly registered, and that mostly the thermometer and hygrometer are our only means of detecting them. On account of their slow motions, the effect of these downward currents during anticyclones is little felt in valleys and plains, as (1) they are even more retarded near great land surfaces; (2) in the colder time of the year, especially when the ground is covered with snow, the radiation from the soil lowers the temperature of the lower strata. Thus during anticyclones in winter a very low temperature is generally experienced in plains and valleys, due to radiation, and a very high temperature and low humidity on isolated mountains, due to descending currents of air.

These conditions are best realised during protracted and considerable anticyclones, and it was Prof. Hann's merit to have explained this fact.[1] The exceedingly protracted anticyclone of December, 1879, in Central Europe, was especially favourable to the proof of the existence of descending currents, as the cold was great in the valleys, even in the high ones, like the Engadine and the Davos, but the air was warm and very dry on isolated mountains. An example from the best mountain observatory of that time, the Puy de Dôme, and the foot of it, will suffice, nine days, December 20-28, 1879, at 6 a.m.

	Feet.		Temp. F.		Relative humidity		Amount of Cloud
Puy de Dôme,	4813	...	38·8	...	38	...	1·3
Clermont (base)	1273	...	8·2	...	91	...	0·7

There is all reason to think that in these days there was no saturated stratum of air even considerably above, say the Puy de Dôme.

I must remark that Prof. Hann, in his last work, "Der Föhn in Bludenz," does not sustain his former opinion that great precipitations on the windward side of mountains is necessary to the appearance of a foehn on the leeward side. His opinion now is, that a considerable barometric gradient and the drawing in of air from considerable heights are alone necessary, for even if the air on the mountains is not abnormally warm, it will come down warm and relatively dry. A. Woeikoff

Ofizerskaja, St. Petersburg, December 15-27, 1882

The Fertilisation of the Speedwell

I fear that Dr. H. Muller's passage in Schenk's "Handbuch" would occupy too much space to be given here in full; but I can condense what he says into a few lines. Dr. Muller takes the *Veronica chamædrys* as representing a type of flowers in which the anthers have to be brought into a position to strike the body of the insect by the action of the insect itself. He finds the same arrangement in the *V. urticæfolia*. These flowers are visited by insects of various kinds, but their structure is, he thinks, explained only by what takes place when they are visited by *Syrphidæ*. When one of these insects visits such a flower, it hovers for some seconds before it, then settles upon the lower lobe of the corolla, without noticing the style which is coloured like the corolla, and which is now under the insect's body. It then crawls higher to reach the nectary, and in doing so bends down the stamens—which are also coloured like the corolla—until the anthers strike against the under part of the insect's body. The pollen thus obtained is carried to another flower, and brought into contact with the stigma when the insect first alights; and fresh pollen is again obtained by the attempts to reach the nectary. Dr. Muller either knows from observation or assumes that in the *V. chamædrys* anthers and stigma are mature at the same time. He attaches importance to the fact that both stamens and style are coloured like the corolla, and therefore appear to escape the observation of the insect; and the thinness of the base of the stamen is also noticed by him as one feature in the adaptation of the flower to the visits of *Syrphidæ*. He does not refer to the looseness of the corolla. Mr. Stapley's suggestion that this may play some part in the work of cross-fertilisation is an ingenious one, and calls for further research.

As to the *V. hederæfolia*, Dr. Muller mentions it as one of the plants that have a tendency to keep their flowers half-shut in cold and rainy weather, and thus to become cleistogamous.

I am sorry that I misunderstood Mr. Stapley's first letter upon any point; but he has misunderstood mine also, if he thinks I was not aware he wished to call attention "to the *adaptation* of the flower for cross-fertilisation." I wrote as briefly as I could, and naturally assumed that he would understand I was not thinking merely of the fact that Diptera drew down the stamens. Arthur Ransom

Bedford, December 23, 1882

THE SACRED TREE OF KUM-BUM

THE dissipation of illusions is always a little painful, even after repeated experience of the process. I must confess, then, to some feeling of injury at learning from Mr. Keane's interesting review in Nature, vol.

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[1] *Zeitschr für Meteorologie*, p. 129, 1876.

xxvii. p. 171, that Huc's "tree of ten thousand images" is nothing more than a common white lilac. Myths of this kind I have generally found to have some substratum of fact at the bottom. They can be rationalised, and mere explosion does not seem to be a satisfactory way of getting rid of them.

Now our knowledge of the indigenous vegetation of China is painfully limited. An immense portion of the flora is doubtless gone beyond recovery in the cultivated districts. Remnants of the primitive, wide-spreading forest remain, however, in the precincts of temples and monasteries, and these woods have always yielded novelties to botanists who have examined them. It had seemed, therefore, little short of certain that the sacred tree of Kum-bum would be something of considerable scientific interest if specimens of it could be got hold of.

The only edition of Huc at hand to refer to is Hazlitt's translation, published by Thomas Nelson and Sons in 1856. The well-known account of the tree will be found on pp. 324-6. According to Huc, the name Kum-bum, or as he spells it, Koun-boum, consists of "two Thibetan words signifying ten thousand images, and having allusion to the tree which, according to the legend, sprang from Tsong-Kaba's hair, and bears a Thibetan character on each of its leaves." Now, according to Kreitner, as quoted by Mr. Keane, "the Abbé Huc tells us that its leaves bear the natural impress of Buddha's likeness and of the Thibetan alphabet." As a matter of fact, he does not say anything like this. What he does say is as follows:—

"There were upon each of the leaves well-formed Thibetan characters, all of a green colour, some darker, some lighter than the leaf itself. Our first impression was a suspicion of fraud on the part of the Lamas, but, after a minute examination of every detail, we could not discover the least deception. The characters all appeared to us portions of the leaf itself, equally with its veins and nerves; the position was not the same in all; in one leaf they would be at the top of the leaf, in another in the middle, in a third at the base, or at the side; the younger leaves represented the characters only in a partial state of formation. The bark of the tree and its branches, which resemble that of the plane-tree, are also covered with these characters. When you remove a piece of old bark, the young bark under it exhibits the individual outlines of characters in a germinating state, and, what is very singular, these new characters are not unfrequently different from those which they replace."

Of the tree itself as Huc saw it some forty years ago, he gives the following account:—

"The tree of the Ten Thousand Images seemed to us of great age. Its trunk, which three men could scarcely embrace with outstretched arms, is not more than eight feet high; the branches, instead of shooting up, spread out in the shape of a plume of feathers, and are extremely bushy; few of them are dead. The leaves are always green, and the wood, which is of a reddish tint, has an exquisite odour, something like that of cinnamon. The Lamas informed us that in summer, towards the eighth moon, the tree produces huge red flowers of an extremely beautiful character."

Hazlitt's translation contains two woodcuts, one (p. 325) of the tree with its canopy, the other (p. 369) of a leaf with its markings. What the history of these illustrations is, there is nothing to show; Huc's book in the original French had, I think, none. The leaf with its markings has a by no means impossible appearance; whether the markings are like Thibetan characters, I cannot say. The outline of the leaf is not unlike that of a fuchsia, but it would not pass for a lilac.

I suspect, then, that there really was in Huc's time a tree with markings on the leaves, which the imagination of the pious assimilated to Thibetan characters. Perhaps it was the last local relic of some unknown endemic tree; in Hongkong I believe many of the endemic species are represented by but a few individuals. It may well have died and been replaced by a lilac, and the genuine markings by the fudged-up image of Budha "etched with some acid on the leaves."

It is disappointing that Szechenyi's expedition seems to have done nothing for botany. As Grisebach says, "We can only guess at the richness of the Chinese flora." Every now and then some one is induced to collect a few plants, and almost invariably they contain something new to science. A more extended knowledge of Chinese plants is now essential to a right understanding of the phyto-geographical facts of the north temperate flora. Unfortunately, the numerous Europeans who visit China are occupied with political, religious, or commercial business, with little time for subsidiary pursuits. But any of them who may chance to read these lines, may rest assured that they will be really doing a useful work by collecting and drying even a few *wild* plants in their respective neighbourhoods.

Kew W. T. THISELTON DYER

NORWEGIAN GEODETICAL OPERATIONS[1]

IN 1861 an Association was formed, under the auspices of Lieut.-General von Baeyer, having for its object the measurement of arcs of meridians, and parallels, in Europe. Most of the Continental nations joined this Association, and have carried out triangulations and spirit levellings of precision to further the objects in view. It is the intention of the Association to measure an arc extending from Palermo to Levanger in Norway, which will, however, probably be extended to the North Cape. The work before us is the report of the measurement of two base lines, and of their connection with the Norwegian triangulation which is to form part of the measurement of the above-mentioned arc. It was thought in 1862 that the existing Norwegian triangulation, supplemented and verified by some new work, would meet the requirements of the Association; but it was found, on investigation, that such was not the case, and moreover that the verifications could not be carried out, because the old trigonometrical stations could not be refound with any certainty. It was therefore decided to commence a new triangulation extending in a chain from the Swedish frontier (south of Christiania), where the chain is connected with the Swedish triangulation, to Levanger, where again a connection is to be made with another portion of the Swedish triangulation. The two base lines already mentioned are situated at the extremities of this chain of triangles, one at Egeberg, near Christiania, and the other at Rindenleret, near Levanger; both were measured during the summer of 1864, and Part I. is the report of these measurements.

The base measuring apparatus used is similar to that employed by Struve for the measurement of several base lines in Russia; it belongs to the Swedish Government, and was used for the measurement of their base lines. The apparatus consists of four cast-iron tubes, each approximately 2 toises[2] in length. One end of each tube is fitted with a small highly polished steel stud, and the other end with a "contact lever." The short arm of the contact lever terminates in a steel stud, which is intended to press against the fixed stud of the adjoining tube; the long arm moves on a scale. A measuring rod capable of varying its length to a slight extent is thus obtained, and this alteration in length can be measured with great delicacy, since the long arm of the lever greatly exaggerates it. This arrangement insures that the pressure between the rods is constant. Each tube is provided with two thermometers, the bulbs of which are bent nearly at right angles to the stem, and are inserted into small holes in the tubes. In order to protect the tubes as far as possible from changes of temperature they are wrapped round with several thicknesses of cloth, and are further inclosed in a wooden box, out of which the two ends of the tube just project. During the measurement of a base line each rod is supported on two trestles, at one-fourth and three-fourths of its length, provided with screw arrangements giving slow motions laterally and in elevation. The rods are not, however, accurately levelled, and a correction has to be made for dislevelment. To measure the small angle of inclination each rod is fitted with a very sensitive level. One end of the level works on trunnions, the other is connected to a micrometer screw by means of which the level can be raised or lowered. The bed of the level is attached to the top of the box, but in such a manner that it can be adjusted truly parallel to the tube. The value of each micrometer division was determined by means of the meridian circle in the observatory at Christiania. It will be seen from the above that, as the measurement of a base line proceeds, the following readings are required for each rod: (1) the contact lever; (2) the thermometers; (3) the micrometer for inclination. These readings were taken and booked independently by two observers. Both base lines were measured twice, once in each direction.

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[1] Publications of the Norwegian Committee of the European Association for the Measurement of Degrees Geodetical Operations. Published in Three Parts. (Christiania, 1880 and 1882.)

[2] A toise is 2·13151116 yards as determined by Col. A. R. Clarke, C B., R.E , F R.S , &c.

Before and after the measurement of each base line each rod was compared with a *standard* rod, the exact length of which was known, namely:

$$= 1727{\cdot}96641\ (1 + 0{\cdot}000011476\ (t - 16^\circ{\cdot}25)) \pm 0.00058$$

expressed in Paris lines[1] based on Bessel's toise, t being expressed in degrees Centigrade. It was found that the rods were slightly diminished in length during the measurement of a base line (on an average 0·005 lines) owing to abrasion. An allowance was made for this diminution in length. The apparatus with which these comparisons were made consists of a massive cast-iron beam, turned up at both ends, and carrying two supports fitted with rollers upon which the rod to be measured rests. One end of this beam is fitted with a fixed steel stud, against which the contact lever of the rod under comparison bears; the other end carries a sliding scale, connected with a contact-lever, and read by means of a micrometer microscope. A set of readings consisted in first measuring the standard rod, then each of the four measuring rods in succession, and lastly the standard rod again; the temperature of each rod was carefully noted. For a complete comparison twelve such sets of readings were taken.

The time occupied in measuring the Egeberg base was 18 days, and the observations for each measuring rod occupied 4 minutes; the Rindenleret base was measured more rapidly, namely, 2½ minutes per rod, due to the site being more level.

A considerable portion of Part I. is taken up in considering the errors to which the measurements of these base lines are liable, in estimating the allowances to be made to correct these errors, and in computing the probable errors of the final results. These errors are due: (1) to errors of observation in the actual measurement of the base lines; (2) to the error in the adopted length of the measuring rods.

Firstly, the errors to which the actual measurement of a base line is liable are as follows:—

A slight uncertainty attaches to the micrometer readings of the levels measuring the inclination of the rods. The probable error is computed to be

Egeberg base	± 0·350 lines
Rindenleret base	± 0·183 ,,

The errors due to the contact levers are next considered. It is shown that the error caused by the small uncertainty in the value of a degree of the scale over which the long arm of the lever moves, is too small to be taken into account, but the error caused by uncertainties in reading the scale is of sensible amount, and is computed to be

Egeberg base	± 0·015 lines
Rindenleret base	± 0·014 ,,

Further, the surface of the steel studs, at the end of the rods, is a portion of a sphere whose radius is considerably less than the length of a rod. Hence an error will occur each time a contact lever does not touch at the centre of the stud, that is if it makes an eccentric contact, and although every care was taken to obtain accurate contacts, it is considered that a correction of the following amounts should be made—

Egeberg base	− 0·351 ± 0·175 lines
Rindenleret base... ...	− 0·314 ± 0·157 ,,

The next source of error is that due to errors in alignment, these errors will always be negative, and are due to the uncertainty in placing the rods in the line given by the directing theodolite. This error is computed to amount to

Egeberg base	− 0·294 ± 0·101 lines
Rindenleret base... ...	− 0·262 ± 0·090 ,,

The computed variation of length of the rods due to alterations in temperature is vitiated by several errors. In the first place, the coefficient of expansion of the rods, as determined by Prof. Lindhagen, is affected by the small uncertainty, 0·000000015. Further, the correction for expansion is computed on the supposition that the thermometers do actually indicate the mean temperature of the rods at the time of taking the readings; but this is an assumption, and in fact it is estimated that the temperature indicated by the thermometers is the temperature the rod had 20·0 ± 5·9 minutes before taking the reading. This estimate is arrived at as follows: It will be remembered that each base line was measured twice; the difference between the two measurements is due to the various errors under consideration, and its probable value can therefore be computed; this computed value will contain, as an unknown, the time of which an estimate is required. Hence, by equating the computed difference to the actual difference the time can be found. The total error in the allowance made for expansion is found to be

Egeberg base	$\pm 0{\cdot}085 + 0{\cdot}525\,\Lambda$
Rindenleret base	$\pm 0{\cdot}071 + 0{\cdot}250\,\Lambda$

where $\Lambda = 20{\cdot}0 \pm 5{\cdot}9$ minutes.

Secondly, the errors due to the uncertainty in the accepted length of the rods are considered under four heads, namely: (1) the error in the length of the standard rod; (2) the error due to the bending of the beam of the comparing apparatus (some experiments were made to obtain data for the calculation of this error); (3) the error in comparing the rods with the standard; (4) the error due to the assumption that the diminution in length of the rods by abrasion is proportional to the length of time in use. The probable error of the accepted length of a rod during the measurement of the Egeberg base is computed to be ± 0·00081 lines, and during the measurement of the Rindenleret base ± 0·00071 lines.

Finally, the base lines had to be reduced to the sea-level; data had been obtained for this purpose by means of spirit-levelling operations. The reduction in the length of the base lines due to this cause is

Egeberg base	− 33·89 lines
Rindenleret base	− 0·852 ,,

Applying all these various corrections to the measured lengths of the base lines the final results are as follows:

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[1] A Paris line is defined by 1 Paris line = $\frac{1}{864}$ toise, hence 1 Paris line = 0·088813 English inch.

Egeberg base ... ... 2025·28316 toises,

with a probable error of ± 0·00129, or $\frac{1}{1,570,000}$ of its length.

Rindenleret base ... ... 1806·3177 toises,

with a probable error of ± 0·00120, or $\frac{1}{1,500,000}$ of its length.

This is a high degree of accuracy as compared with older base lines (as for instance several base lines measured in France between 1798 and 1828, of which the probable errors are $\frac{1}{250,000}$); but this accuracy has frequently been attained of late years, and even surpassed, as, for instance, the base line of Madridejos, measured by General Ibañez in 1858, with a probable error of $\frac{1}{5,865,800}$.

Part II. is the account of the connection of the Egeberg base with the side Toass-Kolsaas, and Part III. that of the connection of the Rindenleret base with the side Stokvola-Haarskallen of the principal triangulation. The observations were made during 1864-66, but owing to an error at one of the stations, due to the bisection of a wrong object, further observations were made at that station in 1877. The connection in each case is very complete, and the work is well tied in. The centres of the trigonometrical stations were very carefully defined by letting an iron bolt into the rock, or, into a large block of stone; the centre of the face of this bolt, marked by a small hole, was the trigonometrical station. The signals, to which the observations were taken, consisted of an upright beam, to which was attached one or two boards about 0·75 m. square, which were painted white or black, and occasionally a vertical stripe 0·11 m. broad was painted on the centre of the board. At several of the stations the theodolite could be placed beneath the signal, and at such stations the signal was placed over the bolt, but in several cases, owing to the nature of the ground, or other causes, the trigonometrical station had to be placed at some distance from the signal, in one case as much as 54 Norwegian feet. In such cases the corrections to be applied to the observations were obtained by measuring a short base line, one end of which was the trigonometrical station, and the direction nearly at right angles to the line joining the station and the signal. Observations were taken from the ends of this base to the various points on the signal, which were bisected from the other stations, and these, together with the observed bearings to and from the other stations, enabled the necessary corrections to be made. The greatest correction thus required was 10′ 37″·34. But even at stations where the theodolite was placed beneath the signal, corrections were required to reduce the observations to the trigonometrical station, because different points on the signal were observed from the other stations, and these points were not vertically over the bolt. In these cases the corrections were computed in the following manner:—A piece of paper, mounted on a board, was placed horizontally on the ground over the centre of the station, and this centre marked on it. Then, by means of a small theodolite, the "traces" of the vertical planes passing through the various points observed to, were marked in pencil on the paper. The theodolite was now shifted, and the corresponding traces marked as before; the intersections of these traces gave a series of points vertically beneath the points on the signal to which observations had been made. From these points, the corresponding bearings to the various stations were plotted on the paper; and, lastly, perpendiculars were dropped, from the point representing the centre of the station, on to these bearings; the length of any one of these perpendiculars divided by the approximate distance to the corresponding station is the tangent of the correction to be applied.

Two instruments were used for measuring the angles; a 10″ universal instrument by Olsen, read by two micrometer microscopes, and a 12″ theodolite by Reichenback, read by four verniers. The errors of graduation of these instruments were investigated, and are given in a tabular form in Part II. Although, owing to the numerous observations taken to each object starting from different parts of the horizontal limb, the errors of graduation must have been eliminated to a very large extent, yet it was thought advisable to apply these corrections to the observations, in order to obtain a more accurate idea of the bearings of each station. The errors of the micrometer microscopes are also given in a table. The 10″ instrument was used at all, the 12″ theodolite appears to have only been used at two stations. A third instrument, a 10″ universal instrument by Breithaupt and Sons, was used for the observations of 1877.

When observing, the instrument was first set at 0°, and a round of angles taken: the telescope was then reversed and the round taken again. The instrument was then set at 15° in the case of the triangulation connecting the Egeberg base, and at 20° (nearly) in the case of the Rindenleret base triangulation, and two rounds taken as before. The instrument was then again moved on 15° and 20° respectively, and so on. Thus in the first case forty-eight, and in the second thirty-six observations were taken to each station. In some few instances even a greater number were taken. The actual observations are not given in the Report, only the mean of four observations—two taken in the same position of the horizontal limb, and two in that position increased by 180°. The time occupied at each station averages four days; some stations were completed in two days.

The observations were compensated by the method enjoined by the Association for the measurement of degrees in Europe, namely, Bessel's method. The observed angles at each station are first compensated amongst themselves. A correction is then applied to each angle thus found, subject to the condition that the sum of the squares of these corrections for the whole triangulation is a minimum, and subject further to the geometrical conditions that the sum of the three angles of a triangle = 180 + spherical excess, and that the length of any side is the same by whatever *route* it is calculated. The necessary calculations are very laborious, and in the case of the Rindenleret base require the solution of simultaneous equations containing seventy-six unknowns. It is very questionable whether the result repays this labour; the method of compensation adopted for the Ordnance Survey, although perhaps not so rigid, compares favourably in this respect. The calculations for compensation are given very fully in the Report.

The Report is accompanied by plates showing the base measuring apparatus and the connecting triangulations.

ELEMENTS OF THE GREAT COMET OF 1882

(*Communicated by Vice-Admiral Rowan, Superintendent U.S. Naval Observatory*)

THE following elements were computed from three observations made at the U.S. Naval Observatory; the first and last being made with the Transit Circle, and the middle one compared with a known star which was afterwards observed on the Transit Circle:—

Wash. M T.		App. α.		App. δ.
		h. m. s.		° ′ ″
Sept. 19·9697877	...	11 14 18·94	...	− 0 34 29·7
Oct. 8·7204363	...	10 28 6·63	...	−10 40 22·6
Nov. 4·7009228	...	9 6 16·22	...	−27 21 26·7

From these observations we deduce—

Perihelion Time = Sept. 17·2228200 Greenwich Mean Time.

$$\left.\begin{array}{rl} \Omega = & 346^\circ\ 1'\ 7''.91 \\ \pi - \Omega = & 69\ 36\ 12.79 \\ i = & 141\ 59\ 52.16 \\ \phi = & 89\ 7\ 42.70 \\ \log a = & 1.9331366 \\ \log q = & 7.8904739 \end{array}\right\} 1882.0$$

$$\text{period} = 793.689 \text{ years}$$

$$\delta\lambda\cos\beta = -0''.06 \quad \delta\beta = +0''.01$$

$$x = r\,[9.9951411]\sin(170^\circ\ 42'\ 12''.72 + v)$$
$$y = r\,[9.9877234]\sin(262\ 46\ 57.39 + v)$$
$$z = r\,[9.4435130]\sin(\ 49\ 20\ 25.11 + v)$$

The observations as given were afterwards corrected for parallax by means of elements previously computed. These elements bear a considerable resemblance to Comet I., B.C. 371; and it may possibly be its third return, a very brilliant comet having been seen in full daylight A.D. 363.

E. FRISBY,
Prof. Math., U.S.N.
Washington, Dec. 19, 1882

THE DUMAS MEDAL

WE recently (vol. xxvii. p. 174) gave the addresses at the Paris Academy of Sciences in connection with the presentation to M. Dumas of a medal in commemoration of the fiftieth anniversary of his election to the Academy. We are now able, by the courtesy of our French contemporary, *La Nature*, to reproduce an illustration of this medal, which was presented by M. Jamin in words both eloquent and touching, as a token of the "love and gratitude" of the distinguished chemists' *confrères*, pupils, and friends. The medal is the work of M. Alphée Dubois.

J.B.A. DUMAS SECRÉTAIRE PERPÉTUEL DE L'ACADÉMIE DES SCIENCES
ALPHÉE DUBOIS
1882

PROFESSOR VON GRAFF'S MONOGRAPH ON THE TURBELLARIANS[1]

THIS splendid folio monograph consists of two volumes, the one comprising the text of over 600 pages illustrated by woodcuts, the other twenty as beautifully executed partially coloured plates as have ever been turned out, all from the author's own original drawings. The publication of the work has been assisted by a grant from the Berlin Royal Academy of Sciences.

Ludwig von Graff is Professor of Zoology at the College of Forestry at Aschaffenburg, in Bavaria. His first memoir on Turbellarians was published in 1873, at which time he first made up his mind to work out from his own observation a revision of the Turbellarians. The present monograph is, as he tells us in the preface, the result of almost incessant work during the last five years. He has made numerous journeys to the Naples and Triest stations, and has also visited many other parts of the European coasts north and south, and the fresh waters in all directions, in order to pursue his investigations on living Turbellarians. He has thus been able himself to examine 70 out of the 168 species of Rhabdocœlida which are known with certainty. The work being thus founded on so wide a personal acquaintance with the forms of which it deals, is of especial weight and value; it constitutes a systematic monograph of the Rhabdocœlida, founded on a sound basis of anatomical structure, and embracing all species hitherto described by other observers, together with those discovered by the author himself (thirty new species).

It is doubtful whether the present work will be followed by a second part embracing in a similar manner all the known Dendrocœlida. The matter depends on the amount of ground which may be covered by Dr. A. Lang's forthcoming monograph on Turbellarians, in the "Fauna and Flora of the Gulf of Naples." If this monograph proves to be so comprehensive that a further one would be superfluous, then Prof. Graff will publish a quantity of material collected by him concerning the Dendrocælida, in three smaller memoirs on the Polyclada, the Triclada, and embryology respectively. The present work is appropriately dedicated to the memory of O. F. Müller and Sir John Dalyell. It is pleasing to find the great merits of the latter thus recognised by a foreign naturalist.

The author does not admit Sidonia = *Rhodope varanii*, which, in opposition to Dr. R. Bergh, he considers to be a nudibranch, or Dinophilus, which has lately been shown to lie near the Archiannelids amongst the Turbellarians; and in the definition he gives of the group excepts the Microstomida, which differ from all other Turbellaria in having a complete periœsophageal nerve ring, in being diæcious, and in multiplying asexually by budding.

Separating, as is now so usual, the Nemertines altogether from the Turbellarians, he divides the group into the Rhabdocælida and Dendrocælida. In the definition given of the two sub-orders, an interesting point of difference is brought out, namely, that in the former the yelk glands are always present in the form of a pair of compact glands, whereas in the latter they are always divided up into numerous separate follicles.

The Rhabdocælida are divided by the author into three groups: I. Acœla; II. Rhabdocœla; III. Alloiocœla, which are thus defined:—

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[1] "Monographie der Turbellarien." 1. Rhabdocœlida. Dr. Ludwig von Graff. (Leipzig: W. Engelmann, 1882.)

1. Acœla. With digestive internal substance; without differentiation of a digestive tract and parenchym tissue. Without nervous system or excretory organs. All forms as yet known provided with an otolith.

2. Rhabdocœla. Digestive tract and parenchym tissue differentiated; a roomy body cavity usually present in which the regularly-shaped intestine is suspended by a small amount of parenchym tissue. With nervous system and excretory organ. Generative organs hermaphrodite (except in Microstoma and Stenostoma). Testes, as a rule, two compact glands. The female glands present as ovaries only, ovario-vitelligenous glands, or separate ovaries and yelk glands. Genital glands separated from the body parenchym by a special tunica propria. Pharynx always present, and very variously constructed. Otolith absent in most cases.

3. Alloiocœla. Digestive tract and parenchym tissue differentiated, but the body cavity much reduced by the abundant development of the latter. With nerve system and excretory organ. Generative organs hermaphrodite, with follicular testes and paired female glands, either ovaries only, or ovario-vitelligenous glands, or separate ovaries and yelk glands. Yelk glands irregularly lobular, rarely partially branched. Genital glands almost always without any tunica propria, lodged in the spaces in the body parenchym. Penis very uniform, and either without chilinous copulatory organs, or with these very little developed. Pharynx a pharynx variabilis or plicatus. Digestive tract lobular, or irregularly broadened out. All marine except one, or possibly two species.

Under the Alloiocœla come the genera—Plagiostoma, Vorticeros, Monotus, and others.

The work commences with a complete list of the literature on Turbellarians from the time of Trembley, who, in 1744, figured a black fresh-water Planarian to that of the publication of the last of Dr. Arnold Lang's important memoirs last year. The list is followed by a general treatise on the anatomy and physiology of the Rhabdocœlida. The account of the nematocysts of some forms is very interesting; their exact resemblance to those of Cœlenterata is fully borne out. *Microstonum lineare* appears to be the only species which, like Hydra and Cordylophora, possesses two kinds of nematocysts. The author thinks he has been able to detect on the surface of the cuticle, trigger hairs in connection with the nematocysts, like those in Hydroids. He considers the rhabdites or rod-bodies homologous with nematocysts, and refers, in connection with this question, to the nematocysts devoid of any thread which occur in many Cœlenterates, intermingled with fully developed ones. The structure of the pharynx is carefully gone into, and its different forms being of much use in classification, receive various names, such as *Pharynx bulbosus*, *P. plicatilis*, &c.

The water vascular system has been studied by von Graff with considerable success. It may consist of a single median main canal with a single posterior opening (Stenostoma) or a pair of laterally-placed canals with a similar single opening or two separate lateral canals with each a posterior opening (Derostoma), or there may be a pair of openings or a single one somewhat anteriorly placed. Ciliated funnel cells or flame cells such as exist in Cestodes, Trematodes, and Triclad Dendrocœles, have been discovered by von Graff also in the Rhabdocœlida. They do not, however, occur in connection with the tips of the ramifications of the water vascular canals, but almost entirely on the larger canals forming the networks. It is impossible here to follow the work further, through the interesting sections devoted to the development of Microstoma by budding, the habits of life and geographical distribution of the Rhabdocœlida. In connection with the discussion on classification, a table of the pedigree of Turbellaria is given, with Proporus as the ancestral starting-point. In this family tree the Dendrocœles are shown as derived from Acmostoma, a new genus of Alloiocœla, characterised by having a distinctly marked narrow ambulacral sole, the Polyclada directly, and the Triclada through Plagiostoma. The ascertained facts as to the structure of Turbellarians seem to point even more closely to their connection with the Cœlenterata. The presence of two kinds of nematocysts in one of the Rhabdocœla and possible occurrence in members of that group of trigger hairs, is a remarkable fact. Dr. Lang, believing that a part of the nervous system in Dendrocœles is truly mesenchymatous as in Ctenophora, and from other grounds concludes with Kowalewsky that the Polyclada are "creeping Cœlenterates which have many points of structure in common with the Ctenophora, some with the Medusæ. Such being the case, naturalists await with great impatience Kowalewsky's promised further information as to his extraordinary Cœloplana, supposed intermediate between Ctenophora and Dendrocœlida. The peculiar azygos character of the otolith in so many Dendrocœlida may perhaps be explained by the similar condition of the sense organ in Cœloplana. Prof. von Graff is much to be congratulated on the completion of this most important and admirable work.

H. N. MOSELEY

NOTES

WE greatly regret to announce the death of Mr. Charles V. Walker, F.R.S., at his residence at Tunbridge Wells, on the morning of December 24, 1882, in the seventy-first year of his age. Mr. Walker had been Telegraph Engineer to the South-Eastern Railway, since 1845. He had been a most zealous worker in the science of electricity, as the many works he leaves behind will testify. Indeed, he was one of the oldest telegraph engineers in the country, was the inventor of several useful appliances in connection with telegraphy, including the instruments by which the block system on railways is worked. His name is especially associated with the origin of the distribution of time by telegraph. On May 10, 1849, Mr. Glaisher wrote to Mr. Walker that he wished to talk with the latter about the laying down of a wire from the Observatory to the Lewisham Station, and on May 23 following, the Astronomer-Royal gave Mr. Walker a brief sketch of the use to be made of the wire referred to, his scheme, as he stated, being "the transmission of time by galvanic signal to every part of the kingdom in which there is a galvanic telegraph from London." It was proposed to lay four wires underground from the Royal Observatory to the railway station at Lewisham, and to extend them to London Bridge. The South-Eastern Railway Company gave every facility. On September 16, 1852, an electric clock at London Bridge Station was erected, and connected by wire with an electric clock at the Royal Observatory, Greenwich. The first time-signal sent from the Royal Observatory was received at London Bridge Station at 4 p.m. on August 5, 1852; and on August 9, 1852, Dover received a time-signal for the first time from the Royal Observatory direct, and it was made visible at certain first-class stations between London and Dover. After that the system rapidly spread, its success depending greatly on the scientific skill and enthusiasm of Mr. Walker. For some account of the subsequent development of the system, the reader may refer to the articles in NATURE, vol. xiv. pp. 50 and 110. Mr. Walker was treasurer of the Royal Astronomical Club for several years, and at the time of his death was president of the Society of Telegraph Engineers.

THE death is announced of Prof. Listing of Königsberg.

THE honour of Companion of the order of the Indian Empire has been conferred upon Surgeon-Major George Bidie, Superintendent of the Central Museum at Madras.

AT the last sitting of the year 1882, the Paris Academy of Sciences elected M. Bunsen, of Heidelberg, a Foreign Associate. M. Bunsen was already a Correspondent in the Section of Chemistry, and he will fill the place vacated by the death of M. Wöhler. It should be remembered that, contrary to the rule for members, who must be French citizens, and Associates, who must be foreigners, the correspondents of the Academy can be elected without any qualification of nationality; but none of them, either French subjects or foreigners, may live in Paris previous to their nomination. This rule is so strict that it is stated that an eminent man of science, wishing to become a candidate, removed his home from Paris to Versailles; and having been successful, returned to Paris, where he now lives.

THE Duc d'Aumale has been elected President of the Académie Française. M. Blanchard, the naturalist, will be President of the Academy of Sciences for 1883; he was vice-president during the past year. The vice-president for 1883, and future president for 1884 would be elected on Tuesday from the Mathematical Section. Before leaving the chair M. Jamin will, according to precedent, read a list of losses experienced by the Academy in 1882, and of the nominations made during the same period; he will also give a *résumé* of the progress of the several publications of the Academy.

ON Tuesday January 2, there was a gathering of people interested in educational progress, at No. 1, Byng Place, Gordon Square, to inspect the College Hall of Residence for Women Students which has lately been established there. Complete as this hall is in itself, we understand that it is only provisional until sufficient approval and support have been obtained to justify the opening of a building capable of accommodating a larger number of ladies. We may, however, regard it as embodying the idea of its founders, and as supplying in miniature a model of that comfortable and well-adapted academic residence which it is their object to provide for female undergraduates and art-students in London. The advantages to the members of this rapidly-increasing class of entering such a hall instead of taking separate lodgings or rooms in a boarding-house, or even living at home (in many cases) are not far to seek. Students in lodgings often suffer from neglect of health and under-feeding, while those who work at home are subject to interruptions and the strain of conflicting claims; and although they might avoid both these drawbacks in a good boarding house, they would still find that their residence was not adapted to the needs of student life. Whichever plan is adopted, girls generally lack opportunities of free intercourse with minds whose training has been about equal to their own, such as of late years they have been able to obtain at Oxford and Cambridge, and which is specially needed in London, the seat of the only English University that as yet admits them formally to degrees. Hence the three greatest benefits of the new hall will be: first, to bring the women students of London into social and intellectual fellowship, and thus to improve the quality of their work by encouraging conference on the subjects of study, without which it is hardly possible to acquire and test accuracy of thought; secondly, to diminish the causes of failure of health by care and good housekeeping; and thirdly, to increase the time at the disposal of students; thus, on the one hand, affording to the zealous worker opportunities of relaxation, which in different surroundings would be absorbed in housekeeping worries or other occupations, and, on the other hand, enabling the less enthusiastic to add to the quantity of their acquirements without increase of conscious effort. The Hall has been established chiefly in the interests of the students of University College (including the Slade School), but its usefulness is much enhanced by proximity to the London School of Medicine for Women, and the British Museum; for on this account we may fairly hope that it will contain numbers of students in the various departments of Literature, Science, and Medicine, and the Fine Arts. Liberality of thought and breadth of sympathy can hardly fail to be promoted, where subjects of interest are so varied amongst companions united by the common principle of serious study. Although the Hall was only opened last term, we notice with pleasure that all the rooms are already taken; hence there is reasonable ground for hope that the larger scheme of the Committee will before long be realised. That the interests of students of science will be well looked after may be gathered from the fact that the presidents of the Royal Society and of the British Association, Prof. Huxley, Dr. Gladstone, Mr. Samuelson, M.P., Prof. Carey Foster, and others are aiding the scheme. Sir John Lubbock is the treasurer of the Building Fund.

DR. VON HOCHSTETTER, for many years president of the Vienna Geographical Society has resigned this post and has been nominated honorary president for life. In his stead Count Hans Wilczek was elected president.

THE death is announced of Karl Winter, the well-known electrician. He died at Vienna on December 7 last.

PROF. W. GRYLLS ADAMS, F.R.S., will deliver a course of lectures on voltaic and dynamic electricity and magnetism, and their applications to cable-testing, electric lighting, &c., at King's College, London, during the ensuing session. A course of practical work in electrical testing and measurement with especial reference to electrical engineering will also be carried on under his direction in the Wheatstone Laboratory. The lectures will be given once a week on Mondays at 2 p.m., and the laboratory will be open daily (Saturday excepted) from 1 to 4. The work will begin on Monday, January 15.

THE French Senate has diminished by a million of francs (40,000*l*.), the Budget of Public Instruction for 1883. It is regarded as a warning given to the Lower House, not to spend with too free a hand the public funds for educational purposes.

THE continual rains are creating serious apprehensions in Paris, and the Seine has again reached the level of disastrous inundations. A similar calamity is befalling other cities in France, amongst which the foremost is Lyons. The calamity having been foreseen by the Hydrological Service, all measures have been taken to diminish as much as possible the extent of the disaster. *Akhbar* states that heavy rains have been experienced in Algiers, and even at Laghouat, where it has been received with a real exultation. The newspapers are full of the disastrous floods caused by the rise of nearly all the great rivers in the Central European plain.

WITH reference to a recent note to the effect that snow fell on November 11 in Madrid to the depth of 1 foot, Mr. Gillman writes that snow began to descend early that morning, but had ceased at midday. He nowhere found it deeper than 6 inches, but this was uniform in the streets and open country. In the night of 11th–12th, the minimum thermometer marked – 11° cent.; barometer on Sunday stood at 688 millims.

THE appearance within the last two years of two comets has been regarded as a most menacing portent by Chinese politicians. Their resemblance to flaming swords is regarded as emblematical of the vengeance of heaven on an unworthy nation. It is stated that in consequence of the last comet, an urgent decree has been promulgated in the name of the youthful monarch, stating that it is a clear indication that the officials are lax in making proper reports to the Throne, and have been keeping the Emperor in the dark as to pestilences and other calamities among the people. His Majesty has reason to believe that improper officials have been appointed; he has, moreover, subjected his Imperial heart to a rigorous examination in the seclusion of his palace, and he is much disquieted at the result. The people, he finds, are poverty-stricken, and await relief, and the present is a time of great anxiety and embarrassment. The crisis must be met with prompt measures and a reverent heart; the ministers are accordingly enjoined to exhibit loyalty and justice, and to strenuously guard themselves against the thraldom of official routine. They are to discover the real state of the country, and to make such dispositions as may give rise to all possible advantage, and eradicate all possible evil. If all this be done, we have the Imperial assurance that the people will live in peace and quietness, till heaven be in harmony with earth, and all harmful influences allayed. If decrees were always obeyed, the comet will have exercised a beneficent influence on the condition of the Chinese people.

ALL interested in photography will find much that is useful and curious in Mr. Baden Pritchard's Year-Book of Photography for 1883.

MR. E. ROBERTS has sent us his handy and useful Tide Table for 1883, containing the times of high water at London Bridge, and showing the possible overflows; to all Londoners interested in any way in their river, this table will prove serviceable. We have also from Mr. Roberts Tide Tables for the Indian ports, and Tide Tables for the port of Hongkong, in handy little volumes, containing many carefully compiled tables calculated to be of great service.

THE total number of visitors to the Royal Gardens, Kew, for the year 1882, was 1,244,167. This is 407,491 in excess of the numbers for 1881, which in its turn was greater by 111,254 than the number of visitors in any previous year. As in 1881 the Sunday visitors (606,935) were about equal in number to those on all the other days of the week put together (637,232).

A NEW natural history magazine in the Flemish language is announced. It is published at Ghent, and the title is *Natura Maandschrift voor Natuurwetenschaffen uitgegeven door het Natuurwetenschappelijk Genootschap von Ghent.* The editors are J. MacLeod, Ed. Remonchamps, and L. Baeklandt. The natural sequence is that another Belgian magazine, in Wallon, will appear. The "gift of tongues" is daily becoming more and more a necessity for a working naturalist, and De Candolle's assertion that English is destined to become the language of science seems gradually more remote in realisation.

THE December number of the *Agricultural Students' Gazette*, Royal Agricultural College, Cirencester, contains an article by Sir J. B. Lawes on the future of agricultural field experiments, in which he points out that the time when isolated field experiments were of value has passed, and that now the questions to be solved in this way are such as can only be answered by carefully conducted experiments lasting over many years. Miss Ormerod contributes a paper on the Gooseberry Caterpillar, the larva of *Nematus Ribesii*, in which she suggests the best mode of preventing its ravages. A readable summary of the recent work of Leuckart and Thomas on the life-history of the Fluke is given by Mr. Ozame. The other papers in the number are on Contagious Diseases, by Prof. Garside; on the Harvest of 1882, by Prof. Little; on Butter-making, by Mr. Weber; besides much matter of more purely College interest. We notice that the College has commenced a series of field experiments on corn crops, in conducting which doubtless the advice of Sir J. B. Lawes will be followed. This *Gazette* in its new form promises to become of permanent value, and is exceedingly creditable to its editors, students of the Royal Agricultural College.

THE third expedition fitted out by the Milan Society for the commercial exploration of Africa, will leave early this month for Massana. The leader of the expedition is Signor Bianchi, who knows Abyssinia thoroughly. Count Salimboni accompanies him as engineer, and Prof. Licata as naturalist.

PROF. DOMENICO LOVISATO and Lieut. Bove, who jointly undertook the last Italian Antarctic expedition, are about to undertake another Antarctic journey for scientific purposes.

NEWS has been received from the German traveller, Robert Flegel, who was sent out to explore the Niger-Binue district. It appears that on April 10 last the traveller crossed the Binue River to the southern shore, and reached the large town of Wukari on April 13. By way of Bantadchi he proceeded, in four days' journey, to the decaying government city of Bakundi, in one and a half days more to Beli, and thence he reached Kontcha in the Adamnua district on May 26. From Kontcha to Jola is only a seven days' route. Flegel, whose health has much improved, strongly advises the establishment of a German station in that healthy and fertile country.

WE have on our table the following books:—Sydney Observatory, Double-Star Results, 1871-81 (Sydney); Der Electricitat und der Magnetism, vol. i., Clerk Maxwell (Springer, Berlin); Cutting Tools, R. H. Smith (Cassell, Petter, and Galpin); A New Theory of Nature, D. Dewar (W. Reeves); Transactions of the Sanitary Institute, vol. iii. (Stanford); The Great Pyramid, R. A. Proctor (Chatto and Windus); Microbes in Fermentation, Putrefaction, and Disease, Ch. Cameron (Baillière, Tindall, and Co.); The Nebulæ, a Fragment of Astronomical History, A. E. Garrod (Parker); Relative Mortality of Large and Small Hospitals, H. C. Burdett (Churchills), To the Gold Coast for Gold, Burton and Cameron (Chatto and Windus); Physical Optics, R. T. Glazebrook (Longman); Essays in Philosophical Criticism, Seth and Haldane (Longmans); Year-Book of Photography, 1883, H. B. Pritchard (Piper and Carter); Report on the Oban Pennatulida (A. M. Marshall and W. P. Marshall); Catalogue of *Batrachia gradientia*, G. A. Boulenger (British Museum); The Brewer, Distiller, and Wine Manufacturer (Churchills); The Churchman's Almanak for Eight Centuries, W. A. Whitworth (Wells, Gardner, and Co.); Celtic Britain, Prof. J. Rhys (S.P.C.K.); Zoological Record, vol. xviii. 1881 (Van Voorst); Rankine's Useful Rules and Tables, sixth edition (Griffin); Madeira Spectroscopic, C. Piazzi Smyth (W. and A. K. Johnston); Ragnarok, the Age of Fire and Gravel, Ig. Donnelly (Sampson Low and Co.); The Electric Lighting Act, 1882, Clement Higgins and E. W. W. Edwards (W. Clowes).

THE additions to the Zoological Society's Gardens during the past week include a Black-eared Marmoset (*Hapale penicillata* ♂) from South-East Brazil, presented by Miss Tilleard; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. W. L. Brodie; a Rose Hill Parrakeet (*Platycercus eximius*) from Australia, presented by Mr. Geo. Lawson, F.Z.S.; a Black Tortoise (*Testudo carbonaria*) from St. Thomas', West Indies, presented by Viscount Tarbat, F.Z.S.; an Indian Cobra (*Naia tripudians*) from India, presented by Capt. Braddick; two Common Curlews (*Numenius arquata*), a Common Lapwing (*Vanellus cristatus*), a Golden Plover (*Charadrius pluvialis*), British, purchased.

BIOLOGICAL NOTES

ON A NEW GENUS OF CRYPTOPHYCEÆ.—It would appear that the interesting fresh-water genus of Algæ described by Bornet and Grunow as Mazæa (*vide* NATURE, vol. xxvi. p. 557) is without doubt the same as Nostochopsis of Wood. This genus of Wood was first briefly described in the *Proc. Amer. Philos. Soc.*, 1869, and more fully, and with good figures, in the "Fresh-water Algæ of the United States," 1872. The Philadelphia species, *N. lobatus*, Wood, is referred by its discoverer to the Rivulaceæ, and is apparently a different species from that described by Bornet and Grunow from Brazil.

FEMALE FLOWERS IN CONIFERÆ.—Quite recently Celakovsky has published a very elaborate criticism (on the structure of the female flowers in Coniferæ, as detailed in Eichler's well-known treatise). To this ("Zur Kritik der Ansichten von der Fruchtschuppe der Abietineen," &c. Prag, 1882), Eichler has replied in a paper read before the Gesellschaft der Nat. Freunde zu Berlin, in which he re-states the chief points of his proof and answers *seriatim* the objections brought against it. Dr. Peters sums these up as follows:—1. In all the vegetative buds of the pine, the two front leaves (Vorblatter) converge forwards towards the bract; it is hence improbable that in the fruit scale they should be turned backwards. Celakovsky, from the fact that in weak buds the former arrangement is somewhat modified, concludes that on the complete falling away of the bud from between the front leaves, these latter are enabled to push themselves backwards and cohere: an opinion not proved. 2. While in the vegetative bud, the leaf immediately following the front leaf falls backwards in abnormal fruit-scales, the portion interpreted as the next leaf falls forwards. To the representation of Celakovsky's, that owing to the fact that the front side being, in the course of development, preferentially assisted, the leaf of the assisted front side first reaches its development, Eichler opposes the statement that in the ordinary buds there is not a trace of such a preferential furtherance. 3. The part that is regarded as the third leaf of the bud cannot be a leaf, because it has its xylem on the dorsal, and its phloem on its ventral surface. Celakovsky takes a twist of 180° for granted. This Eichler denounces as an evasion which would bring all serious scientific discussion to an end. 4. If the fruit-scale were formed by the growing together of two front leaves upon the hinder end of their axis, the latter if it developed further, would come to stand on the front side of the fruit-scale, but *de facto* it under such conditions stands behind. As Celakovsky however thinks that the middle piece of the front scale is half turned round, and is a leaf on the front side of the bud, to which both front leaves on the front side of the bud have adhered, by which means the axis comes to be posterior; therefore this opinion stands irreconcilably contradicted by his own supposition of the simultaneous pushing back of the front leaves. 5. The simplest explanation of the bud-arrangement, and of the bud itself, is got by supposing that the bract and the fruit-scale form together a single leaf which has produced an axillary bud. Here Eichler considers himself compelled to deny the charge of having set out with pre-formed notions. The change in his former opinions was brought about by a more intimate knowledge of the facts. 6. By pressure and excitation (Reiz) the axillary bud causes further changes in the fruit-scale, the formation of the keel and wings, while the central piece which is bounded by them, can separate itself from the side portions and assume the appearance of a special leaf. To Celakovsky's objection, that through the pressure of the bud-axis, only a circumscribed depression, and not a long furrow would be formed, there is this reply, that such a furrow must be produced by the growth of the scale past the early developed bud, and that this furrow can become wider as the scale becomes broader. 7. These keels (midribs) of the fruit-scale press past the bud on both sides, and hinder the development of the first lateral bud-leaves, so that the first bud-leaf now arises upon the hinder side. This explanation, characterised by Celakovsky as a forced hypothesis, is supported by the fact that the leaves could not become formed in a place where there is no room, and because on the other hand the two lateral bud-leaves show themselves if the mid-ribs are wanting or remain feebly developed (*Botan. Zeitung*, December 8).

THE TRACHEÆ IN LAMPYRIDÆ.—Heinrich Ritter v. Wielowiejski publishes in the November number of the *Zeitschrift fur wissenschaftliche Zoologie* a very detailed account of the light-producing organs in *Lampyris splendidula* and *L. noctiluca*. His investigations were carried on at Jena, in Prof. Oscar Hertwig's laboratory. He sums up the most important results as follows:—1. The tracheal-terminal-cells of M. Schultze, which become black under osmic acid, are by no means—as their name would imply—the terminations of the respiratory tubes; for these branch out further on into brush-like masses of much finer capillaries, which are without the chitine spiral; they are very attenuated, and, making their way in the peritoneal layer (peritonealhaut), are numerously distributed to phosphorescent tissue. 2. The tracheal capillaries very rarely end abruptly (blind) in the phosphorescent organs, but most frequently anastomose with one another, forming an irregular meshwork. 3. The capillaries do not seem to enter into the structure of the parenchymatous cells, but rather course along their surface, often irregularly winding around and enveloping these. 4. The tracheal-terminal-cells are nothing more than the outer elements of the peritoneal layer at the base of the tracheal capillaries, which radiate in a brush-like fashion from a chitine-spiral-trachea. Their peripheral processes represent the extension of the latter upon the capillaries, and this relationship is homologous with certain embryonic stages of the tracheal system. 5. The tracheal-terminal-cells are not the seat or point of departure of the light-development. If this appears first in their vicinity, it is only a consequence of the fact that these structures have, owing to their affinity for oxygen, stored up in themselves a supply of this gas, and give it off in greater quantity to the neighbouring tissues. 6. The light-producing function is peculiar to the parenchyma-cells of the light-producing organs. It results from a slow oxidation of a substance formed by them under the control of the nervous system. 7. The ventral light-organ was found to consist of two layers, the parenchyma-cells of which are quite similar to one another in their morphological characters, but they differ from one another in the chemical nature of their contents. 8. The parenchyma-cells (is this the case with all?) seem connected with fine nerve-endings. 9. The light-organs are the morphological equivalents of the fatty-bodies.

THE STONES OF SAREPTA (ASIATIC RUSSIA).—The remarkable masses of stone found in the white sand of the Ergent Mountains at Sarepta have often caused people to inquire how they were formed. Some of them are found of the size of a hazel or walnut, and even larger; others are cylindrical, of the thickness of a half to one werschok (16 werschok = 28 inches), and a quarter to a half arschin (28 inches) long; others again target-shaped are more than a half arschin long, and one to four werschok thick. All the cylindrical ones, which are often also forked and root-shaped, exhibit, when they are broken across, a brown kernel with a white spot in its centre. Their surface is rough, and resembles a number of drops heaped one upon and beside another. When Alexander v. Humboldt visited Sarepta, the then director, Zwick, showed him these stone formations, Humboldt, while declaring that they were worthless recent things, was unable to say how they arose. Zwick, on the other hand, regarded them as very old and very problematical. Gobel also, who was afterwards shown these stones by Zwick, was unable to explain how they were formed. When Auerbach, the secretary of the Moscow Natural History Society, paid Alex Becker a visit twenty-eight years ago, he was brought to the place where these stone deposits were. He looked for an explanation of the formation of these stones and the reason of each stone containing a brown kernel. He was told that the stones were formed by roots. Auerbach said that these would form hydrochloric acid by decomposition. Becker now believes that he can with certainty assert that these formations arise round the roots of several plants that contain *milky* juice. *Tragopon ruthenicus*, *Scorzonera ensifolia*, and *Euphorbia gerardiana* grow plentifully in the white sand. Their long roots are inhabited and seamed by insects, and when their surface is once lacerated, their milky juice keeps perpetually flowing, and as it is sticky, the chalk-containing sand (the sand's colour is due only to the presence of chalk) settles firmly around the root. The root gradually dies, disappears, and there remains in its place a white, often hollow, kernel, together with the brown colour of the root-cortex. As the root is white under its cortex, the kernel also appears white, surrounded with the brown layer of the root-cortex. The round and target-shaped ones may originate from the milky juice running away into the sand, and therefore hardly any of them exhibit a brown kernel. Their guttiform surface can be explained by the drops of the milky juice. The cylindrical, forked, and root-shaped stones show clearly the form of the roots. *Euphorbia gerardiana*, to which these stone formations are chiefly ascribed, has very long roots, root-branches and root-fibres (Alec Becker, *Bull. de la Soc. Imp. des Natur. de Moscou*, 1882, No. 1. p. 48).

AMERICAN RESEARCHES ON WATER ANALYSIS[1]

THE chemical results as to animal in contrast with vegetable organic matter in water, support, in general, the conclusions that have been usually drawn as to the source of organic matter, based on the more highly nitrogenous character of that from animal than that from vegetable *débris*. Still the necessity for caution is shown; *e.g.* samples containing the refuse of canning tomatoes might have been erroneously thought contaminated with animal matter; others, containing a watery infusion of human fæces, with vegetable matter, &c.

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[1] Concluded from p. 213

Of the biological results under the same head, the most noteworthy is the well-marked pathological effect on rabbits of the injection of waters contaminated solely by such vegetable matter as would usually be thought harmless, *e.g.* peaty water. True, in the well-marked cases, the amount of organic matter present was large, but not beyond that in water sometimes used for drinking purposes. The Dismal Swamp water is an example; it has often been chosen for ship-supply, and has been spoken of as a source of supply for the city of Norfolk. On the theory (which has much in its favour) of disease caused by drinking water being due to the presence and action of living organisms, there might possibly be safety in drinking a peaty water, or water filtered through dead forest leaves, when fresh; danger, when, after some exposure, bacteria had been developed; and safety, again, perhaps, after the growth of these had fallen off, and more or less of the available organic matter had been consumed. Ship-captains say the Dismal Swamp water, after a time, becomes remarkably good and wholesome.

As to the putrescent or non-putrescent character of organic matter in water, the chemical evidence goes to prove (in opposition to Tidy's opinion) that the proportionate consumption of oxygen from permanganate within the first hour is rather greater for those waters containing vegetable than for those containing animal matter. Dr. Smart has expressed the opinion that gradual evolution of albuminoid ammonia indicates the presence of organic matter (vegetable or animal) in a fresh or comparatively fresh condition, while rapid evolution indicates that it is putrescent. His interpretations in this respect proved to be correct in a large proportion of cases, but not always.

The biological results under this head accord, on the whole, with the general belief that putrescent organic matter is more dangerous than that in a fresh or but slowly decomposing condition.

Prof. Mallet proceeds to state some *general conclusions with a view to sanitary application as to the value, separately and collectively, of the different processes of water analysis which have been under examination.*

It is not possible to decide absolutely on the wholesomeness or unwholesomeness of a drinking water by the mere use of any of the processes examined for the estimation of *organic matter*, or its constituents. Not only must such processes be used in connection with investigation of other more general evidence, as to the source and history of a water, but this should even be deemed of secondary importance in weighing the reasons for accepting or rejecting a water not manifestly unfit for drinking on other grounds.

There are no sound grounds on which to establish such general "standards of purity" as have been proposed, looking to exact amounts of organic carbon or nitrogen, "albuminoid ammonia," oxygen of permanganate consumed, &c., as permissible or not.

Chemical examination may be quite legitimately applied, first, to the detection of *very gross pollution* (as of a well from crushing of soil pipes), and secondly, to periodical examination of a water supply, so that suspicious changes from the ascertained normal character of the water may be promptly determined and their cause investigated. In the latter connexion there seems to be no objection to the establishment of *local* "standards of purity," based on thorough examination of the supply in its normal condition.

A careful determination of the nitrites and nitrates seems very important.

If he had to watch a large city water supply, the author would use all the three processes; each gives information which the others do not. Where only simple means were practicable, the albuminoid ammonia and permanganate processes might be employed together; but in no case should one only of these methods be resorted to.

Practical Suggestions as to the Use in their present form of the Chemical Processes Studied.—In general, water samples should be examined with the least possible delay after collection. Besides examination of a water in its fresh condition, samples of it should be set aside in half-filled but closed glass-stoppered bottles for (say) ten or twelve days, and one of these examined every day or two, to trace changes undergone.

In the case of the combustion process, however skilful the analyst, duplicate or even triplicate concordant results should be insisted on. To avoid the presence of ammonia from coal gas in the atmosphere about the water-bath, the bath should be heated by steam brought in a small closed pipe from a distant boiler (preferably in another room), and the waste steam and condensed water should be carried off to a safe distance.

As to the albuminoid ammonia process, it would be well to adopt the rule that the distillation be stopped when, and not before, the last measure of distillate collected contains less than a *certain proportion*, say 1 per cent., of the whole quantity of ammonia already collected. To diminish the loss of amines or other volatile forms of nitrogenous matter, a separate distillation should be made with alkaline permanganate added *at once*, besides the usual course of treatment prescribed by Wanklyn, and the results of the two distillations compared. The details of the evolution of ammonia should always be given.

The Tidy form of the permanganate process is rather to be recommended than that of Kubel, if but one be used. The time during which the permanganate is allowed to act in the Tidy process should be increased to at least 12, better to 24 hours, *several* determinations, on different samples set aside at the same time, being made at (say) 1, 3, 6, 9, and 12 hours, to trace the progress of the oxidation.

Suggestions as to possible Improvements on the Processes examined deserving further Investigation—Combustion Process.—The author proposes to evaporate the water in a closed vessel immersed in a water bath, and connected with a good (water jet) air pump, a condensing worm being provided for the aqueous vapour, the feed to be managed through a nearly capillary tube with a glass stop-cock. The evaporation would thus be effected within a moderate time at a fixed temperature much lower than the boiling point. The loss of organic matter by simple volatilisation or oxidation would be greatly reduced; much less sulphurous acid would be required; the tendency to formation of sulphuric acid would be reduced to a minimum, and absorption of ammonia from the atmosphere about the dish quite prevented. In testing this last effect, two bulb tubes containing pure sulphuric acid might be interposed between the vacuum chamber and the pump.

For certain reasons it might be well to evaporate at first with the addition of a small excess of magnesia (as recommended by Lechartier), thus removing all ammonia, and then, the water having been brought down to a small volume, add a moderate excess only of sulphurous acid with a drop of a solution of ferrous salt (as directed by Frankland), and complete the evaporation to dryness—the whole in a jet pump vacuum, as suggested.

Further experiments as to the Williams method (copper-zinc couple) for removal of nitrates, are desirable.

From preliminary experiments, the author thinks nitrates and nitrites may be completely reduced by evaporating to a small bulk with no great excess of phosphorous or hypo-phosphorous acid, guarding against evolution of phosphuretted hydrogen by use of a low temperature, then adding magnesia in small excess, and completing the evaporation. The plan deserves to be carefully tested.

Albuminoid-ammonia Process, including Determination of free Ammonia.—To prevent (or at least largely reduce and make uniform) the loss of ammonia from imperfect condensation, the author would use a retort in a saline solution kept heated by steam (at say 102° or 105° C.), and condense in a glass worm surrounded by ice water, till the distillate should be brought to a uniform temperature not over (say) 5° C. It might be still better to distil in a completely closed apparatus, with a fixed difference of temperature between the retort and the far end of the condensing tube, with glass stopcock to draw off the distillate in successive measured portions, and a small safety-valve near the cold end.

In determination of free ammonia, it might be well to try a closed distilling apparatus connected with a (water-jet) air-pump, so as to maintain a partial vacuum within, keeping the retort at a fixed temperature much below 100° C., and collecting all the ammonia in a flask and one or two bulb tubes, with weak mineral acid placed between condenser and pump. There would be the disadvantage, however, that the progress of evolution of ammonia could not be easily traced by its collection in separate successive measures of the distillate; and it would be necessary to ascertain whether the application of the Nessler test would be at all interfered with by the sodium salts formed from the acid used.

To overcome, if possible, the most serious difficulty in the way of correct determination of free ammonia, viz. the ready breaking up of urea (and other amides) when present, on heating with sodium carbonate, it would be well to ascertain if Schlœsing's method for determination of ammonia admits of being applied to such excessively minute amounts of it as the water analyst is concerned with.

In conduction of the albuminoid-ammonia process proper, *i.e.* the distillation with alkaline permanganate, the author would keep the original volume of liquid in the retort constant, by admitting ammonia-free distilled water through a capillary tube with a glass stop-cock. When there is so much organic matter as to reduce, wholly or greatly, the usual charge of alkaline permanganate, he would first determine at about what a rate the reagent is used up, then progressively supply its solution, so as to keep the original strength as nearly as possible unaltered.

Permanganate Process.—The principle involved in the last paragraph applies also to this process. There should be a constant excess of permanganate all through the process. The process should be carried on at a pretty nearly fixed temperature (say 20° C. if the Tidy method be followed).

In conclusion, the author expresses a wish that more extended biological experiments should be made as to the effects of water variously polluted on the lower animals (other animals as well as rabbits), and that the action of water introduced into the stomach as well as hypodermically injected, should be tested. It would be well to have chemical examinations, on uniform plan, from time to time made of the water supply of the largest cities at periods when the general assent of medical men indicates unusual prevalence of, or exemption from, the classes of disease most probably capable of origination from the organic pollution of drinking-water. The author would especially suggest a combined chemical and biological inquiry as to the possible effects upon living animals of the ferment or ferments of nitrification in different stages of that process. Some minor questions connected with development of nitrites and nitrates from decomposing organic matter also deserve further examination.

LOCKYER'S DISSOCIATION-THEORY[1]

IN February, 1880, I took occasion, on the ground of my observations to the spectrum of chemically pure hydrogen, to take objection, to Lockyer's view, that calcium, at a very high temperature, is dissociated.[2] From the fact, *inter alia*, that of the two calcium lines, H′ and H″, only the first is present in the spectra of so-called white stars photographed by Huggins, Lockyer proceeded to lay down the theory that calcium at a high temperature is decomposed into two substances, X and Y, of which the first gives the line H′, the other the line H″, and that in the stars referred to, only the first is met with. Against this I urged that hydrogen, besides the four known and easily visible lines, has a remarkable line of very intense photographic power, which nearly coincides with Fraunhofer's H′, and that one is the more warranted in regarding the supposed calcium-line observed by Huggins as a fifth hydrogen line, that the hydrogen lines in the spectra of those stars are developed in a striking manner, and also the ultra-violet star lines observed by Huggins, agree with the ultra-violet hydrogen lines photographically fixed by me.[3]

Lockyer, however, has not given up his idea of dissociation, but sought new proofs of it by the spectroscopic method.

He calls attention to the fact, *inter alia*, that in the spectrum of sun-spots, certain iron-lines appear broadened, and others not; that, moreover, many of them, as λ 4918 and λ 4919·7 do not occur in the spectrum of protuberances, which show other iron lines, but do in the spectrum of spots; that in the latter again, the iron lines are occasionally absent, which the former contain, and he proceeds to say: "there is, accordingly, no iron in the sun, but only its constituents."[4]

This argumentation Liveing and Dewar[5] have already opposed, having proved that certain spectral lines of a substance, *e.g.* λ 5210 magnesium, and various calcium-lines, are only visible when certain foreign matters are present; in this case hydrogen on the one hand, and iron on the other; that accordingly the absence of certain iron lines in the spectra of the spots or protuberances may not be attributed to a dissociation, but to the absence of foreign matters which occasion the appearance of these lines in force.

Lockyer now takes his stand, however, on another fact, which is not explained by Liveing and Dewar's experiments, and which certainly seems to afford a firmer basis for his theory of dissociation than the facts referred to above. He says:[1]

"The last series of observations relates to the degree of motion of vapours in the sun-spots, which it is known, is indicated by changes in the refrangibility of lines. If all lines of iron in a spot were produced by iron vapour, which moves with a velocity of 40 km. in a second, this velocity would be indicated by a change of the refrangibility of *all* lines. But we find that that is *not* the case. We find not only different motions, which are indicated by different lines, but observe in the degree of motion the same inversions as in the breadth of the lines. This fact is easily explained, if we suppose dissociation, and *I know no more simple way of explaining it.*"

Lockyer cites as an example that in the spots of December 24, 1880, and January 1 and 6, 1881, a certain number of iron lines appeared bent, while others remained straight.

Now I believe it is possible to explain these facts on the basis of numerous observations in spectral analysis of absorption without needing to have recourse to the hypothesis of dissociation.

It is known that the position of the absorption-band of a substance depends very essentially on the dispersion of the medium in which it is dissolved or incorporated. One often observes that in strongly dispersive media the absorption-bands of a substance are displaced towards the red.[2] Now, the remarkable case often here occurs that certain absorption-bands are displaced with the increase of dispersion of the solvent, while others are not. Thus Hagenbach observed that, *e.g.*, the chlorophyll bands I. III. and IV. lie more towards red in alcoholic than in etheric solution, while the band II. in both solutions shows exactly the same position. I observed similar cases with uranian protoxide salts[3] and with cobalt compounds.[4]

Now Kundt has already called attention to the fact, that for absorption-spectra of gases the same rule holds good as for the absorption spectra of liquid substances. He adds, indeed: "It is only questionable whether, if, *e.g.* hyponitrate gas be mixed with various other transparent gases, the displacements of the absorption-bands are so considerable, that they can be perceived." This doubt, however, does not affect the rule supposed, but merely its experimental verification.[5] The supposition, then, is permissible that, in the same way as with liquids, added media also affect the position of absorption-bands in the case of gases, and that in this case, as in the other, displacements of certain bands occur, while the position of others remains unaltered.

When, therefore, in sun-spots, certain iron lines suffer a displacement, and others in the same place do not, the cause is not motion, but the admixture of a foreign, strongly dispersive gas, which acts on the displaced lines and not on the others. It follows from this, further, that curvatures of absorption lines of the sun-spots need not by any means be always explained as due to motion of the absorbing gases in the direction of the line of observation, but only where all lines of a matter participate in the curvature.

That bright lines of a luminous gas, also, in like circumstances, "by admixture of another non-luminous vapour, or one giving a continuous spectrum," may suffer a displacement, Kundt has already shown.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

MARCUS M. HARTOG, D.Sc., M.A., F.L.S., has been appointed to the Professorship of Natural History at Queen's College, Cork, vacant by the death of Prof. Leith Adams.

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[1] A paper by Herr Hermann W. Vogel, read to the Berlin Academy on November 2, 1882. Communicated by the author

[2] *Proc Roy Soc.*, xxviii. 157.

[3] *Monatsb der Berliner Acad. der Wiss.*, 1880, p. 192.

[4] *Comptes Rendus*, t. xcii. 904.

[5] *Proc. Roy. Soc.*, 30, 93. *Wied. Beibl.*, iv. 366.

[1] Herr Vogel quotes a translation in *Naturforscher.*

[2] Kundt, *Jubelband Pogg. Ann.*, *p.* 620

[3] Vogel, "Pract. Spectralanalyse." Nordlingen bei Beck. P. 248.

[4] *Monatsb. der Akad. der Wiss.* of May 20, 1878.

[5] Kundt formerly doubted also the possibility of proof of an anomalous dispersion in gases and glowing vapours. Recently, however, he has succeeded in getting such proof in the case of sodium vapours (*Wied. Ann.* 10, p. 321).

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 14, 1882.—On the genus *Meliola*, by H. Marshall Ward, B.A., Fellow of Owens College, Manchester. The author has examined the life-history and structure of several species of these epiphytic fungi. The fungus consists of a much-branched mycelium, on which appendages and fruit-bodies occur. The *hyphæ* constituting the *mycelium*, consist of cylindrical cells, with hardened, brown cell-walls and protoplasmic contents; these are attached to the epidermis of the leaves, &c., of tropical plants by rudimentary *haustoria*, which do not pierce the cell-walls of the host, but are firmly adherent to the cuticle. The appendages consist of simple or branched setaceous outgrowths, which spring from the mycelium at various points, and are especially developed around the fruit-bodies from masses of hyphæ, which Bornet considered as forming a special part of the fungus, under the name of "receptacle"; these setæ cannot be considered as subserving any special function, however, and are certainly not tubes for the outlet of spores, as earlier observers have surmised. Other appendages occur in the form of small ovoid or flask-shaped lateral branchlets; some of these become free and subserve vegetative reproduction as *conidia*. The fruit-body, or *perithecium*, arises by continuous development of one of the pyriform lateral branchlets, and the author has studied its development very particularly. The short, ovoid, unicellular branchlet, after becoming separated from the parent hypha by a septum, suffers division into two cells by a septum running obliquely across it; of these two cells one produces the outer walls of the *perithecium*, by continuous cell-multiplication, whilst the other contributes the central portion, or *ascogonium*, by slower division of its contents.

The former cell, dividing up more rapidly, produces a layer of cells which envelope the latter by a process of "epiboly," and the outer cell-walls become hard, thick, and dark-coloured. The latter—ascogenous cell—divides up more slowly into a "core" of thin-walled cells, very rich in protoplasm. After complete envelopment, the cells of the "core" are recognised in vertical sections; certain of them become elongated, and form the earliest *asci*, while others become absorbed—together with inner cells of the enveloping layer—to provide nutritive material for the developing *asci* and their progeny.

The *asci* are produced successively, and are delicate clavate sacs, containing two to eight *spores*, each spore being divided by one, two, or three cross septa. The germination of the *spore* is also described and figured; it throws out an irregular germinal tube, which soon forms rudimentary *haustoria*, and grows forth as a mycelium, similar to that from which the *perithecium* was produced.

The author examines and criticises the views held by Bornet and Fries as to the systematic position of *Meliolæ*; especially the opinion that they are to be considered as tropical representatives of the European *Erysipheæ*. He shows that the original cell from which the *perithecium* arises must be regarded as containing in itself the undifferentiated elements of an *Archecarpium* and *Antheridium*-branch (in the sense of De Bary and others), and that after the primary division into two cells, we must look upon one of these—the one which becomes more rapidly divided up—as the homologue of the *antheridium*-branch and enveloping tissues of the *Erysiphæ*; the more slowly divided cell—which produces the *ascogenous* core—being the equivalent of the *ascogonium*, &c., of those fungi. The details of successive phases of development are amply illustrated by figures and many peculiarities acquired by the group are carefully examined and described.

The author concludes that the *Meliolas* must be looked upon as a group developed along similar lines to those of the *Erysipheæ*, *Eurotium*, &c., but in which the sexual process has suffered still greater reduction or withdrawal, leading to those forms in which it is entirely suppressed.

With respect to the injurious action of these fungi on their hosts, the author decides that no direct parasitic action on the cell-contents takes place, but that injury results indirectly on account of the dense black *mycelium*, when strongly developed, depriving the leaves of air, light, &c.

Chemical Society, December 21, 1882.—Dr. Gilbert, president, in the chair.—The following papers were read:—On the condensation products of oenanthol (part ii.), by W. H. Perkin, jun. The author has studied the action of nascent hydrogen upon oenanthol; when this substance is dissolved in acetic acid and acted upon by sodium amalgam, heptylic alcohol is produced; also an aldehyde, $C_{14}H_{26}O$, and an alcohol, $C_{14}H_{28}O$; if the oenanthol is dissolved in ether, heptylic alcohol, a solid aldehyde melting at 29°·5 ($C_{14}H_{28}O$), and a second substance, $C_{21}H_{40}O$, are formed. By oxidising the aldehyde $C_{14}H_{28}O$ with silver oxide, a small quantity of an acid, $C_{14}H_{28}O_2$, boiling at 300°–310°, was obtained. The author has also studied the action of nascent hydrogen upon the aldehyde $C_{14}H_{28}O$, and discusses the constitution of these new bodies.—On the behaviour of the nitrogen of coal during destructive distillation; with some observations on the estimation of nitrogen in coal and coke, by W. Foster. It is usually stated in text-books that coal contains about 2 per cent of nitrogen, which, during destructive distillation of the coal, comes off as ammonia. The author finds that this statement is not true. A Durham coal was used, containing 1·73 per cent. of nitrogen, and giving 74·5 per cent. of coke. If the total quantity of nitrogen in the coal be taken as 100, that evolved as ammonia is only 14·5 per cent.; as cyanogen, 1·56 per cent; as nitrogen in the coal-gas, 35·26 per cent.; left behind in the coke, 48·68 per cent.—On the absorption of weak reagents by cotton, silk, and wool, by E. J. Mills and J. Takamine. The reagents are sulphuric and hydrochloric acids, and caustic soda. This paper chiefly contains tables, with results calculated to five places of decimals.—On brucine, by W. A. Shenstone. Various observers have stated that brucine, when treated with dilute nitric acid, yields either methyl or ethyl, nitrate or nitrite. The author has studied the action of hydrochloric acid upon brucine quantitatively, and has proved that more than one molecule of methyl chloride is evolved from one molecule of brucine; he concludes that brucine is strychnia, in which two atoms of hydrogen are replaced by two methoxyl groups, and its formula may be written, $C_{21}H_{20}(CH_3O)_2N_2O_2$.—Researches on the induline group, by O. N. Witt and E. G. P. Thomas. "Induline" is a term applied to all coloured compounds formed by the action of amidoazo compounds on the hydrochlorides of aromatic amines with elimination of ammonia. The authors have studied in the present paper the formation of amidoazobenzene, and its action on aromatic hydrochlorides, and especially on anilin hydrochloride.—Preliminary note on some diazo derivatives of nitrobenzylcyanide, by W. H. Perkin.

Meteorological Society, December 20, 1882.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—Three new Fellows were elected, and Capt. J. de Brito Capello and Mr. W. Ferrel, M.A., were elected honorary members.—The following papers were read:—Popular weather prognostics, by the Hon. R. Abercomby, F.M.S., and Mr. W. Marriott, F.M.S. The authors explain over 100 prognostics, by showing that they make their appearance in definite positions relative to the areas of high and low atmospheric pressure shown in synoptic charts. The method adopted not only explains many which have not hitherto been accounted for, but enables the failure, as well as the success, of any prognostic to be traced by following the history of the weather of the day on a synoptic chart. The forms discussed are:—cyclones, anticyclones, wedge-shaped and straight isobars. The weather in the last two is now described for the first time. They also point out (1) that prognostics will never be superseded for use at sea, and other solitary situations; and (2) that prognostics can be usefully combined with charts in synoptic forecasting, especially in certain classes of showers and thunderstorms which do not affect the reading of the barometer.—Report on the phenological observations for the year 1882, by the Rev. T. A. Preston, M.A., F.M.S. The most important feature of the phenological year was the mild winter. The effect of this upon vegetation was decidedly favourable; and had it not been for the gales—especially that of April 28—the foliage would have been luxuriant, and therefore free from insect attacks, but the contrary effect has been produced on insect life, for the scarcity of insects, especially butterflies and moths, has been the general remark of entomologists.—Mr. J. S. Dyason, F.R.G.S., exhibited a series of typical clouds in monochrone, and also a series of sketches of clouds in colour, made in June, July, and August, 1882.

MANCHESTER

Literary and Philosophical Society—Microscopical and Natural History Section, December 12.—Prof. Roscoe in the chair.—Mr. James Heelis made some remarks upon the causes of the movement of the old Rhone Glacier with special reference to the power of gravity to produce such movement when considered in connection with the gradient down which the glacier has passed.—Prof. Osborne Reynolds, F.R.S., communicated and explained an elementary solution of the dynamical problem of isochronous vibration.—Mr. John Boyd exhibited a fine living specimen of *Argulus foliaceus*, a parasite of the carp.—Mr. Charles Bailey, F.L.S., made some remarks on the occurrence of *Selinum carvifolia* in Lincolnshire, and of *Potamogeton zizii* in Lancashire and Westmoreland, and mentioned the localities where he had met with them respectively.—Mr. R. D. Darbishire, F.G.S., gave an account of dredgings made by him in company with Dr. A. M. Marshall and Mr. Archer at Oban in September last, and exhibited specimens of a considerable variety of animals taken.—Prof. A. M. Marshall, M.A., gave a detailed description of three forms of Pennatulida met with during the dredging, and suggested the desirability of the section undertaking or taking part in similar excursions in future years.

DUBLIN

Royal Society, November 20, 1882.—Sections I. and III. Physical and Experimental Science, and Applied Science.—Rev. Gerald Molloy, D.D., in the chair.—The following communications were received:—Rev. H. M. Close, M.A., on the definition of force as the cause of motion, with some of the inconveniences connected therewith.—G. Johnston Stoney, D.Sc., F.R.S., and G. Gerald Stoney, on the energy expended in propelling a bycicle, parts 2 and 3.—Prof. W. F. Barrett, F.R.S.E., physical apparatus for class-teaching.—A. H. Curtis, LL.D., improved apparatus for exhibiting double reflection in the interior of a crystal.—Prof. G. F. Fitzgerald, F.T.C.D., recent advances in physical science, an account of Prof. Rowland's curved gratings for spectrum analysis.—Prof. Fitzgerald exhibited photographs of the solar spectrum taken by Prof. Rowland.

Section II. Natural Science.—Rev. A. H. Close, M.A., in the chair.—The following communications were received:—Prof. V. Ball, M.A., F.R.S., on some effects produced by landslips and movements of the soil cap, and their resemblance to phenomena which are generally attributed to other agencies.—Prof. A. C. Haddon, M.A., exhibition of marine invertebrates, belonging to the Natural History Museum, prepared at the Zoological Station, Naples, with remarks upon the various methods for the preparation of zoological specimens.—G. A. Kinahan, on the geology of Bray Head.

December 18, 1882.—Sections I. and III. Physical and Experimental Science, and Applied Science.—A. H. Curtis, LL.D., in the chair.—The following communications were received:—G. F. Fitzgerald, F.T.C.D., on Dr. Eddy's hypothesis that radiant heat is an exception to the second law of thermodynamics.—Communicated by Howard Grubb, M.E., F.R.A.S.: (*a*) Notes on the transit of Venus, as observed at Armagh Observatory by Dr. Dreyer; (*b*) Notes on the transit of Venus, as observed at Cork Observatory by Prof. England; (*c*) Notes of the transit of Venus, as observed at Rathowen, Co. Westmeath, by Mr. W. E. Wilson.—G. Johnstone Stoney, D.Sc., F.R.S., on means of neutralising echoes in rooms.—G. Johnstone Stoney and G. Gerald Stoney, on geared bicycles and tricycles.—Dr. Otto Bœddicker, on the influence of magnetism on the rate of a chronometer (communicated by the Right Hon. the Earl of Rosse, F.R.S.)—Mr. Grubb informed the Society that Dr. Huggins had authorised him to announce that he had succeeded in photographing the corona of the uneclipsed sun by employing absorbing media.

PARIS

Academy of Sciences, December 18.—M. Jamin in the chair.—The following papers were read:—On a recent memoir, by M. Wolf, of Zurich, on the periodicity of sun-spots, by M. Faye. From further careful study (by a method described) of data for the last 120 years, M. Wolf concludes (1) that there is a period of 10 years; also (2) a period of 11 years, 4 months; and (3) that there is not a period of 12 years, imputable to the action of Jupiter. Spite of the great difference of the two periods, the interval between a minimum and the next maximum is the same in both, viz. 4½ years. After 170 years the phenomena recur in the same order, and with the same numerical values. M. Faye added some remarks by way of theory.—Statistics of preventive vaccination against *charbon* relating to 85,000 animals, by M. Pasteur. The figures (for Eure-et-Loire, where the ravages have been worst) show a marked reduction of the mortality from *charbon*; thus, of the 80,000 sheep vaccinated, only 0·65 per cent. died, whereas the average mortality of the past 10 years is 9·01 per cent.—Contribution to the study of rabies, by M. Bert. He gives results published a few years ago, but little known. *Inter alia*, inoculation with mucus from the respiratory passages of a mad dog caused rabies, but that with the salivary liquids did not. Reciprocal transfusion of blood between a healthy and a mad dog did not cause rabies in the former. The slaver of a mad dog, after filtering through plaster, was harmless, but the portion caught on the plaster caused rabies (which is thus probably due to a microbe).—On the functions of seven letters, by M. Brioschi.—Experiments with a new arrangement of the automotor elevating apparatus with oscillating tube, by M. de Caligny.—M. Faye presented the second and last volume of his "Cours d'Astronomie."—M. de Quatrefages announced the formation of a committee, headed by M. Milne-Edwards (who is now convalescent) for a monument to Darwin, as proposed in England.—On maize at different periods of its vegetation (continued), by M. Leplay.—M. Ladureau (in a memoir) stated that he has found, on an average, 1·80 cc. of sulphurous acid (free and combined) per cubic metre of air in the atmosphere of Lille.—The Secretary called attention to a new work on Galileo, by Signor Favaro, asked to be informed of any documents relative to Fermat (whose works are to be published by the Minister of Public Instruction), and read some telegrams on the transit of Venus.—Observations of the transit of Venus at Algiers Observatory, by M. Trépied. Bad weather marred the work. The spectrum, and photographs taken in the green, blue, and violet, showed no absorption by an atmosphere round Venus.—On the transit as observed at Rome, by M. Millosevich. He thinks the spectroscopic method the only one capable of giving good results, which admit of being tested, for the first contact.—On the great southern comet, as observed at the Imperial Observatory of Rio de Janeiro, by M. Cruls. On October 15 there were two nuclei, and he thinks the appearance of the tail due to two tails (corresponding to the nuclei).—On solar photometry, by M. Crova. By a method described, and by Bougner's method, he arrives at about 60,000 carcels for the intensity of the solar light on a clear day (at Montpellier), an estimate ten times those of Bougner and Wollaston.—Reply to M. Ledieu, &c., by M. Decharme.—On the sensation of white and complementary colours, by M. Rosenstiehl. The introduction of a coloured object in an illumination apparently homogeneous and colourless, at once shows the real lack of homogeneity in the combination of lights. There is often confusion between mixture of lights and mixture of sensations.—Researches on the duration of solidification of surfused substances, by M. Gernez. He worked with U tubes holding phosphorus. The course of the phenomenon is uniform. Previous heating of the phosphorus to different temperatures did not sensibly affect the velocity of solidification. M. Gernez studies the curve for velocity of solidification at different temperatures (43°·8 to 24°·9).—On the measurement of pressures developed in a closed vessel by explosive gaseous mixtures, by M. Vieille. The method (described) was to register the law of displacement of a piston of known section and mass; (results shortly).—On the crystallisation of hydrate of chlorine, by M. Ditte.—On chloride of pyrosulphuryl, by M. Konovaloff.—On the products of distillation of colophanry, by M. Renard.—Production of surgical anæsthesia, by combined action of protoxide of nitrogen and chloroform, by M. de Saint Martin. With protoxide of nitrogen (85 vol.), and oxygen (15 vol.) M. Bert got anæsthesia by operating under pressure. If 6 or 7 gr. chloroform be added per hectolitre, the effect is had quickly at ordinary pressure.—Passage of the bacterium of *charbon* from mother to fœtus, by MM. Strauss and Chamberland.—Physiological properties of oxethylquinoleine-ammonia, by M. Bochefontaine. Like curare, it prevents passage of excitation from nerve to muscle, but, unlike curare, it makes the heart beat more slowly.—Experimental researches on spontaneous contractions of the uterus in certain mammalia, by M. Dembo.—On the formation of embryonal layers in the trout, by M. Hennegny.—Remarks on M. Lichtenstein's paper on pucerons, by M. Balbani.—Orographic note on the region of the Jura between Geneva and Poligny, by M. Bourgeat.—On a phenomenon of molecular mechanics, by M. Trève. He covers the tops of ivory balls, hung in a row, with metallic powder; when one end ball (say the left) is drawn back and let fall on its neighbour, the powder on the right half of the balls is thrown in the direction of the shock; but that on the last ball is thrown from the side opposite to the direction of the shock.

December 26.—M. Jamin in the chair.—The following papers were read:—Observations of the transit of Venus at the Naval Observatory of Toulon, by M. Mouchez. M. Rozet observed the black drop at second contact.—On two objections of Prof. Young of New Jersey, to the cyclonic theory of sunspots, by M. Faye. These are, the absence of visible traces of rotation in most spots, and the small difference of angular velocity in successive zones of the photosphere. M. Faye holds the unequal velocity sufficient to cause vortical movements of any calibre; and the general absence of agitation at the border of spots he attributes to the slowness of gyration there (our cyclones seen from above would show the same). He cites a number of positive indices of gyration.—Theory of the resistance of woven materials to extension, by M. Tresca. Such stuffs suffer elongations which increase less rapidly than the weights; and with equal weight, they show much greater elongation than those of the warp-threads composing them. The mode of interlacing of the threads explains these differences.—On the necessity of introducing certain modifications into the teaching of mechanics, and of banishing certain problems; *e.g.* the motion of the solid body of geometers, by M. Villarceau.—Considerations on the general theory of units, by M. Ledieu.—Separation of gallium (continued), by M. Lecoq-de-Boisbaudran. — Herr Bunsen was elected Foreign Associate in room of Wöhler, deceased.—Chemical studies on maize, &c. (continued), by M. Leplay.—Evolution of microscopic organisms in the living being, and in the dead-body and morbid products, by M. Colin. Microbes are nowhere absent in the respiratory and digestive apparatus, and at many points they are prodigiously numerous. In normal conditions the liquids holding them are harmless, but they become dangerous after putrid alteration.—The first number of a new mathematical journal, *Acta Mathematica*, published at Stockholm (M. Mittag-Leffler, editor), was presented.—A telegram from Montevideo announced success of the transit observations at Santa Cruz.—Observation of the transit of Venus at Nice Observatory, by M. Mouchez. Five photographs were had, under difficult conditions.—Observation of the transit at Avila (Spain), by M. Thollon. They sought to observe Venus's atmosphere spectroscopically at a height of 1200 m., but bad weather prevented their getting any satisfactory results.—Photographs of the great comet of 1882 taken at the Observatory of the Cape of Good Hope, by Mr. Gill. Spite of long exposure (140 minutes for the sixth and last photograph), the stars at the centre of the image are remarkably distinct. More than fifty stars are seen through the tail. Mr. Gill does not doubt that stellar maps might be produced by direct photography of the heavens.—On the formula recently communicated to the Academy regarding prime numbers, by M. de Jonquières.—On the same, by M. Lipschitz.—Reply to a recent note by M. Lalanne on the verification and use of magnetic maps, by M. de Tillo.—Electrodynamic method for determination of the ohm; experimental measurement of the constant of a long coil, by M. Lippmann.—Measurement of the photometric intensity of spectral lines of hydrogen, by M. Lagarde. The curves from the values obtained show the inequality of intensity of the three lines, inequality variable with the induced discharge. With diminutiou of pressure, the curve straightens; at 6·5 mm:, that for red is a straight line.—On the instantaneous pressure produced during combustion of gaseous mixtures, by MM. Mallard and Le Chatelier. With mixtures of H and O, the interior pressure exceeded by more than 2 atm. that corresponding really to the temperature of combustion; and this occurred in less than a ten-thousandth of a second. An explanation is offered.—On bisulphhydrate of ammonia, by M. Isambert.—On a case of physical isomerism of monochlorinated camphor, by M. Cazeneuve.—Biological researches on beet, by M. Corenwinder.—On the reduction of sulphates by sulphuraria, and on the formation of natural metallic sulphates, by M. Plauchud.—On the transformation of nitrates into nitrites, by MM. Gayon and Dupetit. They have isolated four distinct microbes capable of the action; one can live in chicken broth even when this is saturated with nitrate of potash, and decompose 10 gr. of the nitrate per litre daily. The microbes of chicken cholera, the bacterium of charbon, and the septic vibrion, effect denitrification much less easily.—On the poisonous principles of edible fungi, by M. Dupetit. Injecting, subcutaneously, the fresh juice of several such fungi into rabbits, &c., he observed symptoms of poisoning, followed by death. The chemical properties of the active principle recall those of soluble ferments, rather than of known alkaloids. A temperature of 100° renders the juice harmless.—Researches on the production of a general anæsthesia or, a specially unilateral anæsthesia by a simple peripheric excitation, by M. Brown-Séquard. Irritation of the laryngeal mucous membrane with a current of carbonic acid will produce anæsthesia in all parts of the body, without passage of this gas into the blood.—On the physiological action of coffee, by M. Guimaraeo. The experiments (made on dogs at Rio) prove that coffee is at once a stimulant and a repairer. By permitting a greater expenditure and consumption of argotised substances, it evidently increases the power of work.—On the structure of cells of the mucous bodies of Malpighi, by M. Ranvier.—On the fœtal envelopes of Chiroptera of the family of Phyllostomides, by M. Robin.—On an usteria from great depths of the Atlantic, provided with a dorsal peduncle, by M. Perrier. This is a "find" of the *Travailleur* cruse, and is named *caulaster pedunculus*.—On the suctocitiates of M. de Merejkovsky, by M. Maupas. The type deseribed, he says, has been long known.—Mineralogical analysis of the rock impasted in the meteorite of Atacama, by M. Meunier.

BERLIN

Physical Society, December 15, 1882.—Prof. Helmholtz in the chair.—Prof. Christiani demonstrated some acoustic experiments which he had incidentally made. In renovation of the Kœnig tuning-forks injured by the fire in the Physiological Institute, and which had to be freed from their coating of rust, and mounted on new resonance cases, one fork of the series, the fork mi_3, showed after tuning and sounding, when one side of it was turned towards the closed end of the case, a maximum of tone; it did not matter in which direction (right or left) the fork was turned round into the position referred to. Another fork mi_3 of the physical Institute in unison with the first, did not present the phenomenon, and when the forks and cases were exchanged, it appeared that the effect was connected with the new case. It was not explained. A second experiment, made by Prof. Christiani, was named by him "total absorption of tone." A singing flame was tuned approximately to the tone mi_3, and the resonance case bearing the tuning-fork mi_3 was held with its open end horizontal near the upper end of the chemical harmonica. The tone was unaffected. When, however, the same case, without tuning-fork, was brought to the same position relatively to the sounding chemical harmonica, the sound immediately ceased, and the flame burned quietly in the tube. Each time the tone of the flame ceased, when the mouth of a resonator adapted to the pitch was brought to the upper end of the tube, whereas the flame sounded again when the resonator was tuned to a different tone, or was loaded with a tuning-fork. Prof. Christiani means to investigate the phenomenon further.

CONTENTS

PAGE
AUGUSTUS DE MORGAN. By R. TUCKER 217
FISHES OF SWITZERLAND 220
OUR BOOK SHELF:—
Douglas's "China" 221
LETTERS TO THE EDITOR:—
On the Occurrence of Great Tides since the Commencement of the Geological Epoch.—J. G. GRENFELL 222
Sir George Airy on the Forth Bridge.—B. BAKER 222
Altitude and Weather.—Dr. A. WOEIKOFF 223
The Fertilisation of the Speedwell.—ARTHUR RANSOM 223
THE SACRED TREE OF KUM-BUM. By W. T. THISELTON DYER, F.R.S., C.M.G. 224
NORWEGIAN GEODETICAL OPERATIONS 224
ELEMENTS OF THE GREAT COMET OF 1882. By Prof. E. FRISBY . . 226
THE DUMAS MEDAL (*With Illustrations*) 227
PROFESSOR VON GRAFF'S MONOGRAPH ON THE TURBELLARIANS. By Prof. H. N. MOSELEY, F.R.S. 227
NOTES 228
BIOLOGICAL NOTES:—
On a New Genus of Cryptophyceæ 230
Female Flowers in Coniferæ 231
The Tracheæ in Lampyridæ 231
The Stones of Sarepta (Asiatic Russia) 231
AMERICAN RESEARCHES ON WATER ANALYSIS 231
LOCKYER'S DISSOCIATION-THEORY. By Herr HERMANN W. VOGEL . 233
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 23
SOCIETIES AND ACADEMIES 234

THURSDAY, JANUARY 11, 1883

GEIKIE'S GEOLOGY

Geological Sketches at Home and Abroad. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London and New York: Macmillan and Co., 1882.)

Text-Book of Geology. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London: Macmillan and Co., 1882.)

THESE two works, by the same author, are presented to the public at nearly the same time, but there is no other reason why they should be described together. The first is a collection of short papers, each presenting some matter of personal observation or some contribution to geological philosophy. The second exhibits the science of geology in a systematic way, and of necessity deals chiefly with the results of the work of others. The first is addressed to the general reader, and in part to the geologist; the second is addressed specifically to the student.

The sketches of the first volume are not new, but are here collected for the first time. Several of them received their first publication as magazine articles, others have been presented to scientific societies, and a few have taken the form of lectures. They constitute but a small portion of the author's voluminous contributions to scientific literature, and have evidently been selected because of their popular interest. A few are addressed to the popular audience only, and merely present some of the elements of stratigraphical and dynamical geology, with familiar Scottish scenes as texts; but the majority embody original contributions to knowledge, couched in so simple language that the layman reads them without being fully aware that they belong to the frontier of geological thought. Prof. Geikie possesses the happy faculty of addressing himself simultaneously to a professional and an unprofessional audience in such way that the former do not find his science too dilute nor the latter too condensed.

One of the sketches describes a journey to central France, undertaken for the purpose of studying the extinct volcanoes of that region as an aid to the imagination in restoring the condition of Scotland during the Carboniferous period; and another describes a journey to Norway with the parallel purpose of rendering vivid the mental restoration of Scotland in Glacial times. These two are perhaps the most instructive of the collection, for besides making definite additions to the geological history of Scotland, they present admirable illustrations of one of the most valuable methods of scientific investigation. The principles which distinguish modern scientific research are not easily communicated by precept, and it is by no means certain that they have yet been correctly formulated. However it may be in the future it is certain that in the past they have been imparted, and for the present they must be imparted, from master to pupil chiefly by example; and whoever in publishing the result of a scientific inquiry sets forth at the same time the process by which it was attained, contributes doubly to the cause of science.

Two chapters are devoted to a journey in the United States; a journey undertaken, like the others, for a definite purpose—that of enabling the author to see with his own eyes the monuments of erosion for which the Rocky Mountain region is so illustrious. His account deals also with a variety of geological topics, as well as with the peculiar aspects of American frontier life. He describes the geysers of the Yellowstone country, some of the extinct glaciers of the head-waters of the Missouri, the parallel shore-lines of the great extinct lake of Utah, and the great lava field of the Snake River Plain. In another chapter he appears as the apostle of massive eruptions, first recognised by Richthofen, and afterwards by many American geologists, but so foreign to European experience, that the accounts of them had seemed to many English geologists to border on the marvellous, and had even thrown discredit upon American science.

Perhaps the most important paper of all is that upon geographical evolution. It was originally read to the Royal Geographical Society, and has received in various ways so wide a publication, that it is probably accessible already to the majority of the readers of NATURE. The lecture on the weathering of rocks, as illustrated by tombstones, is also included, and a lecture on the geological influences which have affected the course of British history.

In the whole collection there is nothing polemic, nor anything that could even be called controversial. Attention is never directed to an error, except as the merest incident to pointing out that which is true. No words are given to the censure of others, but many to their praise, and one of the chapters has for its theme a eulogy on the work of the early Scottish school of geology.

The style is peculiarly genial and entertaining—a merit unfortunately rare in the writings of modern geologists; but accuracy of statement is not sacrificed to vivacity. As in all his writings, there is nothing sensational, either in description or in speculation. His inductions are not expanded into brilliant, universal theories, but are modestly advanced with all those limitations which impress themselves on the mind of one who constantly questions nature.

Turning now to the text-book, we come to consider a work of greater importance, and one especially deserving of careful criticism by reason of its relation to education. The text-books of this generation must furnish to the geologists of the next their fundamental principles, so that those who prepare them and those who commend them are responsible, not merely to the youth of to-day, but to the science of the future.

There are four features in regard to which a work designed for geological instruction should be scrutinised: Its scope, the arrangement of its matter, the quality of its matter, the manner of presentation.

In the scope of geological text-books, on the range of subjects considered and the relative space allotted to each, there has been a progressive development, parallel with and dependent upon the evolution of geology and cognate sciences. Our knowledge of the earth's history is so dependent upon and interwoven with other departments of knowledge, that a clear presentation of it cannot be made without either reciting the elements of other sciences or assuming them to be known. In the early history of the subject, when the volume of geological material was small, and when the elements of zoology, botany, and chemistry were not so widely diffused as now, it appeared to most writers necessary to devote some space, either in a prefatory or in an incidental way, to these sciences. Mineralogy and palæontology, growing with the growth of geology, were likewise treated with it. But owing to the rapid development of geology, its own subject matter has now become so voluminous that it can only with difficulty be outlined in the compass of a text-book, and step by step it has displaced everything of which a sufficient knowledge could be assumed.

While mineralogy and palæontology have by their growth become more and more differentiated from geology, astronomy has been affiliated in a degree that was not anticipated. Previous to the revelations of the spectroscope, our earth was regarded indeed in origin, composition, and career as analogous to other planets, but only in a hypothetic and speculative way; but now that there is a large body of evidence pointing to identity of composition throughout the solar system, there is no longer any question of a common history, and every advance in celestial physics is now regarded as a contribution to the early history of the earth. A department of astronomical geology has thus arisen.

In the work under consideration no space whatever is permitted to zoology and botany; chemistry is barely mentioned; mineralogy (chiefly descriptive) is accorded only 25 pages; palæontology proper is omitted, but 28 pages are devoted to the principles of palæontological geology—a department of science clearly distinguishable if not distinct from palæontology, and inseparable from stratigraphy; mythological cosmogony is not even mentioned, but the space it has too often occupied is given to physiographical geology—a discussion of the origin of the physical features of the land. Astronomical geology is accorded 23 pages. The bulk of the volume—570 pages out of 910—is devoted to geognosy, and dynamical and structural geology that is, to rocks and rock structures, and to the physical changes whereby rocks originate. Stratigraphy, which until very recently has arrogated the lion's share of space, is here reduced to less than one-third of the total.

The distribution of space thus outlined is eminently judicious, and it may be doubted whether any could be better adapted to the present status of the science and the present demands of instruction. If it has a fault it is in the amount it concedes to the demands of the geologist in the matter of stratigraphy. The student's text-book has not yet been clearly differentiated from the geologist's handbook, and there is certainly an open field to-day for a manual especially adapted to the use of the working geologist, and not primarily arranged for instruction. All of the larger text-books have been partially adjusted to this need, and Prof. Geikie's is not an exception; but in his work the adjustment appears only in the stratigraphical chapter, which embodies a mass of detail that can serve only to bewilder if the student undertakes to master it. If the 275 pages of descriptive stratigraphy were reduced to 50, and a portion of the space thus saved were devoted to a rapid review of the salient points of the geological history of some limited region, as Great Britain, for example, I am prone to believe that the student would be afforded a better insight into the aims and results of geological inquiry.

In the classification and arrangement of the subject-matter of geological text-books, there has been as marked a development as in the scope. The number of different manners in which a congeries of allied topics can be grouped is practically limitless, for the bases of possible groupings are as numerous as the relations sustained by the topics; but not all classifications are of equal utility, and at each stage in the progress of a science there is usually some one which commends itself as of superior advantage. As, in the progress of knowledge, new relations are discovered, and the importance of relations previously known comes to be differently estimated, new classifications are adopted, in comparison with which the old appear crude. Geology is so young a science, that a single generation has witnessed a complete revolution in this regard. The primary classifications of the modern text-books have nothing in common with the earlier editions of Lyell's manual. In the division and arrangement adopted by Geikie, only a single feature is original, but the order of presentation as a whole is new.

The theme of geology is the history of the earth. In its study there are two lines of inquiry, which are so nearly independent that they form co-ordinate branches of the general theme: the one is cosmic, the other terrestrial.

Cosmically considered, the earth is one of a group of worlds believed to have a common origin, and to be pursuing parallel courses of development, in which they have reached various different stages. Assuming this to be true, the less developed worlds present phases, through which the earth has already passed, and by studying them we may learn something of the youth of our planet.

The terrestrial branch of inquiry is concerned chiefly with rocks. The changes of the crust have led to the formation of rocks, and have given to them great variety of composition and structure. It is known, moreover, that rock formation is still in progress, and that agencies whose operations can be witnessed are now forming many varieties of rocks, and are initiating many peculiarities of rock structure. It is possible, therefore, to associate certain rocks and rock structures with certain processes of change, and by this means to derive from a study of the rocks of the crust a history of the changes which led to their formation. This inquiry is greatly facilitated by the fact that rocks have been partly formed from animal and vegetable remains, and by the additional fact that there has been a progressive development of life; so that, the key once obtained, the chronological order of rocks can be deduced from their organic contents.

In presenting the second line of inquiry as to the earth's history it is therefore proper to treat: of the composition of rocks and other materials of the earth's crust (geognosy); of the forms in which rocks are aggregated, or the structure of rock masses (structural geology); of the agencies which in modern times are observed to produce changes of the earth's crust (dynamical geology); of the relation of organic remains to geological formations (palæontological geology); and finally, of the actual order in which the various kinds and groups of rocks succeed each other, and the deduced series of changes the earth has undergone (stratigraphical or historical geology). The first and last of these categories claim their respective positions without question: geognosy constitutes the alphabet of the subject, and must precede all else, while stratigraphical geology depends upon all the other divisions, and must follow them. Palæontological geology is in some sense co-ordinate with dynamical and structural geology taken together, but finds place after them because its use cannot be explained before their principles are known. Whether dynamical geology should precede or follow structural, is a question admitting of discussion. They are to a large extent correlatives, and either is more intelligible if preceded by the other. To give precedence to structural geology is to describe phenomena in advance of their explanation. If dynamical geology precedes, a variety of natural agents are described which have no apparent connection with the general subject. The majority of writers have selected the former alternative; but a few have preferred the latter, and among them our author. All things considered, he appears to have chosen the lesser evil.

The single new departure of the volume consists in the elevation of physiographical geology to the rank of a major division. The same title it is true has been placed by Dana at the head of a primary division of the subject, but it was used by him in a different sense. With Dana it is a synonym for physical geography; with Geikie it is that "branch of geological inquiry which deals with the evolution of the existing contours of the dry land." So far as the subject has had place in earlier treatises it has been regarded as a subdivision of dynamical geology, and the classification which placed it there was certainly logical. In dynamical geology, as formulated by Geikie, the changes which have their origin beneath the surface of the earth (volcanic action, upheaval, and metamorphism), and the changes which belong exclusively to the surface (denudation and deposition) are separately treated. In physiographical geology the conjoint action of these factors of change is considered with reference to its topographical results. Starting from geological agencies as data we may proceed in one direction to the development of geological history, or in another direction to the explanation of terrestrial scenery and topography, and if the development of the earth's history is the peculiar theme of geology, it follows that the explanation of topography, or physiographical geology, is of the nature of an incidental result—a sort of corollary to dynamical geology. The systematic rank assigned to it by Geikie is an explicit recognition of what has long been implicitly admitted: that geology is concerned quite as really with the explanation of the existing features of the earth as with its past history. The separation initiated by our author is an indication of the growing importance of the subject, and it is safe to predict that in the future it will not merely retain its new position, but will even demand a larger share of space.

The following scheme exhibits the general plan of the volume:—

Book 1.—Cosmical aspects of geology.

Book 2.—Geognosy: an investigation of the materials of the earth's substance.

Book 3.—Dynamical geology.

Book 4.—Geotectonic geology; or the architecture of the earth's crust. (*Geotectonic* is a new term proposed as a substitute for *structural*).

Book 5.—Palæontological geology.

Book 6.—Stratigraphical geology.

Book 7.—Physiographical geology.

Comparing this classification with that of other authors, and viewing it with reference to the present condition of the science, we may say without hesitation that it has no superior, and that it is well adapted to existing needs.

G. K. Gilbert,
U.S. Geological Survey

(*To be continued.*)

OUR BOOK SHELF

Uniplanar Kinematics of Solids and Fluids; with Applications to the Distribution and Flow of Electricity. By George M. Minchin, M.A. Pp. viii. + 266. (Oxford: Clarendon Press, 1882.)

In subject-matter this book is almost unique among our mathematical manuals. The only fellow to it is Clifford's "Kinematic." It consists of six chapters, the first dealing with Displacement and Velocity, the second with Acceleration, the third with Epicycloidal Motion, the fourth with the Mass-Kinematics of Solids, the fifth with the Analysis of Small Strains, and the sixth almost as long as the others put together, with the Kinematics of Fluids. The subdivisions of the last chapter are headed —General Properties: Multiply Connected Spaces; Motions due to Sources and Vortices, Electrical Flow; Conjugate Functions. There is also a short appendix, with notes on such subjects as Vectors and their Derivatives, Current-Power, and Routh's Use of Conjugate Functions.

It is impossible, without occupying considerable space, to give an adequate idea of the freshness and originality which mark Prof. Minchin's work. These are notable in the exceedingly valuable sixth chapter, but even on such well-worn subjects as velocity and acceleration, he treats us to many pleasant little surprises. Nor is this accomplished at the expense of the student; the clearness, fulness, and good arrangement specially requisite in a college text-book are all of them conspicuous; and valuable collections of exercises, worked and unworked, and given at intervals. The book is altogether one for which success may be cordially wished, not merely as a reward to the author, but in order that the science of which he treats may go on as steadily and rapidly advancing as it has of recent years been doing.

Die Käfer Westfalens. Zusammengestelt von F. Westhoff. Abtheilung ii. (Supplement zu den Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westfalens, Jahrgang 38, pp. 141-323.) (Bonn, 1882.)

We have already noticed the first part of this work in Nature. The second and concluding portion is now before us. It forms one of the most useful local Beetle catalogues that we have seen, nicely printed (the names being in bold black type), with copious local and other information. The district comprises about 450 square (German) miles, and is varied in its physical conditions. In all, 3221 species are enumerated, in 59 families. The *Staphylinidæ* comprise 667 species, *Curculionidæ* 471, *Carabidæ* 321, *Chrysomelidæ* 265, and *Dytiscidæ* 115. All the other families have each less than 100 representatives, and 10 of them less than 5. The nomenclature followed is that of the newest "Stein-Weise" German list, which, as is well known, has introduced a great multitude of changes and innovations; but other generally received names are indicated in brackets, thus avoiding confusion. Westhoff describes no new species in Part ii., but indicates and names a good many new (chiefly colour) varieties. Probably the rage for naming colour-varieties, so wide-spread at the present day, should be deprecated. For instance, in this catalogue we find a list of 27 named varieties following the indication of *Coccinella* 10-*punctata*, L., and 6 or 8 analogous varieties are appended to many other species of Ladybirds. Taking it as a whole, this excellent catalogue may serve as a model for compilers of lists of the Beetle (or other entomological) fauna of other districts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Equal Temperament of the Scale

In your number of November 8, 1877, p. 34, Mr. Chappell, F.S.A., has intimated that mathematicians who propose to divide the octave into twelve equal semitones instead of "equally tempered semitones," are deficient in musical ear. I have not noticed that any mathematician has replied to him.

Representing (with Mr. Chappell) the number of vibrations in the C of my piano by 1, and the octave *c* therefore by 2, and dividing the octave into 12 equal intervals, I obtain for the vibration-numbers—

$C = 1\cdot$

$C\sharp = 1\cdot0594 = 2^{\frac{1}{12}}$

$D = 1\cdot1224 = 2^{\frac{2}{12}}$

$D\sharp = 1\cdot1892 = 2^{\frac{3}{12}}$

$E = 1\cdot2599 = 2^{\frac{4}{12}}$

$F = 1\cdot3348 = 2^{\frac{5}{12}}$

$F\sharp = 1\cdot4142 = 2^{\frac{6}{12}}$

$G = 1\cdot4983 = 2^{\frac{7}{12}}$

$G\sharp = 1\cdot5874 = 2^{\frac{8}{12}}$

$A = 1\cdot6818 = 2^{\frac{9}{12}}$

$B\flat = 1\cdot7818 = 2^{\frac{10}{12}}$

$B = 1\cdot8877 = 2^{\frac{11}{12}}$

$c = 2\cdot$

In these equal semitones each is equidistant from the preceding and following: as F is to F♯, so is F♯ to G, &c. Hence in whatever key I play a passage on my piano, the divergence from harmonic intervals will be alike at every point; the keys on my piano will have no distinctive character, the key of 3 sharps will not be more "brilliant" or less "plaintive" than that of 4 flats.

In the key of C, the harmonic third, fifth, and seventh will be, according to the above notation, 1·25, 1·5, and 1·75 respectively. As regards the fifth G it is a remarkable numerical coincidence that $2^{\frac{7}{12}}$ only differs from 1·5 by $\frac{1}{600}$, *i.e.* the equal temperament G only differs from the harmonic by its $\frac{1}{400}$ part, a difference so slight that it may be neglected. We tune fiddles by fifths therefore. This coincidence is the fundamental fact which enables us to modulate into various keys on a piano, and it is the reason why the scale must be divided into 12 (and not any other number of) semitones; for it will be found that, until you get to the unmanageably high number of 53, no other equal division of the scale has any note so near the harmonic G.

The crucial point of tempering arises on the third. The E of my piano is $2^{\frac{1}{3}} = \frac{126}{100}$, whereas the harmonic E is $= \frac{125}{100}$; my E is therefore by its $\frac{1}{80}$ part too sharp, *in the key of C*, a perceptible degree of error, unpleasant to many musicians. In ordinary pianoforte tuning, the E (by the plan in Hamilton's pianoforte tuner or some similar compromise) is tuned somewhere between $\frac{125}{100}$ and $\frac{126}{100}$, say $\frac{125\frac{1}{2}}{100}$, and the wolf between this E and the upper *c* is distributed.

This is all very simple so long as we remain in the key of C; indeed if we remain there, we want no tempering. But G♯ is the third to E, and *c* is the third to A♭; on the piano G♯ and A♭ are one. On my equal-semitone piano I have

$$c = 1;\ E = 2^{\frac{1}{3}}\ (= \tfrac{126}{100}\text{ nearly};$$
$$G\sharp = A\flat = 2^{\frac{2}{3}}\ (= \tfrac{157}{100}\text{ nearly});\ c = 2.$$

I now ask the champion of "equally tempered semitones" what is the numerical value of his E and what of his G♯. If he gives them any other values than $2^{\frac{1}{3}}$ and $2^{\frac{2}{3}}$ respectively, it is clear that a *greater* error will be introduced in one part of the scale than is saved in another. Instead of algebraic proof I take an instance—suppose that Mr. Chappell tunes his E at $\frac{125\frac{1}{2}}{100}$; if he equally tempers his G♯ in the scale of E, it will be $\left(\frac{125\frac{1}{2}}{100}\right)^2 = \frac{31}{20}$ very nearly. Then when he puts down the common chord in the key of A♭, his third the *c* will be by its $\frac{1}{32}$ part too sharp, whereas on my equal temperament piano it would only be by its $\frac{1}{80}$ part too sharp. In other words, though the keys of C and E may be somewhat better on Mr. Chappell's piano than on mine, the key of A♭ will be *very much* worse. This is pretty nearly what occurs in practice. The point of my argument is that Mr. Chappell cannot move his E ever so little from the value $2^{\frac{1}{3}}$ without introducing a greater error somewhere else. The term "equally tempered semitone" is inaccurate; the semitones on my piano are all equal; and no one of them can be altered by a disciple of the "equally-tempered semitone" without making them unequal. The "equally-tempered semitones" are *not* equally tempered. Moreover if you "temper" *at all* you lose the effect of the harmonics; by moving E from $\frac{125}{100}$ to $\frac{125\frac{1}{2}}{100}$ you sacrifice harmonic coincidence.

The simple reason that unequal tempering is practised is because all keys are not used equally often. A piano is unequally tempered so that the keys C, G, A, F are fair, E, B♭, E♭ tolerable, the other keys being very much worse than on my equal-semitone piano. On most church organs, being unequally-tempered, if you modulate even transiently into 4 or 5 flats, the effect is unendurable.

The crucial question in tuning is the question, if your E is not $2^{\frac{1}{3}}$ and your G♯ $2^{\frac{2}{3}}$, what values do you put them at? The question of the seventh is more complex; I may observe that though my equal-semitone seventh (1·7818) appears far away from the harmonic seventh (1·75), yet that the B♭ of tuners on the "equally-tempered semitone" system is not much nearer it. Their B♭ is $\frac{16}{9}$ or thereabout, or in other words, the sub-subdominant of C. Therefore, on the piano, you have not got the "harmonic-seventh" at all; the note which replaces it is one that suggests overpoweringly the key of F. This is the secret which underlies several of our rules in harmony. It is also the reason why valve-horn players play B♭ (though an open note) with valve *n.2*, or if they play without a key "lip it up" very carefully.

It is often supposed that the "wolf" has been introduced into music by that most useful though imperfect instrument the piano, and that the noble violin or human voice knows it not, except in so far as our natural good ear for harmonic intervals has been debauched by continually hearing tempered intervals. This is not so; the "wolf" is not only in the piano but in the scale. It is true that a violin can play in harmonic tune so long as the melody runs in one key, or if it modulates into a closely allied key, and *back again the same way*. But suppose my violin begins by rising from C to E harmonically, *i.e.* to $\frac{125}{100}$; then after playing awhile there proceeds to G♯ $(\frac{125}{100})^2$ harmonically, being then in 8 sharps; and then, after playing awhile in 8 sharps, proceeds to *c*; the *c* of the fiddle will then be $(\frac{125}{100})^3$ instead of 2; *i.e.* it will be $\frac{3}{128}$ out of tune. In this simple case the fiddle is supposed to play alone, unfettered by any harmonics but its own; in the case of a string-band, the agreeableness of many modulations actually depends upon some chords being harmonically out of tune, the note in the chord which performs the duty of G♯ to its preceding chord, performing the duty of A♭ to its succeeding chord.

The practical conclusion is that the best plan of tuning a piano for vulgar music and vulgar players is that now ordinarily practised by the tuners, and recommended by Mr. Chappell; but if the piano is to be used equally in all keys (or even frequently in 4 or 5 flats, 5 or 6 sharps) the best plan is to tune it in 12 mathematically equal semitones.

C. B. CLARKE

Animal Intelligence

In an excellent paper on "Animal Intelligence" (NATURE, vol. xxvi. p. 523), Mr. C. Lloyd Morgan says that "The brute has to be contented with the experience he inherits or individually acquires. Man, through language spoken or written, profits by the experience of his fellows. Even the most savage tribe has traditions extending back to the father's father. May there not be, in social animals also, traditions from generation to generation, certain habits prevailing in certain communities in consequence neither of inherited instincts nor of individual experience, but simply because the young ones imitate what they see in their elder fellows?

As is well known, the stingless honey-bees (*Melipona* and *Trigona*) build horizontal combs consisting of a single layer of cells, which, if there is plenty of space, are of rather regular shape, the peripheral cells being all at about the same distance from the first built central one. Now, on February 4, 1874, I met with a nest of a small Trigona ("Abelha preguicosa") in a very narrow hole of an old canella-tree, where, from want of space they were obliged to give to their combs a very irregular shape, corresponding to the transversal section of the hole. These bees lived with me, in a spacious box, about a year (till February 10, 1875), when perhaps not a single bee survived of those which had come from the canella-tree; but notwithstanding they yet continued to build irregular combs, while quite regular ones were built by several other communities of the same species, which I have had.

The following case is still more striking. In the construction of the combs for the raising of the young, as well as of the large cells for guarding honey and pollen, our *Meliponæ* and *Trigonæ* do not use pure wax, but mix it with various resinous and other substances, which give to the wax a peculiar colour and smell. Now I had brought home from two different and distant localities two communities of our most common *Melipona* (allied to *M. marginata*), of which one had dark reddish-brown, and the other pale yellowish-brown wax, they evidently employing resin from different trees. They lived with me for many years, and either community continued, in their new home, to gather the same resins as before, though now, when they stood close together, any tree was equally accessible to the bees of either community. This can hardly be attributed to inherited instinct, as both belonged to the same species, nor to individual experience about the usefulness of the several resins (which seemed to serve equally well), but only, as far as I can judge, to tradition, each subsequent generation of young bees following the habits of their elder sisters. FRITZ MUELLER

Blumenau, St. Catharina, Brazil, November 14, 1882

The Inventor of the Incandescent Electric Light

IN the "Notes" of NATURE, vol. xxvii. p. 209, M. de Chagny is described as "the first electrician who attempted to manufacture incandescent lamps *in vacuo* about twenty years ago." This invention and its successful practical application (irrespective of cost) was made by a young American, Mr. Starr, and patented by King in 1845. A short stick of gas-retort carbon was used, and the vacuum obtained by connecting one end of this with a wire sealed through the top of a barometer tube blown out at the upper part, and the other end with a wire dipping into the mercury. The tube was about thirty-six inches long, and thus the enlarged upper portion became a torrecellian vacuum when the tube was filled and inverted. I had a share of one-eighth in the venture, assisted in making the apparatus and some of the experiments, and after the death of Mr. Starr all the apparatus was assigned to me. I showed this light (in the original lamp) publicly many times at the Midland Institute, Birmingham, and on two occasions in the Town Hall, all of them more than twenty years ago. The light was far more brilliant, and the carbon-stick more durable, than the flimsy threads of the incandescent lamps now in use. It was abandoned solely on account of the cost of supplying the power. As a steady, reliable, and beautiful light, its success was complete. In "A Contribution to the History of Electric Lighting," published in the *Journal of Science*, November 5, 1879, and reprinted lately in my "Science in Short Chapters," may be found further particulars concerning this invention and its inventor. W. MATTIEU WILLIAMS

Stonebridge Park, N.W.

The Reversion of Sunflowers at Night

WHILE the fact that sunflowers turn their faces toward the sun in its course during the day is as old as our knowledge of the plant, I am not aware that any record has been made as to the time of night that they turn to the east again after their obeisance to the setting sun.

One evening during a short stay at a village in Colorado, in the summer of 1881, I took a walk along the banks of a large irrigating ditch just as the sun was setting. The wild variety of *Helianthus annuus*, Lin. (= *H. lenticularis*, Douglass) grew abundantly there, and I observed that the broad faces of all the flowers were, as is usual in the clear sunset, turned to the west. Returning by the same path less than an hour afterwards, and immediately after the daylight was gone, I found, to my surprise, that much the greater part of those flowers had already turned their faces full to the east in anticipation, as it were, of the sun's rising.

They had in that short time retraced the semi-circle, in the traversing of which with the sun they had spent the whole day. Both the day and night were cloudless, and apparently no unusual conditions existed that might have exceptionally affected the movements of the flowers.

I doubt not that many persons like myself have supposed that sunflowers remain all night with their faces to the west, as they are when the sunlight leaves them, and until they are constrained by the light of the rising sun, to turn to the east again. It is not my purpose to offer any explanation of the cause of the phenomenon here recorded, but it seems to me improbable that it could have been an exceptional instance; and I only regret that no opportunity has since occurred to me to repeat the observation.

Washington, December 26 C. A. WHITE

Pollution of the Atmosphere

MR. H. A. PHILLIPS, in NATURE, vol. xxvii. p. 127, thinks that the effect of the increasing quantity of hydrocarbons in the air from the combustion of coal will be to make climates more extreme. It seems to me the effect will be the direct contrary. Gaseous and vaporous hydrocarbons absorb heat much more powerfully than air, and whatever makes the atmosphere absorb and retain more solar heat than at present will tend to equalise temperatures between day and night, and also between different latitudes. I think, however, that any possible effect of hydrocarbons will be quite insignificant in comparison with the effect of the watery vapour of the atmosphere, which, as Tyndall has shown, moderates climates by its power of absorbing solar heat. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, December 28, 1882

A "Natural" Experiment in Complementary Colours

ON page 79 of vol. i. of the "Life, Letters and Journal of Sir Charles Lyell," his visit to the Fall of the Rhine at Schaffhausen is described, and he notes that "as the sun shone on the foam it took very much the rose-coloured tint so remarkable on the snow in the Alps."

His experience as regards the colour being observed in the full sunlight seems to differ from that of Mr. Chas. T. Whitmell, which you published in NATURE, vol. xxvi. p. 573.

E. J. BLES

Moor End, Kersal, near Manchester, January 8

BAIRDS' HARE AND ITS HABITS

SEVERAL instances have been recorded in which individual male mammals have produced milk from their mammary glands for the nutriment of their young. But that the young of a mammal should be ordinarily suckled by the male parent is such an extraordinary anomaly that it is very hard to believe it. Yet that such is the case in an American species of hare (*Lepus bairdii*) would seem to be highly probable from observations made by Dr. Hayden and his party during one of their expeditions in the Yellowstone Mountains. In the last number of the *American Naturalist*, Mr. Lockwood gives the following details on this curious subject:—

"In the months of May and June, 1860, Prof. F. V. Hayden and his party of United States explorers found themselves up in the Alpine snows of the Wind River Mountains, where they were detained several days in an attempt to feel their way to the Yellowstone. On May 31 Dr. Hayden declared that a new species of hare was around, as he had observed unusually large hare-tracks in the snow. As the Doctor expressed himself to us:—The tracks were very large, the feet being wide-spread, and the hair thick between the toes, thus really furnishing the animal with snow shoes." In June, one was captured, and the Doctor named the species *Lepus bairdii.* The animal seemed limited to that small Alpine territory. But one specimen was secured, and no more was heard of this hare until 1872, when Dr. Hayden and party were in that region in the months of August and September. At this time five specimens of Baird's hare were obtained by Mr. C. Hart Merriam, the naturalist to the Hayden Survey. Of these four were adult males, and all had large teats and udders full of milk. The hair round the nipples was wet, and stuck to them, showing that they had just been suckling their young. To make all certain, resort was had to dissection, when the sex was demonstrated. Not only did Mr. Merriam make dissection, but also Dr. Josiah Curtis, a naturalist of the United States Geological Survey, with the same result. In the face of such testimony disbelief would seem discourtesy."

NOTES FROM THE LETTERS OF CAPTAIN DAWSON, R.A., IN COMMAND OF THE BRITISH CIRCUMPOLAR EXPEDITION.[1]

July 30, *Fort Chipewyan, on Lake Athabasca*

AFTER practically incessant travel since leaving England, at last I find myself condemned to a week's rest, as there are no boats going to Port Rae until the Mackenzie River boats return. But here we are in the lap of luxury; we get bread, butter, and milk, which we have not tasted for ages, to say nothing of the novel experience of sleeping under a roof and on a bed. I have had a most delightful journey, but it all seems like a dream to look back to: my memory is a kaleidoscope of pine trees, rapids, lakes, and golden sunrises and sunsets. Down stream we travel day and night. At sunset the boats are lashed together, and then the crew go to sleep. It is very nice drifting down in the silence amongst the pines, but bed-time comes at last. I then roll myself in a blanket, lie down, and look at the stars till I fall asleep. At sunrise I wake to find the crew on the shore, boiling their kettle, and a cup of tea is very refreshing. My blanket and my hair, too, I find dripping wet with dew when I wake.

At noon or so we land, and cook more tea, and make breakfast usually off pemmican, which is composed of buffalo flesh dried and pounded, and put in a leather bag, with grease poured over it. It is not nice, but it supports life. When we have such a luxury as flour, it is baked into cakes in a frying-pan. We get into the boat again, and eat our breakfast whilst drifting down stream. Bye-and-bye the current becomes more rapid, and at last we see the river disappearing in a cloud of spray. Here is a Portage, so the boats pull to shore and the cargo is landed. The crew then return. I take my place in the boat, and after each man has laid aside his pipe, settled himself in his seat, and got a good grip of his oar, we shove off and dash into the rapid as fast as twelve oars can take us, with shouts of "Hurrah! boys!" (the only English words the Indians know) and "ekwa," a Cree word, meaning "Come on." The guide or steersman stands on a seat in the stern steering the boat with a long oar—a picturesque figure, with his long black hair waving behind him. In a moment we are among the rapids, and seem to sink into a mass of foam, from which we emerge sideways, and are carried towards a projecting rock. Wild exclamations in French from the guide! the bow oarsman seizes a pole, and sends the boat off, and then we spin down the tail of the rapids, not without one or two bumps that make the whole frame of the boat quiver. The whole distance, a mile or two, is done in two or three minutes, and it is not bad fun. After the boats have run the rapids it is dinner-time, and then the crew set to work to carry the cargo over the portage—a work of two or three hours. In some places the boats themselves have to be hauled across on rollers, which is pretty hard work. We continue our way down stream, stopping about 4 o'clock for tea, and at sunset reach another Portage. Here we camp, and in a very short time the tents are pitched, a tree felled to make a camp-fire, and kettles singing thereon. Supper and bed-time make up the day. Such is a fair specimen of a day's river travelling. With a fair wind we sail, especially on the lakes.

The crews are Chipewyans; their language is chiefly made up of clicks and gurglings in the throat, and differs altogether from Sioux, Cree, and the other languages spoken further south.

A Roman Catholic priest here showed me a Chipewyan grammar and dictionary that they have composed. There are over sixty sounds in the language, so they have to invent additional letters. There is something Asiatic in the appearance of these Indians, with their small moustache and tufts of hair on their chin, quite unlike the Indian of the plain. They are Roman Catholic.

After leaving Portage la Loche, on July 24, the first day's journey took us down to the Terre Blanche falls; here we had to haul the boats over a small hill, as the river is a succession of falls and rapids for about half a mile; a very pretty place, the river runs between limestone cliffs, crowned with pine trees, and all stained bright orange colour with lichen.

On the 28th we reached the Athabasca, a splendid river, usually half a mile in width, sometimes more. Its course is pretty straight to the north, so we often had a view of some fifteen miles or so down the valley.

On the 29th, having a fair wind, we made a hundred miles. We met two lots of Indians; from the first we got some moose, the first fresh meat we had tasted for a long time, and from the others we got some raspberries and asketoon berries, which were very refreshing.

As we drifted down the river, the pines began to give place to poplar, the poplar to willow, and the willow to reeds, till at last we saw Lake Athabasca before us, a rocky coast to the north, and to the east water as far as the eye could reach.

A fresh breeze took us across the lake in two hours, and we received a hospitable welcome at this place, together with all sorts of luxuries that had become quite strange to us.

This is quite a large place; there are about a dozen houses, two churches, two bishops, a sisterhood, and some missionaries. The country is rocky, and most desolate. To the south and west the great lake stretches away to the horizon, and the land view is composed of hills of reddish granite, no soil, plants growing here and there out of occasional crevices, and a few stunted firs scattered about. There are woods in the valleys, but the trees are of no size. No sound breaks the stillness but the weird cry of the loon, a sort of maniacal laugh that is almost a wail; and the solitude is heightened by the reflection, that for 1000 miles north, south, east, and west all is wilderness.

Towards the lake the view is pretty, as there are many islands covered with pines.

The weather is cooler than it has been, I am glad to say. For days we had the thermometer at 85° and 86°, and even higher; but though hot, the summers are short, and I think that of this year is over. The mosquitos, at any rate, are beginning to disappear, and now the climate is nearly perfect, like the best English summer weather.

August 5.—There was a fine aurora last night: a curtain of flame seemed to descend from the sky nearly overhead and right across the sky, and after waving about for a few moments, died away again. Yesterday I went to see the Roman Catholic Mission; they have quite a pretty church, which has been built some thirty years. I was also taken to see the sisters, of whom there are six. They all seemed very flourishing, and have a very nice house.

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[1] Continued from p. 105.

August 6, *Sunday.*—I was at the English church this morning. It is a nice little church, and there was a congregation composed of the Hudson's Bay people, twenty or thirty. Most of the Hudson's Bay people are Scotch, many coming from the Orkneys. The Bishop Bompas is very pleasant, he is a great traveller, and has lived amongst the Esquimaux at the mouth of the Mackenzie River, and he works very hard.

August 9.—The weather has been stifling hot, 89° indoors, for the last three days, quite like the West Indies. Yesterday I went over to see a performance at the Roman Catholic Mission of the school children, got up by the sisters in our honour. They sang, and acted, and danced remarkably well. They have very good memories I am told.

It is curious living together without money, as one does in this country. Everything is done by barter, the unit of value being a skin; the average value of a beaver skin is said to be worth twenty ducks, or forty white fish, or twenty plugs of tobacco, so that for a plug of tobacco (about an ½ oz.) one can get a duck or two white fish, a large fish about two feet long, and very good eating. This place, like all other habitations in the north-west, swarms with large wolf-like dogs. These are used in winter for drawing carrioles, and a team of four dogs will draw 500 lbs. or more. The Indians use them too, in summer, as pack animals.

The boats have just made their appearance, four black specks on the horizon to the north, so we shall be off in a few hours.

THE SWEDISH EXPEDITION TO SPITZBERGEN, 1882

THE results of the researches of the expedition despatched to Spitzbergen last summer by the Swedish Academy of Sciences, under the eminent *savants* Baron G. de Geer and Dr. Nathorst, for the study of the geological and geographical features of the island, are very interesting. In the first instance, these gentlemen have drawn two maps, showing the exact geographical features of the island, as compared with those prepared by two previous expeditions. Of these, one shows the outlines of the fjords and valleys in the southern part of the island, with the boundary of the inland ice, and the other the relative depth of the seas around Spitzbergen and Scandinavia. From the latter it appears that these two land-formations are really elevated ridges on a comparatively level plateau, which sinks abruptly in the ocean west of Spitzbergen. In the second instance, the expedition has ascertained that the deep fjords and narrow valleys of the island have not been formed by upheaval of the terrestrial crust or by strong water-courses, but are due to the action of glaciers during the Glacial period, while from the marks on the rocks of the Beeren Island, it may be assumed that the Spitzbergen glaciers extended even so far.

At the close of the Glacial period a sudden subsidence, followed by a still greater rising of the shores, both of Spitzbergen and Scandinavia, most probably took place, which is demonstrated by the discovery, in Scandinavia as well as Spitzbergen of old gravel beaches and the shells of salt-water mussels far inland. The existence in Spitzbergen of some of the most characteristic species of the Scandinavian flora and fauna, may perhaps be explicable by migration from Scandinavia, at a period when the plateau between the two ridges was above the level of the sea, we may assume, shortly after the close of the Glacial period. It seems impossible to explain otherwise how, for instance, birds, particularly those living on land, could have found their way to this island, some 700 miles distant from the Scandinavian peninsula.

At the same period, the common Scandinavian "Blaamusling," *Mytilus edulis*, and a few other species have, no doubt, also migrated into the island. This species is now, however, extinct, but the large quantities of shells found on the shores indicate that at one time it must have been common enough. The latter circumstance seems to prove that the climate of Spitzbergen at an earlier period was much milder than at present, and corroborates also the theory of a connection having existed between Spitzbergen and Scandinavia about the Glacial period, as such a land-barrier would have caused the eastern arm of the Gulf Stream, which now flows by the North Cape, to have taken a more northerly direction, and thus carried the softening elements of a southern clime to the now desolate rocks in the Arctic Ocean. C. S.

THE INCREASE IN THE VELOCITY OF THE WIND WITH THE ALTITUDE

THE fact that the upper strata of the atmosphere as a rule move more rapidly than those near the earth's surface, has long been inferred on theoretical grounds, though little direct evidence beyond the marvellous and often unexpected voyages of aëronauts, or casual observation of the clouds, has hitherto been furnished in its favour. The practical value of this fact is beginning to be felt by engineers since the investigations undertaken by Mr. T. Stevenson in 1876, and more recently (see *Journal* of Scottish Meteorological Society, vol. v. pp. 103 and 348), showed that even for moderate heights the old notion of assuming the wind to be of uniform velocity at all altitudes was seriously in error, and that to rely upon it in the case of lofty structures might entail disastrous consequences.

While Mr. Stevenson's experiments have shown that the wind's velocity increases very considerably, especially near the surface, they do not touch the question of the increase noticed at great heights, nor can the formulæ or conclusions derived from them be said to throw any light on a matter which evidently contains the germs of many important truths for the meteorologist.

Where the engineer ends in fact the meteorologist may be said to begin; but in this case the engineer ends a little too soon, since Mr. Stevenson's latest experiments terminate at the top of a pole only 50 feet high, where he leaves us with a formula "believed to be sufficiently accurate for practical purposes," and which is said to give the velocity for "*great heights* above sea-level." Whence Mr. Stevenson obtains this formula, or on what data he believes it to be approximately correct, we are not told, and here the question is left in a state of uncertainty for greater heights, in which we trust neither engineers nor meteorologists will allow it long to remain. It might even be advantageous to the former, if instead of trusting to a few empirical formulæ, they would ask the meteorologists what they knew about the matter, and joined with them in endeavouring to discover a rational formula which would yield satisfactory results at all elevations.

Theoretically the main factor at small elevations in determining the increase of velocity, would appear to be the diminution of friction as we rise above the surface, and as this must occur most decidedly near the surface, so the velocity must increase in the first few feet "per saltum." Mr. Stevenson's experiments and curves show this very clearly. Indeed up to a height of 15 feet the increase is so sudden, so irregular, and so clearly dependent on the nature of the surface, that no attempt has been made to include this space within a formula.

There is, however, another factor which acts *positively* in the same direction, and which, while operating for the most part at great heights, where its influence ultimately predominates to the exclusion of the friction factor, must be felt to some extent at comparatively moderate elevations.

I allude to the general increase in the barometric gradient with the height above the earth's surface, due to the general temperature gradient between the equator and the poles, in conjunction with the earth's rotation. This fact has been thoroughly investigated by Mr. Ferrel of the U.S. Coast Survey, and the results given in his "Meteorological Researches," vol. i. In this work he has given on p. 45 the mean west-easterly component of the velocity at the surface due to the causes just mentioned, and the term by which this increases with the height (in metres) for every fifth degree of latitude on the mean of all longitudes, for the months of January and July, and for mean annual temperatures, calculated from the observed barometric pressures and temperatures in every part of the world.

For latitude 50° the eastward velocities at the surface and increment terms for the elevation are as follows in two different measures.

	Mean temperatures.	January.	July
Miles per hour	$3·35+8·6h$	... $3·97+12·1h$	... $2·73+5·1h$
Feet per second	$4·91+·0024h$ ...	$5·82+·0033h$ ...	$4·00+·0014h$

where *h* represents the height in miles and feet respectively.[1]

Owing to *this cause alone* therefore the eastward (and therefore in our latitudes the prevailing) motion of the atmosphere will be increased on the mean of the year by 8½ miles per hour at a height of 5280 feet, or by 2⅛ miles per hour at a height of about 1300 feet.

The increase in the horizontal velocity which results from the joint action of these two factors, is thus probably very different from that which would arise from a mere diminution of friction alone, since at great heights this would theoretically become almost insensible.

For a thoroughly satisfactory solution of the matter, nothing will avail except anemometrical observations made at every possible elevation (preferably, as I lately suggested in a paper read before the Meteorological Society, with instruments attached to kite-strings), but in the absence of these at present, it may be worth while to use some excellent observations on the velocity of different cloud-layers recently communicated to the Austrian *Zeitschrift fur Meteorologie*, by Dr. Vettin,[2] for the purpose of showing the complete breakdown of Mr. Stevenson's formula when applied to "great heights above sea-level."

The following table, which is taken from Dr. Vettin's paper, gives the mean velocity of the clouds from all directions, at five altitudes to which they respectively belong, and which, together with their velocities, have been measured by methods described in detail in the paper from which it is extracted:—

TABLE I.

Name of station.	Barometric pressure.[3] in.	Height in feet.	Number of observations.	Mean velocity in feet per second
Upper cirrus ...	... 11·968 ...	23,000 ...	879 ...	59·5
Under cirrus ...	... 17·953 ...	12,800 ...	1047 ...	51·8
Cloudlets[4] ...	... 22·441 ...	7200 ...	1588 ...	35·0
Cloud ...	... 25·733 ...	3800 ...	1871 ...	30·4
Under cloud ...	... 28·127 ...	1600 ...	1292 ...	37·4
Wind (sea-level) ...	29·922 ...	0 ...	4168 ...	19·8

It will be seen from this table that while there is a rapid increase in the velocity of the wind through the first 1600 feet, an abrupt diminution occurs between this height and 3800 feet, after which the motion again increases at a more moderate rate.

Now Mr. Stevenson's formula for heights above 50 feet is $\frac{V}{v} = \frac{H}{h}$, where V, v, H, h, are the velocities and heights at the upper and lower stations respectively. If we apply this formula to the preceding table and calculate the heights at the higher levels from those at the lower ones, we get for the most favourable cases the following values:—

Observed Velocity.	Observed Height.		Calculated Velocity.	Calculated Height.
37·4 ...	1,600	... ...	259·2 ...	12,800
51·8 ...	12,800	... ...	113·1 ...	23,000

which are so absurdly in excess of those observed at the same levels, and so far beyond what we might reasonably expect as to render it doubtful whether this formula is true for any height above the first 100 feet. Even the formula which is supposed by Mr. Stevenson to fail above 50 feet gives better results than this one at the higher levels.

This formula is $\frac{V}{v} = \sqrt{\frac{H+72}{h+72}}$, and from the same observed values as those used above gives the following calculated values:—

Height		Velocity.
12,800	... ...	103·7
23,000	... ...	69·3

but even these are far in excess of those observed. Both formulæ moreover fail lamentably up to 1600 feet, for even if we assume that 19·8 represents the velocity, not at sea-level, but at an elevation of 100 feet above it (an exceedingly favourable assumption since the velocity at this height would considerably exceed that at sea-level), the first formula would make the velocity at 1600 feet *four* times, and the second more than *three* times that actually observed by Dr. Vettin.

It is plain, therefore, that both formulæ must fail considerably below 1600 feet, and until further evidence is furnished it would seem probable that neither of them give correct results much above 100 feet or so.

For practical engineering purposes no doubt they would succeed only too well, since they would probably give a maximum velocity far in excess of the truth, and this, judging from examples such as the Tay Bridge, would be no disadvantage when the force of the wind enters into engineering calculations.

It would surely be better, however, if we could arrive at a somewhat closer approximation to the truth, and better still if we could arrive at the truth itself. This, as I have already pointed out, will be only accomplished for the lower strata by further experiment in the same direction as that already followed by Mr. Stevenson, modified by attaching anemometers to kite strings, and for the upper strata, which chiefly concern the meteorologist by observations of the clouds similar to those made by Dr. Vettin.

Meanwhile, however, I have found a formula which gives very much more satisfactory values at the higher levels than those furnished by Mr. Stevenson. This formula is—

$$\frac{V}{v} = \sqrt[4]{\frac{H}{h}}$$

And although I do not expect it will be found to hold *very near* the surface, it certainly accords, omitting the anomalous case at 3800 feet, from 1600 feet to 23,000 feet, or through a range of 21,400 feet, very closely with the values observed by Vettin.

The figures observed and those calculated from this formula are as follows:—

TABLE II.

Height of lower stratum.	Height of upper stratum.	Velocity Observed.	Velocity Calculated.
3,800 ...	7,200 ...	35 ...	35·6
1,600 7,200 ...	12,800 ...	51·8 ...	51·5[1]
12,800 ...	23,000 ...	59·9 ...	59·5
23,000 ...	41,000 ...	67[2] ...	68·7

[1] The mean of the two. [2] Calculated by Vettin.

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[1] It must be noted that the surface velocities given in this table are somewhat in excess of the truth, owing to the neglect of surface friction, but this does not affect the increment terms to any large extent.

[2] *Zeitschrift für Meteorologie*, Band xvii., July and September Heft; "Die Luftstromungen uber Berlin."

[3] Reduced from the original figures in millimetres.

[4] Wolkchen.

Moreover, if we assume that 19·8 is the velocity at 100 feet, the velocity at 1,600 feet calculated from it by this formula would be almost exactly *equal* to that observed, instead of *four* times as much as it was when calculated from $\frac{V}{v} = \frac{H}{h}$. The empirical formula $v = 28{\cdot}1 + 0{\cdot}2 \sqrt{h}$, where v is the velocity in feet per second, and h is the corresponding height in feet, gives for the four elevations above, results in close agreement with those observed, but it would probably fail below 1,600 feet.

Mr. Stevenson in his first paper uses a formula $\frac{F}{f} = \sqrt{\frac{H}{h}}$ where $F f$ are the forces ("pressures" I suppose is meant by this objectionable word) at the two levels corresponding to H and h.

In his second paper he says he prefers the formula $\frac{V}{v} = \frac{H}{h} = \frac{P}{p}$, where P and p are the pressures at the two levels to the formula $\frac{V}{v} = \frac{\sqrt{H}}{\sqrt{h}}$, from which and some other remarks it would appear that pressure and velocity are considered to vary directly with each other. This is a notion which is certainly at variance not only with the hitherto generally accepted empirical formulæ, but is distinctly contrary to the results lately deduced by Mr. Ferrel from the hydrodynamical theory (see Van Nostrand's *Engineering Magazine*, vol. xxvii. p. 141). The formula usually found in the text-books is $p = {\cdot}00492\, v^2$, and the one deduced by Ferrel is

$$p = \frac{{\cdot}0027}{1 + {\cdot}003665\, t} \frac{P}{P'} v^2,$$

where $P\, P'$ are the barometric pressures at the level under consideration and at sea-level respectively, and t is the temperature in degrees Centigrade; and though from the latter formula it is evident that the pressure at the higher levels will be less for the same velocity than below (at the height of Mont Blanc, for example, it would be reduced by about one-half) there is nothing to lead us to infer that $\frac{V}{v} = \frac{P}{p}$.

If in the formula $\frac{F}{f} = \sqrt{\frac{H}{h}}$ which was discarded by Mr. Stevenson, we make the ordinary assumption that $\frac{F}{f} = \frac{V^2}{v^2}$ we get the formula which I have already shown gives results which agree closely with the values observed by Vettin above 1,600 feet, and which even below this height is much nearer the truth so far as can be inferred from the slender data employed, than the formula preferred by Mr. Stevenson.

If we take the heights as abscissæ, the curve traced out by the velocity-ordinates will be much flatter than the ordinary conical parabola, and at great heights will approximate very nearly to a straight line parallel to the axis. Ferrel's increment-term makes it a straight line all through, but his formula assumes that the temperature gradient between the pole and the equator is the same above as at the earth's surface, it leaves out friction altogether, and also supposes the velocity for the same gradient to be the same at all heights, whereas according to theory it should increase with the height in the ratio $\frac{\cos i}{P}$, where i is the angle the wind makes with the isobar, and P is the barometric pressure at the level under consideration. Notwithstanding these omitted factors, of which the first and last probably tend to destroy each other, it will be found that the addition of the increment corresponding to each altitude to the velocity at the surface observed by Vettin gives us the following fair approximation to the values actually observed, though the calculated values are too small at 1600 feet and too large at 23,000 feet by just about the same amount:—

Height.	Velocity observed.	Calculated from Ferrel's Formula.
23,000	59·5	75·0
12,800	51·8	50 5
7,200	35	37·0
3 800	30·4	28·9
1,600	37·4	23·6
0	19·8	

In conclusion, it is evident that, quite apart from the meteorological side of the question, more investigations like those undertaken by Mr. Stevenson, are urgently required to determine the actual rate of increase of the velocity at moderate heights, from which a formula like the one I have recommended may be deduced, which will yield values more within the range of probability than those furnished by the one which is apparently supposed to suffice for the rest of the atmosphere after we have reached the top of the fifty-foot pole.

E. DOUGLAS ARCHIBALD

KRAO, THE "HUMAN MONKEY"

THROUGH the courtesy of Mr. Farini, I have had a private interview with this curious little waif, which he is now exhibiting at the Royal Aquarium, Westminster, and for which he claims the distinction of being the long-sought-for "missing link" between man and the Anthropoid apes. Krao certainly presents some abnormal peculiarities, but they are scarcely of a sufficiently pronounced type to justify the claim. She is, in fact, a distinctly human child, apparently about seven years old, endowed with an average share of intelligence, and possessing the faculty of articulate speech. Since her arrival about ten weeks ago in London, she has acquired several English words, which she uses intelligently, and not merely parrot-fashion, as has been stated. Thus, on my suddenly producing my watch at the interview, she was attracted by the glitter, and cried out *c'ock, c'ock*, that is, *clock, clock!* This showed considerable powers of generalisation, accompanied by a somewhat defective articulation, and it appears that her phonetic system does not yet embrace the liquids *l* and *r*. But in this and other respects her education is progressing favourably, and she has already so far adapted herself to civilised ways, that the mere threat to be sent back to her own people is always sufficient to suppress any symptoms of unruly conduct.

Physically Krao presents several peculiar features. The head and low forehead are covered down to the bushy eyebrows with the deep black, lank, and lustreless hair, characteristic of the Mongoloid races. The whole body is also overgrown with a far less dense coating of soft, black hair about a quarter of an inch long, but nowhere close enough to conceal the colour of the skin, which may be described as of a dark olive-brown shade. The nose is extremely short and low, with excessively broad nostrils, merging in the full, pouched cheeks, into which she appears to have the habit of stuffing her food, monkey-fashion. Like those of the anthropoids her feet are also prehensile, and the hands so flexible that they bend quite back over the wrists. The thumb also doubles completely back, and of the four fingers, all the top joints bend at pleasure independently inwards. Prognathism seems to be very slightly developed, and the beautiful round black eyes are very large and perfectly horizontal. Hence the expression is on the whole far from unpleasing, and not nearly so ape-like as that of many Negritos, and especially of the Javanese "Ardi," figured by me in NATURE, vol. xxiii. p. 200. But it should be mentioned that when in a pet, Krao's lips are said to protrude so far as to give her "quite a chimpanzee look."

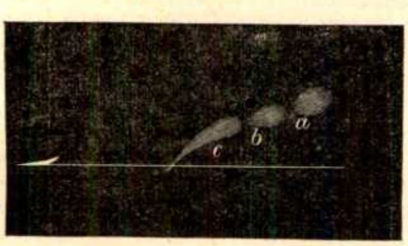
Apart from her history one might feel disposed to regard this specimen merely as a "sport" or *lusus naturæ*, possessed rather of a pathological than of a strictly anthropological interest. Certainly isolated cases of hairy persons, and even of hairy families, are not unknown to science. Several were figured in a recent number of the Berlin *Zeitschrift für Ethnologie*, and, if I remember, both Crawfurd ("Journal of an Embassy to Ava") and Col. Yule ("Mission to the Court of Ava") speak of a hairy family resident for two or three generations at the Burmese capital. This family is reported to have come originally from the interior of the Lao country, and in the same region we are now told that little Krao and her parents, also hairy people, were found last year by the well-known eastern explorer, Mr. Carl Bock. Soon after their capture, the father appears to have died of cholera, while the mother was detained at Bangkok by the Siamese Government, so that Krao alone could be brought to England. But before his death a photograph of the father was taken by Mr. Bock, who describes him as "completely covered with a thick hairy coat, exactly like that of the anthropoid apes. On his face not only had he a heavy, bushy beard and whiskers, similar in every respect to the hairy family at the court of the King of Burmah, who also came from the same region as that in which Krao and her father were found; but every part was thoroughly enveloped in hair. The long arms and the rounded stomach also proclaimed his close alliance to the monkey-form, while his power of speech and his intelligence were so far developed that before his death he was able to utter a few words in Malay."

Assuming the accuracy of these statements, and of this description, little Krao, of course, at once acquires exceptional scientific importance. She would at all events be a living proof of the presence of a hairy race in Further India, a region at present mainly occupied by almost hairless Mongoloid peoples. From these races the large straight eyes would also detach the Krao type, and point to a possible connection with the hairy, straight-eyed Aino tribes still surviving in Yesso and Sakhalin, and formerly widely diffused over Japan and the opposite mainland.[1]

A. H. KEANE

FIGURE OF THE NUCLEUS OF THE BRIGHT COMET OF 1882 (GOULD)[2]

ALTHOUGH this comet presented a beautiful spectacle, when seen with the naked eye, I have been disappointed at the small amount of work which I have been able to do in the way of accurate observation. I give herewith the only two good sketches which I have been able to make. The aperture employed was 15 inches, and the power was 145 diameters.

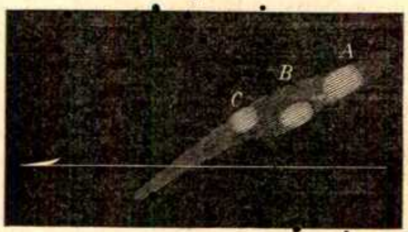


1882, October 13.

1882, *October* 13.—(See the figure.) The nucleus is curved as in the drawing. It consists of three masses. I am sure of a break at *a*; tolerably sure of the break at *b*, and I suspect a break at *c*, but I am not certain of it.

1882, *October* 14.—The night is very poor. (In general the appearances of last night are confirmed.) The nucleus is about 1′ long.

1882, *October* 17.—(See the figure.) There are three masses, plainly separated. *B* is farther north than the line *A*—*C* by 3—4″. There is a dark division between each pair of masses. *B* and *C* are nearly in the parallel.



1882, October 17.

The brush of light from the mass *A* toward the east, comes from the south side of *A*, as it is drawn. From the W. end of *A* to the E. end of the brush of light, is about 15″.

1882, *October* 18.—The dark space between *A* and *B* is about 10″; it is as wide as *A* itself, and wider than on October 17. *C* is certainly seen as a separate mass; *A* and *B* are bright and stellar in appearance, more so than on October 17. *C* is, however, fainter than then. The dark axis of the tail extends quite up to the coma.

1882, *October* 19.—Cloudy. The nucleus is seen as before. *A* and *B* are seen, as also the dark space between them. *C* is not seen, but this is probably on account of the unsteady air.

I regret that my opportunity did not allow me to make any further sketches of value.

Washburn Observatory, University of Wisconsin,
Madison, November 3, 1882

NOTES

THE office of Director of the Geological Survey of Scotland, vacant by the promotion of Mr. Geikie to be Director-General, has been filled up by the appointment of Mr. H. H. Howell, District Surveyor of the Geological Survey of Scotland during the earlier years of its progress, but who since the separation of the Scottish branch of the establishment in 1867, has been continuously employed in England, where he has personally surveyed large tracts of the northern counties, and where for some years past he has had the direct personal supervision of the whole of the field-work in that district. He will not be able to enter fully on his duties in Scotland until the area now under his charge in the north of England has been completely surveyed. The promotion of Mr. Howell having caused a vacancy in the rank of District Surveyor, Mr. W. Whitaker has been appointed to the post. This geologist is well known for his detailed surveys of the Tertiary deposits of the London basin. He is at present engaged in the survey of Norfolk.

THE United States Transit of Venus expedition, under Prof. Newcomb, arrived at Plymouth on Sunday as passengers by the Union Steamship Company's steamer *Moor*, from Cape Town. They report that their observations were made at Wellington, fifty-eight miles from Cape Town, under extremely favourable conditions, two good observations of internal contact and 236 photographs being obtained, of which more than 200 can be measured.

THE annual general meeting of the Association for the Improvement of Geometrical Teaching will be held, through the kindness of the Council, at University College, Gower Street, on Wednesday, the 17th instant, at 11 a.m. In addition to the usual routine business, the president (R. B. Hayward, F.R.S.)

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[1] See my paper on "Aino Ethnology" in NATURE, vol. xxvi. p. 524.

[2] Paper by Prof. Edward S. Holden, in the *American Journal of Science and Arts*.

will propose "that the Committee for Elementary Plane Geometry be instructed to publish Part I of the Plane Geometry, and to take such steps as they may deem advisable to secure its recognition as a basis of instruction and examination in geometry." It will be in the recollection of some of our readers that the object of the Association was extended at the last annual meeting, so as to include the effecting of improvements in the teaching of elementary mathematics and mathematical physics. This extension has met with great approval, and the novelty of this year's meeting will be the reading of three papers: (1) "The Teaching of Elementary Mechanics," by W. H. Besant, F.R.S. (2) "The Basis of Statics," by Prof. H. Lamb, of the Adelaide University. (Prof. Lamb is of the opinion that the true and proper basis of statics is to be sought for in the principles of linear and angular momentum). (3) "Notes on the Teachings of Dynamics," by Prof. Minchin. The reading of the papers will be followed by a discussion, in which it is hoped that Prof. G. Carey Foster, F.R.S., Prof. Minchin, and others will take part. The papers will be read at the afternoon meeting which begins at 2 p.m. The two present honorary secretaries will resign office; Mr. Levett, in consequence of the pressing necessity of his other duties, and Mr. Tucker, in consequence of the state of his health, which compels him to withdraw from some of his engagements; both gentlemen, however, hope to remain on the council and act as *amici curiæ* to their successors in office.

THE well-deserved honour of C.I.E. has been conferred upon Surgeon-Major James Edward Tierney Aitchison (Bengal Army), F.L.S., who did such excellent botanical work with Sir Frederick Roberts's force in the Kuram Valley during the Afghan war.

GEOLOGISTS will regret to hear that one of the most promising of the younger members of their number, Mr. E. B. Tawney, of the Woodwardian Museum, Cambridge, died suddenly at Mentone on the 30th ult.

THE death is announced, on December 22 last, of Dr. Carl Hornstein, professor of theoretical and practical astronomy, and director of the observatory in the Carl Ferdinands University, Prag, at the age of fifty-eight years.

THE thirty-sixth annual general meeting of the Institution of Mechanical Engineers will be held on Thursday, January 25, and Friday, January 26, at 25, Great George Street, Westminster. The chair will be taken by the president, Percy G. B. Westmacott, Esq., at half-past seven p.m. on each evening. The following papers will be read and discussed:—Report on the hardening of steel, by Prof. F. A. Abel, C.B., F.R.S., of Woolwich; on the molecular rigidity of tempered steel, by Prof. D. E. Hughes, F.R.S., of London; on the working of blast furnaces, with special reference to the analysis of the escaping gases, by Mr. Charles Cochrane, of Stourbridge, vice-president; on the St. Gothard tunnel, by Herr E. Wendelstein, of Lucerne; on the strength of shafting when exposed both to torsion and end-thrust, by Prof. A. G. Greenhill, of Woolwich.

THE old female Hippopotamus (*Adhela*) presented to the Zoological Society in 1853 by the then Viceroy of Egypt, died in the Gardens on the 16th ult., after having for some time past exhibited manifest signs of old age. Her mate (*Obaysch*) died in 1877, after having lived twenty-seven years in the Gardens. It is thus evident that about thirty years is the extreme limit of Hippopotamine existence, as it is not at all likely (judging from the state of the teeth and bones) that either of these animals would have been able to support existence so long in its native wilds, as under the favourable circumstances in which it lived in the Regent's Park.

DR. BLASIUS of Brunswick has recently shown that the fossil remains of a species of Souslik, found in various parts of Northern Germany, which are usually attributed to *Spermophilus altaicus*, really belong to *S. rufescens*, Keys. et Bl. It is probable that the cave-bones from the Mendip Hills, upon which Dr. Falconer established his *Sp. erythrogonoides* (Pal. Mem., ii., p. 453), are really of the same species, and that this Rodent, now driven far east into the steppes of Orenburgh (like other members of the Steppe-fauna), formerly extended all over Northern Europe, and even into the British Islands.

RECENTLY, our readers may remember, Miss Baxter of Balgavies, sister of Sir David Baxter, and aunt of the Right Hon. W. E. Baxter, and the late Dr. Baxter, Procurator-Fiscal of Dundee, gave jointly 150,000*l*. for the endowment and erection of a college in Dundee. Buildings have been acquired, professors appointed, and the work of the college will soon be begun. Miss Baxter has just given another 10,000*l*. to provide a laboratory, and the trustees of the late Dr. Baxter also 10,000*l*. to found a Chair of Law.

AS the late M. Gambetta was a member of the Society of Dissection, an autopsy of his body was made. The weight of his brain was found to be 1100 grams; M. Mathias Duval, Professor in the Faculty of Medicine, found the structure of the brain to be very fine, and the third convolution, which M. Broca associates with the speechifying faculty, to be remarkably developed.

THE project of the United States for establishing an universal meridian has been sent to the Paris Academy of Sciences for approval. It is expected that Great Britain may object to this measure, and it has been proposed that, in consideration of the services rendered to geography by England, that the Greenwich meridian should be selected as the start-point for time and longitude.

OUR Paris Correspondent writes that the second Paris inundation is developing its ravages with peculiarities which prove that modern engineers do not pay sufficient attention to the effects of their works on the *régime* of the stream they profess to regulate. The level of the Seine is just as high at Charenton as it was in 1876, although it is 20 centimetres less elevated at Pont Royal, where it reaches only 7 metres. The reason of this difference is that an ignorant Municipal Council authorised the building of a bridge which crosses the river obliquely, and a new quay at Bercy, where in some places the dimensions of the bed of the stream have been diminished by not less than 55 metres. If the rains continue it is feared that one of the Paris bridges, the Invalides, will be carried away, which will produce real disaster.

PROF. BÖRNSTEIN, of Berlin, has brought out a small work on meteorology under the title of "Regen oder Sonnenschein." In conjunction with Prof Landolt he has also nearly completed an important work ("Physikalisch-Chemische Tabellen") containing all the most reliable determination of constants required in chemical and physical work, some of which will be published in a collected form for the first time.

MR. J. P. MCEWEN, of Hong Kong, under date November 28, 1882, sends us some observations of the comet, taken on the morning of the 27th:—26d. 15h. 43m. 17s. mean time at place, the distance from Sirius measured by sextant was 34° 32′; 26d. 15h. 50m. 30s. mean time at place, the distance from Procyon was 39° 31′; longitude in time, 7h. 36m. 40s. A line drawn from the small brightest star in the lower part of the sword-scabbard of Orion through Sirius almost exactly passed through the nucleus of the comet; the apparent length of the tail was about twice that of Orion's belt. It was getting very indistinct, and on the 27th, owing to the bright moonlight, was more so than if the night had been dark and clear. Mr. McEwen has seen the comet several times, and when at its greatest brilliancy, stars of the 4th or 5th magnitude could be distinctly seen through the tail. The tail pointed in a direction about midway between Sirius and Procyon. M. Dechevrens, the director of the Zi-ka-Wei Observatory (near Shanghai) has devoted a good deal of attention to this comet, the result of which will directly be published.

Amateur Mechanics is the name of a new illustrated monthly Magazine, conducted by Mr. P. N. Hasluck, and published by Trubner and Co.

WE have received from the U.S. Naval Observatory the results of the observations made to determine the longitude of the observatory of the J. C. Green School of Science, Princeton, N.J. The final result is that the latter is 0h. 9m. 34s.·538 east of the central dome of the observatory.

THE earthquake in Panama on November 7 was followed by a violent shock on November 13 at 2.30 a.m. It was observed also at Taboga and Colon. It is remarkable that all the Central American earthquakes since August last have occurred between midnight and daybreak. Their general direction was invariably from north to south, and it is supposed that they proceeded from one and the same cause. The West Indian cable broke, at a point about thirty miles from land, during a violent shock. The centre of the disturbance seems to lie near the West Indian Isles. During the second week of December seven shocks were felt in the Spanish province of Almeria. On December 8 at 10.1 p.m. a fearful shock lasting four seconds was felt at Tecuci (Roumania). Its direction was from south-east to north-west. Another earthquake is reported from Hermagor (Carinthia). It occurred on December 10 at 2 a.m., and was preceded by a terrible thunderstorm.

AN "Illustrirte Bienenzeitung," organ for the propagation of rational apiculture, will be edited by Prof. Adolphson of Zurich, beginning on the 1st inst.

IN the Pelion district a moderately violent earthquake occurred on December 11, but no damage was done. Upon the island of Santorin new volcanic activity has recently been noticed; also in the subterranean volcano which formed near Missolunghi.

THE additions to the Zoological Society's Gardens during the past week include a Himalayan Bear (*Ursus tibetanus*) from Burmah, presented by Capt. Connor; two Bronze Fruit Pigeons (*Carpophaga ænea*) from India, presented by Mrs. A. H. Jamrach; four Barred-shouldered Doves (*Geopelia humeralis*) from Australia, presented by Mr. Ernest L. Bentley; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from Moluccas, presented by Mr. K. Digby; a Gannet (*Sula bassana*), British, presented by Mr. Thomas Keen; a Cape Bucephalus (*Bucephalus capensis*) from South Africa, presented by Mr. H. Pillans; a White-fronted Lemur (*Lemur albifrons* ♀) from Madagascar, four Wood Thrushes (*Turdus mustelinus*), a Golden-winged Woodpecker (*Colaptes auratus*) from North America, two Cirl Buntings (*Emberiza cirlus*), two Crested Grebes (*Podiceps cristatus*), a Razorbill (*Alca torda*), a Bar-tailed Godwit (*Limosa lapponica*), a Red-throated Diver (*Colymbus septentrionalis*), British, purchased.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE ON MAY 6.—The right ascensions and declinations of the moon for 1883, both in the *Nautical Almanac* and the *American Ephemeris*, depend upon Hansen's Tables, with the recent corrections of Prof. Newcomb. They furnish as accurate positions as are obtainable from existing tabular data, and it will be of interest to trace their bearing upon the circumstances of the total eclipse of the sun which crosses the Pacific on May 6. On laying down the belt of totality upon the Admiralty chart of this ocean, it appears that the following islands are included within it, viz.:—Rance, Buffon, Beveridge, Flint, Caroline, and Chanel Island (in the Marquesas); the positions read off from the general chart or for Flint, Caroline, and Chanel Island, from the enlarged Admiralty charts are as follows:—

Rance Island,	Long.	176	22	West.	Lat.	24	20	South.
Buffon ,,	,,	170	0	,,	,,	20	39	,,
Beveridge ,,	,,	167	50	,,	,,	20	0	,,
Flint ,,	,,	151	50	,,	,,	11	25	,,
Caroline ,,	,,	150	6	,,	,,	9	54	,,
Chanel ,,	,,	140	31	,,	,,	7	55	,,

From direct calculation for each of these points the following local mean times of beginning of totality, the duration of the same, and the sun's approximate altitude at the time, result:—

	Totality begins May 6.				Duration.		Sun's Altitude.
	h.	m.	s		m.	s.	°
Rance Island,	8	47	36	a.m.	3	27	29
Buffon ,,	9	22	18	,,	4	20	38
Beveridge ,,	9	34	48	,,	4	1	41
Flint ,,	11	19	43	,,	5	26	61
Caroline ,,	11	33	4	,,	5	7	63
Chanel ,,	0	43	32	p.m.	1	47	63

It should be mentioned that the semi-diameter of the sun has been taken from the *Nautical Almanac*; that of the moon was obtained from her horizontal parallax, using the factor 0·2725. The duration of totality at Sohag in Egypt in the eclipse of last May was exactly given by this arrangement.

THE MINOR PLANETS.—The part of the *Berliner Astronomisches Jahrbuch* for 1885, containing ephemerides of the minor planets for 1883, has been issued to the various observatories in advance of the publication of the annual volume. It contains approximate places for every twentieth day of 224 of these bodies, the latest being No. 225, with accurately calculated opposition ephemerides of 43, each extending over about five weeks; this division of the *Jahrbuch* occupies upwards of one hundred pages.

There are six cases during the year where the planets approach the earth about opposition, within her mean distance from the sun. On June 22 *Phocea* is at a distance of 0·93, declination + 16°; on July 12 the distance of *Clio* is 0·96, declination −35½°; on August 1 that of *Isis* is 0·90, declination −28°; on October 1 that of *Polyhymnia* is 0·98, declination +8½°; on October 20 that of *Virginia* is 0·98, declination +13°, and on December 11 *Flora* in perigee is at a distance of 0·97, with declination +18°. Galle's method of determining the solar parallax, so strongly advocated and ably applied by Mr. Gill, is not likely to fail for want of opportunities of applying it. As regards the magnitude near opposition we have in the case of *Phocea* 9·0; *Clio*, 10·2; *Isis*, 8·8; *Polyhymnia*, 9·7; *Virginia*, 9·9; and *Flora*, 8·2.

During the year 1883 four of these planets descend below 14m., from coming into opposition not far from aphelion.

COMET 1882 *c*.—Mr. Gill has secured five complete observations of this comet (discovered by Mr. Barnard in September) on the meridian S.P., with the transit-circle at the Cape of Good Hope, between November 11 and 30, so that places for upwards of a fortnight after the perihelion passage will be available for calculation.

THE EDUCATION OF OUR INDUSTRIAL CLASSES[1]

IT is, I believe, according to precedent, now that another year's work of the Science Classes here has been crowned by the award of prizes, that I should address you on some topic allied to the matters which have brought us together to-night. I need not search long for a subject, for the scientific education of those engaged in our national industries—upon the success or failure of which, in the struggle for existence, the welfare of our country so largely depends—is now one of the questions of the day. I propose, therefore, to lay before you some facts and figures bearing upon the education of our industrial classes, and I shall attempt to make what I have to say on that special point clearer, by touching upon some preliminary matters, which will show how it is that such a question as this has not been settled long ago; and further, that we can, if we wish, settle it now in the full light of the experience gained elsewhere, instead of wasting let us say a quarter of a century in costly experiments which may perhaps leave us in confusion more confounded. To begin, then, why is this question being discussed now? There is a great fact embodied in the most concrete fashion in the way in which our Government is now compelled to deal with our national education. Side by side of the Education Department by which our Minister controls in the main that book learning which has been given time out of mind, there has sprung up during the last thirty years another department—the Science and Art Department—by which he controls a new kind of national learning altogether. We have added to the old study of books a new study of things. This new learning was, we may say, only introduced in 1852, in which year the Queen in her speech on opening Parliament said: "The advancement of the fine arts and of practical science will be readily recognised by you as worthy the attention of a great and enlightened nation." We have since found out that they are indeed worthy the attention of a great nation, and more than this, that no nation can be called enlightened whose citizens are not skilled in both; in fact, that they are to peace what cannon and swords are to war. But for a nation to foster them is one thing, to include them in a national scheme of education is another. Ought they to be so included? Let us see. What do we mean by education? Roughly speaking, we may say that there are two distinct schools of thought on this subject, although the existence of these two schools is not so generally recognised as it should be. According to one view, the human mind is an elastic bag into which facts are to be crammed for future use. A variation of the view is that the mind is inelastic, and then the stuffing process becomes more serious, and instead of depending upon a natural expansion, a process like that in use by the manufacturers of soda-water is employed. It is not to be wondered at that the youthful mind likes neither of these methods; what ought to be a true delight becomes a real agony, and hence it is, as a Warwickshire man wrote many years ago—

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[1] An address delivered in presenting the prizes at the Coventry Science Classes, by J. Norman Lockyer, F.R.S.

"Love goes toward love
As schoolboys from their books;
But love from love
Toward school with heavy looks."

—The mind on this view resembles a store where, as our American cousins say, everything, from a frying-pan to a frigate, which shall be useful to the owner in after life, is to be found. Hence such terms as Grammar School, Trade School, Science School, Commercial Academy, and hence I am sorry to say, systems of examination which too often only serve to show what a boy can remember, and little care about either what a boy can do, or whether he can think. So much for one view. Now for the other. It is more difficult to image it, but in the absence of a better illustration, the mind may be likened to the body—a thing to be trained so that its grace, its freedom, its strength, its grasp, indeed all its powers in all directions and in all ways may be brought out by proper training. If the training is one-sided its power cannot be many-sided, but it is most useful when many-sided. Therefore, as each muscle of the body has to be properly trained to make a perfect man, so must the educational system brought into play be such as to train to its uttermost and bring out each quality of the mind. Each faculty of it when called into play becomes as a two-edged sword in the arms of a strong man. In this, or some such way, then, may we picture to ourselves the difference between instruction in its real sense, and education in its real sense. Now, which of these systems is the better one? We shall see at once that the first may give us a mind stored with facts covering a large or a small area; it may be bookkeeping, or it may be Latin, or anything else. But will the mind be able to use this store in all cases? We grant *knowledge*, but may not *wisdom* linger? Those of us who have got to Voltaire's second stage, and who have studied men, know that this too often happens, and that much knowledge does not prevent the owner from being absolutely unfitted to grapple with the problems which each rising sun brings to him for solution. The other system, on the other hand, if the training is not thoroughly all-round, may give us a man who finds that the questions presented to him on his entrance to active life are precisely those which require the application of that quality of mind, whichever it may be, which was least trained at school. He may find himself face to face with problems of the existence of which he never dreamed, and so far removed from his experience that his mind, however powerful in some directions, fails to grapple with them. We seem, then, on the horns of a dilemma. Instruction may provide us with a store of facts, which the mind does not know how to use. Education may provide us with a mind which has been trained in a world utterly different from the real one. How can we escape from this dilemma. *We must use the materials of that instruction which is most useful to us in our progress through life as a basis for the complete education of the mind.* Which instruction is the most useful to us? The poet tells us, that "the proper study of mankind is man"; but when we come to prose and read the views of those who best know the needs of modern society, and especially industrial society, we read something like this which I quote from the report on elementary and middle class instruction, published by the Royal Commission of the Netherlands: "The idea of *Industrial Society* not limited to agriculture, manufactures, and trade or commerce, but understood in its widest signification, points plainly to the acquiring of the knowledge of the present world, and to its application to economical and technical pursuits." Now, here is a subject on which a volume might be written, but I shall only point out to you the obviousness of the importance of the study, not merely of ourselves, *or* of the world around us, but of ourselves, *and* of the world around us. This lands us in the necessity of training our minds in literature or humanities, and science and art—the study of the humanities enables us to know the best thoughts, and the most stable conclusions on vital questions, arrived at by our forerunners and those who are fighting the same battles in other lands. The study of science enables us, on the other hand, to get a true idea of the beautiful universe around us, of our real work in the world, and of the best manner in which we can do that work in closest harmony with the laws of Nature. Did we study the external world alone we should not profit by the experience of those that preceded us. Did we study humanities alone we should be shorn of half our natural strength in face of many of the problems placed before us by the conditions of modern life; and, more than this, all the glories of the beautiful world on which our lot is cast, and the majesty of the universe of which that world forms part would hardly exist for us, or give rise only to dumb wonder. Here let me tell you a little story: Three years ago when travelling in America, one morning, at a little station—we were approaching the Rocky Mountains—I was astonished to see a very old and venerable French curé in his usual garb enter the car, and as he was evidently in some distress of mind, and as evidently had little command of English, I asked him in his native language if I could be of any service to him. There was a difficulty about a box which I soon settled, and then we sat down and entered into conversation. He soon found out that I was very astonished to see him there and told me so. I acknowledged it. "It is very simple," he said, "I am very old, and six months ago I was like to die and I was doing my best to prepare myself for the long journey. In my fancies I imagined myself already in the presence of *le bon Dieu*, and I fancied this question addressed to me, 'M. le curé, how did you like the beautiful world you have left'? I rose in my bed as this thought came into my head for I—I who—figure to yourself—had dared to preach of a better world for fifty years, was, oh! so ignorant of this. And I registered a vow that if *le bon Dieu* allowed me to rise from that bed of sickness I would spend the rest of my life in admiring his works—*et me voici!* I am only on my journey round the world; I am going now to stop at the Yosemite Valley a few days *en route* for San Francisco and Japan, and the box, Monsieur, which your kindness has rescued for me contains a little scientific library, now my constant companion in my delicious wanderings." Our general scheme of education, therefore, unless it is to be one-sided, must combine science with the humanities. But, so far, I have said nothing about art. Now, from the educational point of view, science and art are very closely connected, inasmuch as in the early stages of both studies the student's powers of observation are brought out and trained in the most perfect way, while in the later stages, to succeed in either, he must have learned that very important thing—how to use his hands—and at whatever age you put it that a boy or a girl should use the hand neatly and skilfully, before that age you should take care that some elementary grounding at all events, in the only training which can do this, shall have been given. No amount of Greek, or of useful or of useless geography, or even of rule of three, can prevent the fingers being all thumbs, unless some such training has been given, and for the very earliest training drawing is undoubtedly the best. But this is by no means the only advantage of the combination. Anyone who has to go over thousands of examination papers finds in nineteen cases out of twenty that an orderly drawing or diagram is generally associated with an orderly mind. In fact, a diagram may be regarded as an index of the amount and accuracy of the knowledge possessed by the student. The text of the student who fails in the diagram is generally a more awkward jumble than the diagram itself. Hence the facts show that this training of the hand is accompanied by much good mental results. This is now so generally recognised, that in a not distant period, no professor of biology, for instance, will attempt to demonstrate practically microscopic structure to students who have had no preliminary training in drawing. This is one example out of many which might be given, for as natural science is the study of nature, and as we can only study her by phenomena, the eye, and the hand, and the mind, must work together to achieve success, and he who attempts to describe the geology of a district, the minute structure of a frog's foot, an eclipse of the sun, or the rings of Saturn, in words, and words only, has only done half his work; to complete it he must appeal to art for aid. Now, many of you may be prepared to concede, without any further insistance on my part, that an elementary acquaintance with art is of great, nay, of even essential importance, not only for its own sake, but because of its aid in natural studies. We must then add art to science and literature in order to form a complete curriculum. Here pardon me one moment's digression from the direct line of my argument. Many will agree that science is aided by art who deny that art is aided by science to the same extent. Indeed, some are prepared to urge that one who proposes to devote himself to art can derive no possible benefit from the study of science. Let us inquire into this a little. If we wish to excel in the art of figure-painting, we must know anatomy, a most important branch of science; and as a matter of fact, many artists study anatomy as minutely as many surgeons do; and in the old days, when the artist and the poet were more saturated with the knowledge of the time than they are now, we find the great Leonardo at once professor of anatomy and founder of a school of painting as yet unsurpassed. If we pass from the figure to ornamental design, or if we wish to show objects in perspective, is not every line, whether straight or curved, dominated by an appeal to geometry? Again, suppose we take landscape. Here we meet with phenomena of colour as much regulated by law as are the phenomena of form, and an anatomy of colour is fast being formulated, which to the artist of the future will be as precious as the anatomy of form has been in the past, and will ever continue to be. Let us take, for instance, an artist who wishes to paint a sunset, one of the most magnificent sights which it is given to man to witness. The sky is covered with clouds here and there, and not only do the colours of the clouds vary, almost from moment to moment, but in all cases they present the strongest contrast to the colour of the sky itself. The artist is bewildered, and finds each effect that he would seize to be so transient that at last he gives up in despair the attempt to note down the various tints. But the possession of a knowledge of the part played by the lower strata of our atmosphere in absorbing now one and now another of the components of the light of the setting sun, would change this despair into a joy almost beyond expression. For the bewildering changes of colour are then discovered to be bound together by a law as beautiful as the effects themselves. There is another point of view. One is frequently pained in seeing in an otherwise noble work of art, evidences that the artist was crassly ignorant of the phenomena he attempted to represent, and in his attempts to transcend nature had only succeeded in caricaturing her, painting, for example, a rainbow in perspective, or a moon with its dark side turned towards the setting sun. Yet these are almost trifles, and, in fact, here we have the excuse of the ignorant artist—now, I am thankful to say, the representative of a class that is fast disappearing—for his defence is, that he has nothing to do with such small matters, and that accuracy of this kind may quite properly be sacrificed to secure the balance of his picture. Now, to return to the main drift of my address, we have seen that in any complete system of education neither science nor art must be neglected by the side of the old humanities—the old more purely literary studies; and it is indeed fortunate for us that we live in an age in which the laws and the phenomena of the external world have been studied and formulated with such diligence and success that it is as easy now to teach science, in the best possible way, as it is to teach classics in the best possible way. It is half a century since the Germans found out the importance of the new studies from a national point of view. We are now finding it out for ourselves, and finding it out not a moment too soon, and it is not needful for me to tell you that the transformation which is going on is acknowledged to be one of the highest national importance. It is no longer an abstract question of a method of education; it is a question of the life or death of many of our national industries, for, in a struggle for existence, how can a man who wins his bread by the application of national laws to some branch of industry, if he be ignorant of those laws, compete with the man who is acquainted with them? If for man we read nation, you see our present position. How far then have we got with our transformation, limiting our inquiry to primary and secondary instruction? First, as to elementary education. The idea of the education—the compulsory education, if necessary, of all the citizens in a state—dates from the time of Luther. It is a horrible thing that we should have had to wait three and a half centuries since his time for such a measure, which is an act of simple justice to each child that is brought into the world. In 1524 Luther addressed a letter to the Councils of all the towns in Germany begging them to vote money, not merely for roads, dykes, guns, and the like, but for schoolmasters, so that the poor children might be taught, on the ground that if it be the duty of a State to compel its able-bodied citizens to take up arms to defend the fatherland, it is *a fortiori* its duty to compel them to send their children to school, and to provide schools for those who, without such aid, would remain uninstructed. Thanks to our present system, now about ten years old, out of an estimated population of 8,000,000 children between the ages of two and fifteen, we had last year nearly four millions at school, and out of an estimated population of 4,700,000 between five and thirteen, we had 3,300,000 at school. Among this school population elementary science is at last to be made a class subject, and we find mechanics, mathematics, animal physiology, and botany among the specific subjects in addition to the three R's. 120,000 children received education in these specific subjects last year, and if we are justified in assuming that as many will learn science when it becomes a class subject as now already learn drawing, we may expect in a year or two to have this 120,000 swelled into three-quarters of a million. I must again insist upon the fact that practical teaching in science is the only thing that can be tolerated. Of course, with a new subject the great difficulty is the difficulty of the teacher. Any system, therefore, of economising teaching power is of the highest importance. I am glad to know that a system suggested by Col. Donnelly, which uses the utmost economy of teaching power, has been carried into admirable practical effect at Birmingham, and I believe also at Liverpool, and other large towns. So that in the most important centres we may be certain that science will be taught in the best manner. It is worth while to dwell on this system for a moment. Under it practical teaching is given to boys and girls of the fifth and higher standards, and also to the pupil teachers. The subject chosen for the boys is mechanics, that for the girls domestic economy, giving each of these subjects a wide range of meaning. There is a central laboratory in which the experiments are prepared, and from which the apparatus ready for use is conveyed in a light hand-cart to the various schools—twenty-six in number in Birmingham—belonging to the Board. In this way it is possible to give twenty lessons a week, and the circuit of the schools can be made in a fortnight. In the intervals between the visits of the demonstrator the class teachers recapitulate his lessons and give the children written examinations. About 1200 children are now being instructed in this way. To make the instruction as real as possible, children are brought out to aid in performing the experiments, objects are passed round, and questioning at the end of the lecture is encouraged. In the education, then, of our children, from the ages of five to thirteen, we may reasonably expect to find that science teaching will in the future be carefully looked after. We now come to the secondary education. Here, again, great progress has been made during the last few years. The real difficulties against its introduction have been the overcrowded state of the old curriculum, the scarcity of teachers, the want of sympathy with it, and the ignorance of its importance on the part of some headmasters. But to those headmasters who held the view that no real training could be got out of a subject which boys studied without positive pleasure, parents began to reply that whether the boy liked it or not he must get that knowledge somewhere. But where the experiment was really tried under good conditions it was soon found not only that the boys were willing to give three or four hours a week of their playtime to scientific subjects, but that the one or two hours filched from the curriculum were more than made up for by the greater ease with which the other subjects could be learnt, in consequence of the additional training of the mind which the new subjects gave. We may hope, then, that in the course of time our secondary education may be much improved in the direction indicated. What we may expect, taking the principle of natural selection as our guide will be this. First, the head-masters will themselves be men chosen among other grounds for their knowledge of science, they will become more and more all round men. Next, the curriculum will be arranged not for the few who go to the University, but for the many who do not. We shall have more science and less Greek in the early years of the school course. We shall have laboratories, and drawing rooms, and workshops. In some schools we may find modern living languages taught in a living way replacing the dead languages altogether. Now, here our difficulties begin. We are face to face indeed with the same difficulties which the Continental nations, our precursors in educational matters, have experienced. Our secondary education is at the present moment all but absolutely separated from the primary one. Of the 4,000,000 scholars on the books of elementary schools last year there were only 44,000 over the age of fourteen, and it is to be feared that the remainder left school at that age, most of them, the best as well as the worst of them, to fight the battle of life with such an education as they had got up to that time. Germany, again, was the first to find out that this would never do, even though in that country science and art was taught in the Primary School. And for the reason that though such a meagre education might possibly do for ordinary workers in their hives of industry, it was totally insufficient for the future foremen, overseers, and the like, and special schools were established to carry their education further. Quite of late years this question has been studied in the most interesting way in the Netherlands, under the advice of a wise minister, whose example will be followed some day in our own country. Let me briefly refer to it. This work began in 1863. In that year in Holland there were no middle class or secondary schools for artisans, but there were evening schools for drawing which dated from 1827. "Burgher Schools" were established to provide the secondary instruction still felt to be needed by those who otherwise would have to content themselves with the primary instruction (although in its more extended form it contained natural philosophy, mathematics, and modern languages). In these schools—some day, some night schools (in these the lessons went on from September to May), with a course of two or three years, we find mathematics, theoretical and applied mechanics, and mechanism, physics, chemistry, natural history, either technology or agriculture, drawing, gymnastics, and other subjects among the fixed subjects, modelling and foreign languages being permissive. These burgher schools were compulsory in all parishes of 10,000 inhabitants. The evening burgher schools especially were at once seized on with avidity, chiefly by apprentices and the like. Here let me give you some statistics which will show you how these schools were working even ten years ago. They are much more flourishing now, but I have not the figures. I will show how the Dutch (of whom it cannot be said, to vary an old rhyme,

> In matters of *learning* the fault of the Dutch,
> Is giving too little and asking too much.

for the instruction is practically free), who are already learning a trade or working at one, use the evening hours for the further cultivation of their minds.

	Population.	Number of students in Burgher Schools.
Delft	23,000	171
Utrecht	64,000	283
Deventer	81,000	285
Dordrecht	26,000	146

Among the students at these schools in 1874 were 1582 carpenters and joiners, 472 smiths, &c., 236 plumbers and masons, 170 goldsmiths, engravers, &c., 320 painters, to give examples. Higher burghar schools were also established in the chief towns. In these schools still more advanced instruction was given: and here the course was for five years. In all these schools there was a considerable state endowment, and an endowment on the part of the town, so that the fees were almost nominal, and in some cases even the instruction was gratuitous. When I was inspecting these schools in Holland with an eminent man of science, whose advice had helped largely to make them such a success, and when I expressed to him my astonishment at the smallness of the fees—only a very few shillings a year—he put before me the question of State aid to schools in a way which had never struck me before. He said: "We regard it as a sort of education insurance. A small tax is paid by everybody during the whole of his life, and in this way a man who brings up children for the service of the State is helped by him who shirks that responsibility; and the payment which each citizen is called upon to make towards this instruction is spread over his whole life, and does not come upon him when he is probably most pinched in other ways. Now for one practical result of the establishment of these schools. The year 1863 found Holland full of the notion that every hour a child spent away from the desk or the bench after thirteen was time wasted; but after these burgher schools were instituted a change came over the spirit of that dream, and now no employer of labour except of the lowest and most manual kind in Holland, will look at a boy who cannot produce a certificate from his burgher school. Another very remarkable thing was soon observed, with a most important moral for us. The great difference between their burgher schools and the old gymnasia, the equivalents of our grammar schools, was a greater infusion of science into the teaching, and the introduction of three modern languages in addition to Dutch, Latin and Greek being omitted altogether from the curriculum. After four years of this training, many of the boys showed such high promise that all connected with them thought it a pity that they should not enter a university. They were therefore allowed six months as an experiment to take up Latin and Greek, and the result was that in a great number of cases they beat the gymnasia boys in their own subjects, and passed with flying colours. The Real Schul in Germany and the modern sides of our own secondary schools are almost the exact equivalents of the higher burgher schools to which I have especially called your attention. What, then, is the experience which has been gained in these gigantic educational experiments, experiments by which we may profit, as we are so late in the race, if we care to do so. One point is that if a chance is put before those who have passed through the elementary schools of further culturing their minds, they seize upon it with avidity. Another is that the employers of labour appreciate the value of the greater intelligence thus brought about. It is better to have to instruct in a trade men who have shown themselves anxious to learn, than to have to do with blockheads. Another, I think, is this: Your best secondary school is best for everybody; a secondary school with a properly mixed curriculum of literature, science, and art, is best for him who proceeds either to the University or to the workshop. A second-rate education in a second-rate school, gives us a second rate man, and we do not want our national industries to be worked entirely by second-rate men. On this point I am glad to fortify what I have said by a reference to Dr. Siemens' important address at the Midland Institute the week before last. He says: "It is a significant fact that while the thirty universities of Germany (you see they do not educate by halves in Germany; they have seven times as many universities as we have in England) continued to increase, both as regards number of students and high state of efficiency; the purely technical colleges, almost without exception, have during the last ten years been steadily receding, whereas the provincial Gewerbe Schuls have, under the progressive minister, von Falke, been modified so as to approximate curriculum to that of the gymnasium or grammar school. "As regards middle-class education, it must be borne in mind that at the age of sixteen, the lad is expected to enter upon practical life, and it has been held that under these circumstances at any rate it is best to confine the teaching to as many subjects only as can be followed up to a point of efficiency and have reference to future application. It is thus that the distinction between the German gymnasium or grammar school and the real Schule or technical school has arisen, a distinction which, though sanctioned to some extent in this country, also by the institution of the modern side, I should much like to see abolished." We see then the gradually increasing weight of opinion, and the result of the experiments both in Germany and Holland, and I may add France, point to these conclusions. Some kind of secondary education must be provided for the best students when they leave the elementary school, either before they begin work or while they are at work. Our secondary education should go practically along one line, how far soever the student goes along that line, some, of course, will go further than others; *provided always that our secondary education is the best possible, that is, having the broadest base.* Now, if this be generally conceded our problem in England, at the present moment, is simpler than we thought it. We are face to face with the fact that it is for the good of the nation that those who have passed most successfully through the elementary education must continue that education in a secondary school, whether for two, or for three, or for six years, matters little for the argument. Are we then to build technical schools for such students? Thirty years ago the answer would have been yes. To-day we may say firmly, no. If a town has a grammar-school, let the town see that the curriculum of that school is based upon our best secondary models If the town has no such school, then let it build one. If one school is not sufficient, then build two. That town will be the best off in the long run which gives the greatest number of free exhibitions from the elementary schools into such a school as this, and that town will be the wisest which holds out such inducements at the earliest possible moment. I have lately read with much interest a copy of resolutions and suggestions, passed at a meeting of an Association of Elementary Teachers in the north of England. From these we may gather that this question is already one of practical politics. It is agreed that the secondary education of the best boys leaving the elementary schools must also on. It is also taken for granted that the question lies between building a technical school or utilising the grammar school. One argument used in favour of the latter cause is, that the grammar school will be strengthened by drawing to itself the best boys from the elementary schools. The present proposals are that a number of free scholarships should be competed for annually, that these free scholarships shall, if need be, be supplemented by exhibitions from the fund at the disposal of the Governors (I should not accept this at once. Why should not the town pay them?), and the length of time for which these scholarships shall be tenable is not to be less than three years. You see, then, that in the north of England, at all events, it is conceded that the best children in our elementary schools should have a three years' course in a school of higher grade in which, of course, all the class subjects in the Elementary Code will be expanded, and all the linguistic studies of the grammar school taken in hand. When this system is at work, as it is bound to be in a few years, two things will happen, and it is as well we should be prepared for them. In the first place, our secondary schools—all of one model, the best model, be it understood—must so arrange its curriculum, that the students can leave after a three years' course, if need be, for the workshop or the office, or after a longer course for the University. That is the first point. The second one is this. The present system of apprenticeship will be called in question. A boy who has been educated to the age of sixteen will learn very much more in three or four years, and will be very much more valuable to his master during that time than he who was formerly bound apprentice at the age of thirteen or fourteen, with his fingers all thumbs, and no mind to speak of. It seems to me as it does to a daily increasing number, that the present mode of dealing with those matters which were formerly regarded as arts and mysteries known only to a few, and carried on on a small scale under the eye of the master, is dead against the system of apprenticeship as it has come down to us. Now the master does not teach, and the boy in nine cases out of ten has no opportunity of grasping the whole of the art or mystery at all. Many of you will begin to think that you are listening to the play of Hamlet with the part of the Prince of Denmark omitted, for so far I have said nothing whatever about technical education. I have said nothing about it for the reason that I believe the less said to a boy about technical education before he is sixteen years old the better. I now proceed to discuss this question, which is far more important, far more a national question, than you would gather from the debates in Parliament. What is technical education? It is the application of the principles of science to the industrial arts. And the rock ahead against which I am anxious to join Dr. Siemens in warning you is this: Under the influence of the present scare—for it is a scare, and a real one—there is a chance that attempts may be made to teach the applications to those who are ignorant of principles, whereas we have to fight those who study applications with a full knowledge of the principles which underlie them. We may congratulate ourselves on the fact that when we have once made up our minds as to the right place of technical instruction in our scheme of education, we have much of the necessary machinery already at our disposal; and the recent action of the City Guilds and of the Government is enormously increasing the quantity and improving the quality of this machinery. Let us first consider the classes now formed all over the country under the auspices of the Science and Art Department. Their development in the last thirty years has been something truly marvellous. When the Queen, in 1852, opened Parliament, there were already 35,000 students of art, but practically no students of science, in this country, amongst the industrial classes. That 35,000 will, if the present progress goes on, give us nearly 1,000,000 students of art at the end of this year; while the science schools have increased from 82 in 1860 to 1400 in 1880, with 69,000 students. The system which has thus developed so enormously has dealt chiefly with pure science, but for the future we shall have side by side with it, and built upon the same lines, a system of teaching the applications of this pure science to each of our national industries. He who wishes in the future to have to do in any way with the manufacture of alkali, gas, iron, paper, or glass, to take instances, or in the dyeing of a piece of silk, or the making of a watch, to take others, will find the teaching brought to his door, and obtainable almost for the asking. Here, again, we may congratulate ourselves, for while those who know most about the subject tell us that the more ambitious attempts at technical instruction in Germany and elsewhere have failed, because the teaching is not in sufficiently close contact with the works in which the processes are actually carried on, the system to which I have drawn your attention will enable the instruction to be given at night to those who have already begun practical work during the day. We have, then, come to this: that putting together what is most desirable in the abstract, and what has been practically proved to be the best, the education of our industrial classes should be, and can easily be, something like this. The boy will go to an elementary school till he is thirteen. He will then pass with an exhibition, if necessary, to a secondary school till he is sixteen. He will there go on with his science—now a class subject in the elementary school—and begin the study of languages. At sixteen he will leave school and begin the battle of life, and can still in the evening proceed further with his studies in pure science, if the secondary education has left him too ill-equipped in that direction. Having thus got the principles of pure science into his mind he will be able to take up the technical instruction in the particular industrial art to which he is devoting himself. But be the number of our future foremen and managers who who have had this extra three years of secondary instruction, large or small, if there be in Coventry let us say out of your population of 45,000, one thousand boys, or girls, or men, who are anxious not only to learn science, but its application to their particular industries, then the Government is ready to endow Coventry with a sum varying from two thousand to six thousand pounds a year, according to the results of the examinations, if two subjects of pure science are taken up, and the students pass. The City Guilds are prepared to endow the town with from 1000*l.* to 2000*l.* a year additional, provided some application of the principles of science to the industrial arts is taken up, and evidence forthcoming that the principles themselves have been studied. Now if among your 45,000 there is not 1000 who care for these things which are vital to your trades, seeing that abroad these things are cared for, how can your trades stand against foreign competition? Let such a system as this go on for twenty years, and we shall hear nothing more of the decay of our national industries. Now here I am bound to point out a distinct gap in the present system. We have classes for art, classes for pure science, classes for applied science, but where are the classes for languages? The modern languages are taught so badly in our secondary schools, that it is hopeless to expect that sufficient knowledge, either of French or German can be acquired in the three years' course to enable the student to find out what his French and German rivals are doing in the branch of industry which he takes up; and we must, moreover, consider those who may wake up to the importance of studying science and its technical applications after the chance of a secondary education is lost. Such classes then are a real want. But I will not end my address by a reference to what I regard as an unfortunate gap, but would rather conclude what I have to say by pointing out that the scheme I have sketched out need be no Utopia, so far, at all events, as a supply of well-trained teachers is concerned. This, up to the present time, has been the real difficulty. But now that the authorities at South Kensington have started summer courses of lectures to teachers, and that they actually pay the teachers for going to learn, the methods of teaching, both in the elementary and secondary schools, and evening classes, cannot fail to improve. Quite recently, too, we have seen the inauguration of a Normal

School, where Royal Exhibitioners and other free students are admitted without payment; where the teacher has the first claim, and where he can attend any single course for a nominal fee. Now every town of importance in the country should associate itself with the Government in this attempt, and should have one, at least, of its citizens always in training there, so that the scientific instruction in that town, whether primary, secondary, or tertiary, should always be at its highest level. On the other side of the road, too, at South Kensington, is rapidly rising another institution where we may hope the teachers of our technical instruction will receive an equally careful training. So that you see, to bring what I have to say to a conclusion, that though we are late in the day, though many people have not yet made up their minds as to what is best to be done—and I acknowledge that the question is hedged in with difficulties on all sides—there is an easy solution of the difficulty based on the experience of other countries, which is at the same time an act of simple justice; that this solution requires no dislocation if we adopt it, but simply a natural growth of our existing means, and that all the newest developments of our educational machinery will all fall naturally into place.

THE TRANSIT OF VENUS[1]

The Observations at the Cape

THE long looked-forward-to transit of Venus occurred yesterday afternoon, causing, we may be sure, a flutter of excitement amongst astronomers throughout the whole of the world. To some the special duty was entrusted of carefully noting everything connected with the ingress of this familiar planet, and after they had concluded their labours at the setting of the sun, it fell to astronomers in other portions of the globe to pay equally minute attention to the planet's egress. By and bye we may expect columns of thoughtfully worked-out details in connection with this peculiar and interesting astronomical event, all of which will tend to still further solve the problem of the exact distance of the sun from the earth. We need not remind our readers that herein consists the whole scientific value of the transit. When crossing the sun's disc the planet is at its nearest distance from the earth—estimated at about 25,000,000 miles—and through the peculiar facilities thus afforded of directly measuring its parallax, observers are enabled to calculate the parallax of the sun, which to astronomers is a matter of very considerable importance. The credit of the suggestion of this particular method of calculation is due to Dr. Halley, and it is still popularly held to be the best for the purpose. But accompanying the rapid strides astronomic science has taken in its development since the days of Halley, instrumental means have been invented and accepted by modern astronomers, which appear to afford methods, perhaps even more exact, of arriving at the desired result. For all this, however, the transit of Venus retains a powerful hold upon the popular mind, and, indeed, upon the minds of many astronomers, as the best method. There is, too, one specially strong argument why a particular interest should be taken in this planet's transit. No one who witnessed the phenomenon yesterday will live to see it again,—unless, indeed, he fairly outrivals old Parr and other gentlemen famed for longevity. Occurring as these transits do at the unequal but regular recurring intervals of 8, 122, 8, and 105 years, no one could well expect to see more than two in a lifetime. The last took place in 1874, while the next will occur in December, 2007. It need, therefore, be no longer surprising why, both popularly and scientifically, the event is regarded as one of such special interest, and why the most eminent scientific observers are selected to note everything that takes place.

Before proceeding to refer to the observations which were taken yesterday at the Royal Observatory we may mention that, acting under the advice of the Astronomer-Royal of the Cape of Good Hope (Dr. Gill), the British Transit of Venus Committee decided upon establishing stations at Aberdeen Road and Montagu Road as auxiliary places of observation to the principal station here at the Observatory itself. And before proceeding further it may be added that Natal has come forward very pluckily in this matter, exhibiting an amount of interest in astronomic science which does great credit to that colony. Mr. Escombe himself contributed a sum of between 400*l.* and 500*l.* for the purpose of providing a proper telescope; while two merchants subscribed 50*l.* each, the Corporation of Durban giving 300*l.*, and the Natal Government voting 500*l.* towards founding an observatory for the colony, and the defraying of expenses connected with taking observations of the present transit. As a pleasant sequel to this, we are glad to learn by telegraph, that Mr. Neison, who was in charge of the party of observation there, most successfully observed the internal contact at Durban, the enterprise of Natal thus meeting with a well-merited reward. As announced by us some time since, South Africa was selected by the Americans as a station for one of their photographic transit of Venus expeditions under the charge of Prof. Newcomb, who has the reputation of being one of the most celebrated of living astronomers. On arrival here, Prof. Newcomb, after consultation with the best authorities as to atmospheric conditions, &c., finally decided, with the kind consent of the trustees ofthe Huguenot Seminary to take his observations from the foot of the gardens of that institution at Wellington. We hope to shortly hear of the entire success of the labours of the party, and perhaps to see some specimens of their photographic skill.

At the Observatory itself it need scarcely be said that for some weeks past great preparations had been made for the event. There are few living astronomers who have more carefully studied the subject of the transit of Venus than the present Astronomer-Royal here, Dr. Gill, and few are more thoroughly posted up in all the details of the rare occurrence. In 1874 Dr. Gill was Chief Astronomer to Lord Lindsay's Transit of Venus Expedition to the Mauritius, where he not only took most valuable observations, but evinced a very intimate acquaintance with the entire subject. It was only to be expected, therefore, that in this instance no detail in connection with the arrangements for a proper observation in Cape Colony would be lost sight of by the Astronomer-Royal. The few visitors who received invitations to the Observatory yesterday found Dr. Gill courteous and affable as ever, but wholly absorbed in the important work on hand. "You may go here and go there, look through that glass and have a peep through the other one," were his remarks just before commencing operations, "but whatever you do, please don't speak to me or any of the observers until the internal contact has been made." No injunction not to speak to the "man at the wheel" could have been more respected than this, and from that moment until a couple of hours later Dr. Gill and his assistants became objects of almost reverential awe to those outside the pale of strict astronomic science.

One of the principal instruments employed was a new equatorial telescope by Grubb of Dublin, made and sent out here specially for the transit of Venus, the old wind tower in which it is now mounted having been prepared as an observatory for its reception. There was also a heliometer which had been used at the last transit by Dr. Gill at the Mauritius, and was afterwards borrowed by him from Lord Lindsay for use on the Isle of Ascension, where he made a determination of the sun's distance from the planet Mars. Subsequently this fine instrument was purchased by Dr. Gill and was brought out here as his private property on his being appointed Astronomer-Royal at the Cape. Another noticeable instrument employed yesterday was the great theodolite intended for the trigonometrical survey of India. The designs of Col. Strange, however, from which it was constructed, were so long in being carried out in manufacture that General Walker, the Director of Survey, decided not to bring it into use, especially as it was somewhat too heavy for service in the field. Upon the application of Dr. Gill, it was lent by the Indian Government, for the purpose of some special researches in which that gentleman was engaged at the time, and it was successfully employed the other day in taking observations of the great comet. The other instruments included a small equatorial telescope of 3½ inches aperture, which was used by Mr. Stone on the occasion of the last transit of Venus; an equatorial telescope of 7 inches aperture, which has also been for some time at the Observatory, and a telescope of 2½ inches aperture belonging to Capt. Jurisch, examiner of diagrams in the Surveyor-General's department. Having mentioned the several instruments, we must go on to state by whom they were used. Dr. Gill himself observed the contact of Venus with the sun's limb, with the new 6-inch aperture equatorial, a similar observation being taken by Mr. Maclear with the 7-inch equatorial. Dr. Elkin, a scientific friend and guest of the Astronomer-Royal, took observations with the heliometer; Mr. Freeman, with the great theodolite; Mr. Pillans, with the small equatorial; and Capt. Jurisch with his own equatorial. Several important measures were also taken at the heliometer by Dr. Gill and Dr. Elkin.

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[1] From the *Cape Times*, December 7, 1882.

With regard to the weather, which of course was a very important element, the sky was perfectly clear, and altogether suitable for the purpose of observation. There was a light south-east wind blowing, and this prevented the definition being so steady as might have been wished. We are "officially" assured, however, that on the whole the observations made at the Cape of Good Hope may be regarded as perfectly satisfactory, and that they will add considerably towards the solution of the problem of the sun's exact distance from the earth. We have already intimated that at the suggestion of Dr. Gill, other stations than that of the Observatory had been selected. At Aberdeen Road, Mr. Finlay (of comet fame), the first Assistant at the Cape Observatory, and Mr. Pette, third Assistant, were provided with an equatorial of six inches aperture, and the report received last evening by telegraph, was that complete success had attended their labours. Mr. Marth, the well-known astronomer, was detailed at Montagu Road in charge of one of the British Transit of Venus Expeditions, and was provided with a 6-inch aperture equatorial, his assistant, Mr. Stephen, formerly of the Observatory, and now of the Treasury Department, Cape Town, being provided with a 4½-inch equatorial. In his report last evening, Mr. Marth states that the sky was cloudless, but a heavy dust-storm prevailed during the day. He reported, however, that the important internal contact was observed satisfactorily both by himself and Mr. Stephen. A report from Capt. Skead, in conjunction with Mr. Spindler, of Port Elizabeth, states that they also obtained satisfactory observations.

We fear that the courtesy of the General Manager of Telegraphs, Mr. Sivewright, must have been sorely tested by the frequent demand upon his staff for signals for the purpose of determining longitudes, &c. The telegraphic department, we ought to state, has given the utmost facilities in connection with these operations, and thanks to the co-operation of the General Manager, everything connected with his department was accomplished without a hitch. The transit of Venus expedition will indeed be indebted to Mr. Sivewright for his energy and devotion in their interests. This additional work has necessarily fallen heavily upon the shoulders of the staff at the Observatory. Not only has the normal work of that establishment been carried on as diligently as heretofore, but there has been the additional task of taking observations of the great comet, which with other things has told severely upon the endurance of Dr. Gill and his assistants. Judging, though, from what we saw there yesterday, there is no sign of anyone breaking down under the strain of extra work.

The signals for time comparison were sent to the observers engaged in the transit about nine o'clock on Tuesday evening The night is described as having been beautifully clear, and the occultation of the bright star Spica Virginus was observed in the early morning. Signals were also sent to Mr. Eddie, Graham's Town.

We have thus far briefly sketched the manner in which the observations were taken yesterday—excepting the somewhat primitive method of smoked glass adopted by a good many of the general public, to whom the transit of Venus was not quite such a matter of exquisite nicety as to such gentlemen as those to whom we have just alluded. From a non-astronomic point of view there was, even with the aid of the proper instruments, only to be seen a dark spot crossing the sun, resembling very much a Wimbledon bull's eye. Roughly speaking, the planet made its external contact at five minutes past three o'clock, when through a proper instrument it might have been seen minutely notching into the sun's edge. At twenty-five minutes past the hour—still roughly speaking, for when the calculations are worked out there might be a fractional part of a second one way or the other—the internal contact occurred.

The sun set long before the transit had been completed. It consequently fell to the lot of other astronomers to observe its egress, which of course was as eagerly watched for as had been that of the ingress. The ingress, it might be interesting to mention, was visible in North and South America; Europe, excepting the west of Russia and the north of Norway and Sweden; the whole of Africa, Madagascar, Seychelles, and the Mauritius. The egress was visible in North and South America, Australia, New Zealand, and nearly the whole of the South Pacific. This egress will have been completed by about eight o'clock this morning, and then all interested in the subject of Venus may look forward to another 122 years before the interesting occurrence again takes place.

ELECTRIC RAILWAYS[1]

We have grown so accustomed to the regular announcement—"serious—accident on such and such a railway, several passengers injured" that we have almost come to regard railway accidents as inevitable, just as parents mistakingly think the measles and whooping cough necessary accompaniments of childhood. But speed no more means disaster than a densely crowded city means disease. The first effect of overcrowding is undoubtedly to produce fever and other complaints. If, however, the knowledge and practice of the laws of hygiene increase more rapidly than the population of a town, the death-rate, as we have seen, diminishes, instead of augmenting. And so it is with locomotion; the stage-coach journeys of our ancestors were slow enough for the most staunch conservative, and yet the percentage of the passengers injured on their journeys was far greater than even now with our harum-scarum railway travelling. The number of passengers has increased enormously, but the safety has increased in an even greater rate. If then we can devise methods introducing still greater security, a far larger number of passengers may travel at a far greater speed and with less fear of danger than at present.

Accidents constitute one charge against railway conveyance, but there is another, and that is the cost. Cheap as railway travelling now is, compared with the departed stage-coach locomotion, the price of the tickets is still far too high for railways to fulfil, even in a small degree, one of their most important functions, and that is transporting labourers from parts of the country where labour is scarce, to others where it is abundant and labourers in demand.

But how is a happier state of things to be realised? We cannot expect the railway companies to lower their fares merely to benefit humanity. If, however, we can prove to them that the present system of railways is neither the most remunerative to themselves nor the most beneficial to the community at large, we may hope to win the attention of railway directors, whose stock question is, and quite rightly, "Will it pay?"

Those of you who have read the life of Stephenson know what a protracted fight he had to carry one of his most cherished ideas, and that was the employment of a locomotive engine to draw the train, instead of a stationary engine to pull it with ropes or chains. His adversaries saw the disadvantage of adding the weight of the locomotive to the weight of the train, whereas Stephenson was especially struck with the enormous waste of power in the friction of ropes or chains passing over pulleys. [Experiments were then shown proving, *first*, that the mass of the locomotive necessitated the engine having a greater horse-power to get up the speed of the train quickly as well as a greater horse-power to keep up the speed; *secondly*, that the friction and wear and tear of ropes, such as were employed on the London and Blackwall Railway, would have been an insuperable hindrance to the development of railways.] From this was deduced that, since in Stephenson's day the only feasible mode of communicating the power of a stationary engine to a moving train was by means of ropes, his decision to adopt the locomotive was perfectly correct at the time it was made.

Attempts have been made to propel trains by blowing them through tubes, or by blowing a piston attached to the train through a tube, but such attempts at pneumatic railways have nearly all been abandoned. The employment of air compressed into a receiver on the train by fixed pumping engines stationed at various points along the line, and employed to work compressed air engines on the carriages has been effected with considerable success by Col. Beaumont, especially for tram-lines. The weight of the compressed air-engine is, however, still very considerable. Any system of pumping water through a pipe and employing the water to work a hydraulic engine on the train is hardly worth considering, seeing that the mechanical difficulties of keeping up a continuous connection between the moving train and the main through which the water is pumped seem insuperable. Gas-engines worked with ordinary coal-gas, stored perhaps under pressure, might be employed on the moving train, but the advantage arising from the absence of boiler and coal would be more than compensated for by the fact, that the weight of a gas-engine per horse-power developed is so much greater than that of a steam-engine. None of these systems, then, of dispensing with a locomotive is by any means perfect, and the success of the recent experiments on the electric transmission of power has turned the attention of engineers to the consideration, whether electricity could not successfully supplant steam for the propulsion of trains and tram-cars; whether it could not, in fact, supply an efficient means of transmitting power, the absence of which caused Stephenson to abandon ropes in favour of a heavy locomotive engine.

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[1] Abstract of a lecture at the Royal Institution by Prof. W. E. Ayrton, F.R.S.

The whole question, like every similar one, is mainly a question of expense; and what we have to consider is, whether electric transmission on the whole leads to greater economy than can possibly be obtained by the employment of any kind of locomotive. The average weight of a locomotive is about that of six carriages full of people; ten carriages compose an ordinary train, hence the presence of the mass of the locomotive adds at least 50 per cent. to the horse-power absolutely necessary to propel the carriages alone, and therefore at least 50 per cent to the amount of coal burned. But there is another most serious objection to the engines, perhaps even more important than the preceding. The heavy engine passing over every part of the line necessitates the whole line and all the bridges being made many times as strong, and therefore many times as costly, and the expense of maintenance consequently also far greater, than if there were no locomotive. And it is not possible to make the engine much lighter; for it would not have then sufficient adhesion with the rails to be able to draw the train; in fact, you cannot diminish the weight as long as the train is propelled with only one or two pair of driving wheels as at present. The employment of electricity, however, will enable a train to be driven with every pair of wheels, just as the employment of compressed air enables every pair of wheels to brake the train.

To propel a train, we must either utilise the energy of coal by burning it, or use the energy possessed by a mountain stream, or the energy stored up in chemicals, and which is given out when the chemicals are allowed to combine, or we must employ the energy of the wind. Practically we employ at present only the first store for propelling railway trains—the potential energy of coal; and that is to a great extent the store on which we shall still draw, even when we employ electric railways. For experience shows that, with the modern steam-engine and dynamo, at least one-twentieth of the energy in coal can be converted into electric energy; and that this is at least twenty times as economical as the direct conversion of the energy of zinc into electric energy by burning it in a galvanic battery.

But it may be asked, did not Faraday's discovery, in 1831, that a current could be produced by the relative motion of a magnet and a coil of wire, settle this point half a century ago? Theoretically—yes; practically, however, the problem was very far from being solved, because the dynamo machine was very unsatisfactory, and it was not until Pacinotti, in 1860, suggested the solution of the problem of obtaining a practically continuous current from a number of intermittent currents, and until Gramme, about 1870, carried out Pacinotti's suggestion in the actual construction of large working machines, that the mechanical production of currents became commercially possible. [Experiments were then shown illustrating the complete electric transmission of power, a gas-engine on the platform giving rapid motion to a magneto-electric machine, and the current thereby produced sent through an electro-motor at the other end of the room, which worked an ordinary lathe.]

In electric transmission of power there is not only waste of power from mechanical friction, but also from electric friction arising from the electric current heating the wire, through which it passes.

It was then explained and demonstrated experimentally that this latter waste could be made extremely small by placing so light a load on the electro-motor, that it ran nearly as fast as the generator or dynamo, which converted the mechanical energy into electric energy; actual experiments leading to the result that for every foot-pound of work done by the steam-engine on the generator, quite seven-tenths of a foot-pound of work can be done by the distant motor.

One reason why electric transmission of power can be effected with so little waste is because electricity has apparently no mass, and consequently no inertia; there is, therefore, no waste of power in making it go round a corner, as there is with water or with any kind of material fluid. Another reason why electro-motors are so valuable for travelling machinery is on account of the light weight of the motor. Experiment shows that one horse-power can be developed with 56 lbs. of dead weight of electro-motor, and that for large electro-motors of several horse-power the weight per horse is even much less; a result immensely more favourable than can be obtained with steam, gas, or compressed-air-engines.

In addition to the loss of power arising from the heating of the wires by the passage of the current, there is another kind of loss that may be most serious in the case of a long electric railway, viz.; that arising from actual leakage of the electricity due to defective insulation. To send an electric current through a distant motor, two wires, a "going" and "return" wire must be employed, insulated from one another by silk, guttapercha, or some insulating substance; and if the motor be on a moving train, there must be some means of keeping up continuous connection between the two ends of the moving electro-motor and the going and return wire. The simplest plan is to use the two rails as the two wires, and make connection with the motor through the wheels of the train; those on one side being well insulated from those of the other, otherwise the current would pass through the axles of the wheels, instead of through the motor. It is this simple plan that is employed in Siemens' Lichterfelde Electric Railway, now running at Berlin; the insulation arising from the rails being merely laid on wooden sleepers having been found sufficient for the short length, 1½ mile. The car is similar to an ordinary tram-car, and holds twenty passengers. [Photographs were then projected on the screen of this and of the original electric railway laid by Siemens in the grounds of the Berlin Exhibition of 1879, and exhibited in 1881 at the Crystal Palace, Sydenham.] It was explained that on this latter railway, which was 900 yards long, both the ordinary rails were used as the return wire, and that the going wire was a third insulated rail rubbed by the passing train. [Photographs were then projected on the screen of Siemens' electric tram-car at Paris, used to carry fifty passengers backwards and forwards last year to the Electrical Exhibition.] In this the going and return wires were overhead and insulated, connection being maintained between them and the moving car by two light wires attached to the car, and which pulled along two little carriages running on the overhead insulated wires, and making electric contact with them. [Experiments followed, proving that although two bare wires lying on the ground could be quite efficiently employed as the going and return wire, if the wires were short and the ground dry, the leakage that occurred if the wires were long and the ground moist was so great, as to more than compensate for the absence of the locomotive.] Consequently Prof. Perry and myself have for some time past been working out practical means for overcoming these difficulties, and we have arrived at what we hope is an extremely satisfactory solution. Instead of supplying electricity to one very long, not very well insulated rail, we lay by the side of our railway line a well insulated cable, which conveys the main current. The rail, which is rubbed by the moving train, and which supplies it with electric energy, we subdivide into a number of sections, each fairly well insulated from its neighbour and from the ground; and we arrange that at any moment only that section or sections, which is in the immediate neighbourhood of the train, is connected with the main cable; the connection being of course made automatically by the moving train. As then leakage to the earth of the strong propelling electric current can only take place from that section or sections of the rail, which is in the immediate neighbourhood of the train, the loss of power by leakage is very much less than in the case of a single imperfectly insulated rail such as has been hitherto employed, and which being of great length, with its correspondingly large number of points of support, would offer endless points of escape to the motive current.

Dr. Siemens has experimentally demonstrated that an electric railway can be used for a mile or two; Prof. Perry and myself, by keeping in mind the two essentials of success, viz. attention to both the mechanical and electrical details, have, we venture to think, devised means for reducing the leakage on the longest railway to less than what it would be on the shortest.

For the purpose of automatically making connection between the main well-insulated cable and the rubbed rail in the neighbourhood of the moving train we have devised various means, one of which is seen from the following figures.

A B (Fig. 1) is a copper or other metallic rod resting on the top of and fastened to a corrugated tempered steel disc D D (of the nature of, but of course immensely stronger than the corrugated top of the vacuum box of an aneroid barometer), and which is carried by and fastened to a thick ring E E made of ebonite or other insulating material. The ebonite ring is itself screwed to the circular cast-iron box, which latter is fastened to the ordinary railway sleepers. The auxiliary rail AB and the corrugated steel discs DD have sufficient flexibility that two or more of the latter are simultaneously depressed by an insulating collecting brush or roller carried by one or by all of the carriages. Depressing any of the corrugated steel discs brings the stud F, which is electrically connected with the rod AB, into contact with the stud G electrically connected with the well-insulated cable.

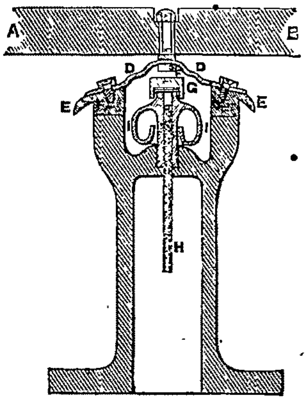

FIG. 1.

As only a short piece of the auxiliary rail AB is at any moment in connection with the main cable, the insulation of the ebonite ring EE will be sufficient even in wet weather, and the cast-iron box is sufficiently high that the flooding of the line or the deposit of snow does not affect the insulation. The insulation, however, of G, which is permanently in connection with the main cable, must be far better. For this purpose we lead the gutta-percha, or india-rubber, covered wire coming from the main cable through the centre of a specially formed telegraph insulator, and cause it to adhere to the inside of the earthenware tube forming the stalk. And as, in addition, the inside of each contact-box is dry, a very perfect insulation is maintained for the lead coming from the main cable. Consequently as all leakage is eliminated except in the immediate neighbourhood of the train, this system can be employed for the very longest electric railways. Fig. 2 shows a modification of the contact box when the insulated rail L, instead of extending all along the line, is quite short and is carried by the train, and by its motion presses forwards and downwards a metallic fork on the contact-box, thus making contact between F and G. [Other diagrams were explained, illustrating modifications of the contact-boxes; in one case the well-insulated cable is carried inside the flexible rail, which then takes the form of a tube, shown in Fig. 3; in another case the cable is insulated with paraffin oil instead of with gutta-percha or india-rubber, shown in Fig. 4, &c.]

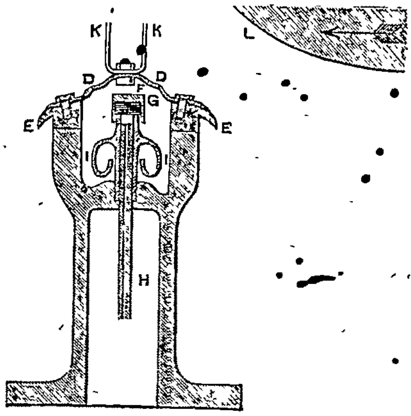

FIG. 2.

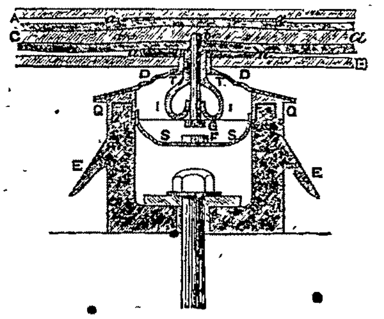

FIG. 3.

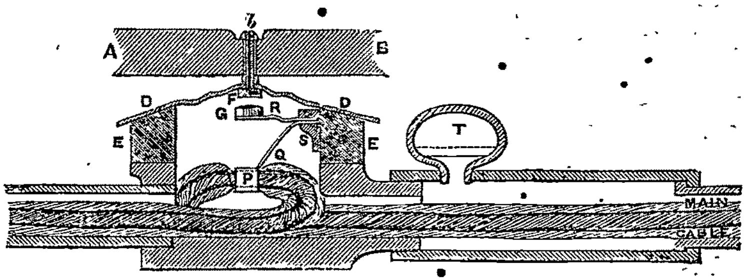

FIG. 4.

The existence of these contact boxes at every 20 to 30 feet also enables the train to graphically record its position at any moment on a map hanging up at the terminus, or in a signal-box or elsewhere, by a shadow which creeps along the map of the line as the train advances, stops when the train stops, and backs when the train backs. This is effected thus:—As the train passes along, not only is the main contact between F and G automatically made, as already described, but an auxiliary contact is also completed by the depression of the lid of the contact-box, and which has the effect of putting, at each contact-box in succession, an earth fault on an insulated thin auxiliary wire running by the side of the line. And just as the position of an earth fault can be accurately determined by electrical testing at the end of the line, so we arrange that the moving position of the earth fault, that is the position of the train itself, is automatically recorded by the pointer of a galvanometer moving behind a screen or map, in which is cut out a slit representing by its shape and length the section of the line on which the train is, as shown in Fig. 5. In addition, then, to the small sections of 20 feet or more into which our auxiliary rubbed rail is electrically divided, there would be certain long blocked sections one mile or several miles in length, for each of which on the map a separate galvanometer and pointer would be provided. [Experiments were shown of the system of graphically automatically recording the progress of a train.]

In the preceding systems there are several contact-boxes in each section of the insulated rubbed rail, and several sections of the insulated rail in each section of the line blocked, but in the next system the rubbed rail is simply divided electrically into long sections each of as great a length as the particular system employed to insulate the rubbed rail will allow. In this case we arrange that the electric connection between the main cable and the rubbed conductor shall be automatically made by the train as it enters a section, and automatically broken as the train leaves a section. The model before you is divided into four sections, each about 11 feet in length, and you see from the current detectors that as the train runs either way, it puts current into the section just entered, and takes off current from the section just left.

[Experiments were then shown of the ease with which an electric train could be made to back instead of going forwards, by reversing the connections between the revolving armatures and the fixed electro-magnets of the motor; also that the accidental reversal of the field magnets of the main stationary generator, although it had the effect of reversing the main current, produced no change in the direction of motion of an electric engine, the direction of motion being solely under the control of the driver.]

But more than this, not only does the train take off current from the section 1 when it is just leaving it, and entering section 2, but no following train entering section 1 can receive current or motive power until the preceding train has entered section 3. [Experiments were then shown proving that with this system a following train could not possibly run into a preceding train even if the preceding train stopped or backed.] Now why does the following train when it runs on to a blocked section pull up so quickly? The reason is because it is not only deprived of all motive power, but is powerfully braked, since when electricity is cut off from a section, the insulated and non-insulated rail of that section are automatically connected together, so that when the train runs on to a blocked section the electromotor becomes a generator short circuited on itself, producing, therefore, a powerful current which rapidly pulls up the engine. [Experiments were then shown of the speed with which an electromotor, which had been set in rapid rotation and then deprived of its motive current, pulled up when its two terminals were short-circuited.]

Whenever, then, a train, it may be even a runaway engine, enters on a blocked section, not only is all motive power withdrawn from it, but it is automatically powerfully braked, quite independently of the action of the engine-driver, guard, or signalman. No fog, nor colour-blindness, nor different codes of signals on different lines, nor mistakes arising from the exhausted nervous condition of overworked signal-men, can with this system produce a collision. The English system of blocking is merely giving an order to stop a train; but whether this is understood or intelligently carried out is only settled by the happening or non-happening of a subsequent collision. Our Absolute Automatic Block acts as if the steam were automatically shut off and the brake put on whenever the train is running into danger; nay, it does more than this—it acts as if the fires were put out and all the coal taken away, since it is quite out of the power of the engine-driver to re-start his train until the one in front is at a safe distance ahead.

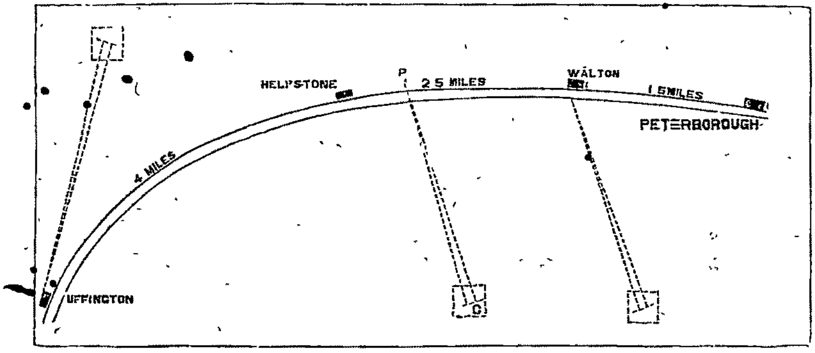

FIG. 5.

But all trains will undoubtedly be lighted with electricity; must, then, the train be plunged into darkness when it runs on to a blocked section to which no electric energy is being supplied? No! If some of the electric energy supplied to a train when it is on an unblocked section be stored up in Faure's accumulators, such as are at present used on the Brighton Pulman train, the lamps will continue burning even when the train has ceased to receive electric energy from the rubbed rail.

When, then, we commit the carrying of our power to that fleet messenger to which we have been accustomed to entrust the carrying of our thoughts, then shall we have railways that will combine speed, economy and safety; and last, but not least to us Londoners, we shall have the entire absence of smoke, the presence of which nearly causes the convenience of the Underground Railway to be balanced by the pernicious character of its atmosphere.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 21, 1882.—"On the Origin of the Hydrocarbon Flame Spectrum." By G. D. Liveing, M.A., F.R.S., Prof. of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Prof., University of Cambridge.

In previous communications[1] to the Society we have described the spectra of what we believe to be three compound substances, viz., cyanogen, magnesium-hydrogen, and water.

In these investigations our chief aim has been to ascertain facts, and to avoid as far as possible adopting any special theory regarding the genesis of the spectra in question.

Specific spectra have been satisfactorily proved to emanate from the compound molecules of cyanogen, water, and magnesium-hydrogen, so far as we can interpret in the simplest way the many observations previously detailed. The fact that a fluted spectrum is produced under certain conditions, by a substance which does not give such a spectrum under other conditions, is of itself a proof that the body has either passed into an isomeric state or has formed some new compound; but we are not entitled to assert, without investigation, which of these two reasonable explanations of the phenomena is the true one. There is, however, a spectrum to which we have had occasion to refer in the papers on the spectra of the compounds of carbon, which closely resembles that of a compound substance, and which we, in common with some other spectroscopists, have been led to attribute to the hydrocarbon acetylene, without, however, being able to bring forward such rigid experimental proofs of its origin as we have adduced in the case of the three substances above referred to. In other words, the experimental evidence that the hydrocarbon flame spectrum is really due to a hydrocarbon was always indirect. Thus, we showed that many flames containing carbon, such as those of hydrogen mixed with bisulphide of carbon or carbonic oxide, and the flame of cyanogen in air, did not give this spectrum, and these particular flames are known, from the investigations of Berthelot, to be incapable of generating acetylene under conditions producing incomplete combustion. On the other hand, we found that a flame of hydrogen mixed with chloroform, which easily generates acetylene, gives the hydrocarbon flame spectrum in a very marked manner, and it is known that the ordinary blow-pipe flame, in which the same spectrum is well developed, contains this hydrocarbon.

These and other experiments point to the intimate relation of hydrogen and carbon in the combined form of acetylene to the production of this spectrum during combustion. In our various observations on the spectrum of the electric arc taken in different gases, the flame spectrum was always noticed, and seemed to be independent of the surrounding atmosphere. In the mode in which those experiments were conducted, it was easily shown that the carbons were never free from hydrogen, and that the gases always contained traces of aqueous vapour. Under these conditions acetylene is formed synthetically during the electric discharge, the line spectrum of hydrogen being absent; so that we were never convinced that the spectrum was not due to the former substance.

It is well to remark in passing, that our previous work on the spectrum of the carbon compounds was mainly directed to that particular spectrum which is characteristic of the flame of cyanogen, and only indirectly to the flame spectrum of hydrocarbon. We were further supported in connecting the latter spectrum with acetylene, by observing that cyanogen compounds are continuously formed when the arc discharge takes place in gases containing nitrogen, and that in all probability their formation is due, as Berthelot has shown, to a reaction taking place between acetylene and nitrogen. Berthelot is positive in his assertions that cyanogen is never formed by a direct combination between carbon and nitrogen, and any such apparent combination is due to impure carbon, and the presence of an imperfectly dried gas; in other words, hydrogen is essential to the production of cyanogen under such conditions according to the views of Berthelot.

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[1] "On the spectra of the Compounds of Carbon with Hydrogen and Nitrogen." I and II. *Proc. Roy. Soc.*, vol. 30, pp 152, 494. "On the Spectrum of Carbon," *ib.*, vol. 33, p 403. "General Observations on the Spectrum of Carbon and its Compounds," *ib.*, vol. 34, p 123. "On the Spectrum of Water," *ib.*, vol. 30, p. 480, and vol. 33, p. 274. "Investigations on the Spectrum of Magnesium," *ib.*, vol 32, p 189

The fact that carbonic oxide, which is one of the most stable binary compounds of carbon, forms a distinct spectrum of a character similar to that of the flame spectrum, tended to support the view that the flame spectrum might originate with acetylene. The similarity in the character of the magnesium-hydrogen spectrum to that of the hydro-carbon flame spectrum induced us to believe that they were due to similarly constituted compounds, and seeing we felt sure about the accuracy of the view, which assigns the former spectrum to some compound of magnesium with hydrogen, we accepted the analogy in favour of the supposition that acetylene is the substance which produces the flame spectrum; or, at any rate, that acetylene is a necessary concomitant of the reaction taking place during its emission, and consequently might give rise to this peculiar spectrum.

Having examined this question in the way described, we adopted the view of Angström and Thalén as to the genesis of this spectrum in opposition to the views of Attfield, Morren, Watts, Lockyer, and others, who held that this spectrum was really due to the vapour of carbon. The delicate character of the experiments which were required to discover the origin of the peculiar set of flutings in the more refrangible part of the spectrum of cyanogen made it apparent that, whatever views as to the origin of the hydrocarbon flame spectrum were adopted by different workers, it could not be regarded hitherto as experimentally proved which was the correct one.

With the object of being able to exhaust this question, a special study was subsequently made of the ultra-violet line spectrum of carbon, in order to ascertain whether any of its lines could be found in the spectra of the arc or flame. We have found that the ultra-violet lines of metallic substances have as a rule the greatest emissive power, and are often present when no trace of characteristic lines in the visible part of the spectrum can be detected. If carbon resembled the metals in this respect, then we might hope to find ultra-violet lines belonging to its vapour, thus enabling us to detect the volatilisation of the substance at the relatively low temperatures of the arc and flame. The test experiments made on this hypothesis are recorded in the paper entitled "General Observations on the Spectrum of Carbon and its Compounds." It is there shown that some seven of the marked ultra-violet spark lines of carbon occur in the spectrum of the arc discharge, although one of the strongest lines, situated in the visible portion of the spectrum at wave-length 4266, could not be found. Further, it is proved that the strongest ultra-violet line of carbon does occur in the spectrum of the flame of cyanogen fed with oxygen. Thus it seems probable that the same kind of carbon molecule exists, at least in part, in the arc and flame, as is found to be produced by the most powerful electric sparks, taken between carbon poles or in carbon compounds.

Now the spark gives us the spectrum which is associated with the highest temperatures, and therefore it is assumed that this spectrum is that of the simplest kind of carbon vapour. If that be the case, we cannot avoid inferring that denser forms of carbon vapour must exist in arc and flame, emitting, like other complex bodies, a fluted, in contrast to a line, spectrum; or rather that the two distinct kinds of spectra may be superposed. Such considerations showed that a series of new experiments and observations must be made with the special object of reaching a definite conclusion regarding the origin of the flame spectrum, and the following paper contains a summary of the results of such an inquiry.

Vacuous Tubes.—We have heretofore laid little stress on observations of the spark in vacuous tubes on account of the great uncertainty as to the residual gases which may be left in them. The film of air and moisture adherent to the glass, the gases occluded in the electrodes, and minute quantities of hydrocarbons of high boiling-point introduced in sealing the glass, may easily form a sensible percentage of the residue in the exhausted tube, however pure the gas with which it was originally filled. The excessive difficulty of removing the last traces of moisture we learnt when making observations on the water spectrum, and the almost invariable presence of hydrogen in vacuous tubes is doubtless due in great measure to this cause. Wesendonck (*Proc. Roy. Soc.*, vol. 32, p. 380) has fully confirmed our observations as to this difficulty. By a method similar to that employed by him, we have, however, succeeded in so far drying tubes and the gases introduced into them that the hydrogen lines are not visible in the electric discharge. For this purpose the (Plucker) tube was sealed on one side to a tube filled for some six or eight inches of its length with phosphoric anhydride, through which the gas to be observed was passed, and on the other side to a similar tube full of phosphoric anhydride, which was in turn connected by fusion to the (Sprengel) pump. To dry the gas it is not enough to pass it through such a tube or even a much longer one full of phosphoric anhydride; it has to be left in contact with the anhydride for several hours, and to get the adhering film of moisture out of the tube it has to be heated after exhaustion while connected, as above described, with the drying tubes up to the point at which the glass begins to soften, and kept at near this temperature for some time. To get most of the gases out of the electrodes, the tube must be exhausted and sparks passed through it for some time before it is finally filled with the gas to be observed. Even when these precautions have been observed, the lines of hydrogen can often be detected in tubes filled with gases which should contain no hydrogen. The general result of our observations on the spectra observed in tubes so prepared is that the channelled spectrum of the flame of hydrocarbons is not necessarily connected with the presence of hydrogen; it does not come and go according as hydrogen is or is not present along with carbon in the way that the channelled spectrum of cyanogen comes and goes according as nitrogen is present or absent. Our observations confirm those of Wesendonck on this point. This spectra given by various tubes containing carbon compounds are described in the paper.

Tubes filled with carbonic oxide exhibit in general at different stages of exhaustion the following phenomena. When the exhaustion is commencing and the spark will just pass, the spectrum is usually that of the flame of hydrocarbons and nothing else. As the exhaustion proceeds, the spectrum of carbonic oxide makes its appearance superposed on the former, and gradually increases in brilliance until it overpowers, and at last at a somewhat high degree of exhaustion, entirely supersedes the flame spectrum. This is when no jar is used. In the earlier stages of exhaustion, the effect of the jar is to increase the relative brilliance of the flame spectrum, and diminish that of the carbonic oxide spectrum, and at the same time to bring out strongly the lines of oxygen and carbon; at a certain stage of the exhaustion, when the flame spectrum is very weak without the jar, the effect of the jar is to bring it out again, but without sensibly enfeebling the carbonic oxide spectrum, and without bringing out the carbon lines. At a still higher stage of exhaustion, when the carbonic oxide spectrum is alone seen without the jar, the flame spectrum is sometimes, not always, brought out by putting on the jar, though the carbon lines again show well. At this stage, at which the flame spectrum is not seen at all, the distance between the striæ in the wide part of the tube is considerable, and much metal is thrown off the electrodes, which are rapidly heated by the discharge. In a tube filled with carbonic oxide mixed with a little air imperfectly dried, when not too highly exhausted, the carbonic oxide spectrum, that of the flame of hydrocarbons, and that of cyanogen, may all be seen at once superposed when no jar is used. With a jar and a tolerably high exhaustion the carbonic oxide spectrum, the hydrocarbon flame spectrum, and the carbon line spectrum, may all be seen at the same time.

Spectrum of the Spark in Compounds of Carbon at Higher Pressures.—In the spark taken between poles of purified graphite in hydrogen, the spectrum of hydrocarbon flames is seen, and it increases in brilliance, as the pressure of the gas is increased up to ten atmospheres, and continues bright at still higher pressures so far as we have observed, that is, up to twenty atmospheres. The spark without condenser in carbonic oxide at atmospheric pressure, shows both the spectrum of carbonic oxide and that of the hydrocarbon flame; and as the pressure of the gas is increased, the former spectrum grows fainter, while the latter grows brighter, no jar being used. The line spectrum of carbon is also visible. At the higher pressures the flame spectrum predominates and is very strong. The observations were carried up to a pressure of twenty-two and a half atmospheres. On letting down the pressure, the same phenomena occur in the reverse order. All the parts of the flame spectrum, as seen in a Bunsen burner, are increased in intensity as the pressure is increased. The fact that the effects of high pressure are so similar to those produced by the use of a condenser at lower pressures, seems to point to high temperature as the cause of those effects. But against this, we have the fact that at reduced pressure we get in carbonic oxide, the carbonic oxide spectrum and the line spectra of carbon and oxygen simultaneously, without that of the hydrocarbon flame. As we cannot doubt that a very high temperature is required to give the line spectrum of carbon, we must suppose that reduced pressure is unfavourable to the stability of the molecular combination, whatever it be, which gives the hydrocarbon flame spectrum. Wesendonck has remarked (*loc. cit.*.) that in carbonic acid at pressures too low for the flame spectrum to be developed without a jar, it is only in the narrow part of the tube that the use of a jar brings out that spectrum. It would appear, therefore, that the constraint, due to the confined space in which the discharge occurs, has the same effect, in regard to the stability of the combination producing the spectrum in question, as increase of pressure.

Cyanogen Flame Spectrum.—Our former observations "On the Flame Spectrum of Cyanogen Burning in Air" were made on cyanogen gas, prepared from well-dried mercury cyanide, which was passed over phosphoric anhydride, and burnt from a platinum jet fused into the end of the tube. We observed what Plucker and Hittorf had noted, that the hydrocarbon bands were almost entirely absent, only the brightest green band was seen, and that faintly. When gaseous cyanogen is liquefied by the direct pressure of the gas, the researches of Gore (*Proc. Roy. Soc.*, vol. 20, p. 68) have shown that it is apt to be contaminated with a brownish, treacley liquid, which probably arises from the imperfectly purified or dried cyanide of mercury. In order to obtain pure cyanogen, we have prepared quantities of liquid cyanogen, not by compression, but by passing the already cooled gas into tubes placed in a carbonic acid and ether bath. By this method of condensation any easily liquefiable substances are isolated, and any permanently gaseous substance escaped. The samples were sealed up in glass tubes into which different reagents were inserted. After such treatment the cyanogen was used for the production of the flame in dry air or oxygen. The liquid cyanogen was left in contact with phosphoric anyhdride, Nordhausen sulphuric acid, and ordinary sulphuric acid. By means of a special arrangement of glass tubing surrounding the flame dry oxygen could be supplied, or oxygen made directly from fused chlorate of potash could, by means of a separate nozzle, be directed on to the flame, and thus perfectly dry and pure gases used for combustion. Liquid cyanogen which had remained in presence of the above reagents gave only the single green hydrocarbon flame line faintly in dry air, all the cyanogen violet sets being strong. When oxygen made directly from chlorate of potash was directed on to the flame, all the hydrocarbon flame sets appeared with marked brilliancy. The set of lines which we have formerly referred to as the three set of flutings of the cyanogen spectrum, showed marked alteration of brilliancy with variations in the oxygen supply. Thus, liquid cyanogen purified by the action of the above reagents, does yield the spectrum of hydrocarbon flames on combustion in pure oxygen. From the great precautions we have taken, we feel sure that the amount of combined hydrogen in the form of water or other impurities in the combining substances must have been exceedingly small, and that the marked increase in the intensity of the flame spectrum, when oxygen replaces air is essentially connected with the higher temperature of the flame, and is not directly related to the amount of hydrogen present. This being the case, it must be admitted that the flame spectrum requires a higher temperature for its production during the combustion of cyanogen than that which is sufficient to cause a powerful emission of the special spectra of the molecules of cyanogen. Now, the two compounds of carbon, which give the highest temperature on combustion are cyanogen and acetylene. Both of these compounds decompose with evolution of heat, in fact, they are explosive compounds, and the latent energy in the respective bodies is so great that if thrown into the separated constituents a temperature of near four thousand degrees would be reached. The flames of cyanogen and acetylene are peculiar in this respect, that the temperature of individual decomposing molecules is not dependent entirely on the temperature generated by the combustion, which is a function of the tension of dissociation of the oxidised products, carbonic acid and water. We have no means of defining with any accuracy the temperature which the particles of such a body may reach. We know, however, that the mean temperature of the flames of carbonic oxide and hydrogen lies between two and three thousand degrees, and if this be added to that which can be reached by the substance independently, then we may safely infer that the temperature of individual molecules of carbon, nitrogen, and hydrogen in the respective flames of cyanogen and acetylene may reach a temperture of from six to seven thousand degrees.

A previous estimate of the temperature of the positive pole in the electric arc made by one of us, was something like the same value.

The formation of acetylene in ordinary combustion seems to be the agent, through which a very high local temperature is produced, and this is confirmed by the observations of Gouy on the occurence of lines of the metals in the green cone of the Bunsen burner, which are generally only visible in spark spectra. On this view acetylene is a necessary agent in the production of the flame spectrum during combustion. The fact that the flame spectrum is often invisible when the arc is taken in a magnesia crucible, although the cyanogen spectrum is strong, but may be made to appear by introducing a cool gas or moisture, must be accounted for by an increased resistance in the arc producing temporarily a higher mean temperature. The experiments in course of execution, where the arc will be subject to a sudden increase of pressure, will, we trust, solve this difficulty.

Further evidence of the high temperature of the cyanogen flame is afforded by the occurrence in the spectrum of that flame, when fed with oxygen, of a series of flutings in the ultra-violet, which appear to be due to nitrogen. The series consists of four, or perhaps more, sets, each set consisting of a double series of lines overlapping one another. The lines increase in their distance apart on the more refrangible side, otherwise the flutings have a general resemblance to the B group of the solar spectrum.

The four sets commence approximately at about the wave-lengths 2718, 2588, 2479, 2373 respectively. They are frequently present in the spectrum of the arc taken in a magnesia crucible, and show strongly in that of the spark taken without a condenser either in air or nitrogen. As they appear in the spectrum of the spark in nitrogen, whether the electrodes be aluminium or magnesium, and do not appear when the spark is taken in hydrogen or in carbonic acid gas, they are in all probability due to nitrogen. When a large condenser is used they disappear.

Linnean Society, December 21, 1882.—Alfred W. Bennett, M.A., in the chair.—Prof. Adolph. Ernst, of Venezuela, and Dr. W. C. Ondaatje, of Ceylon, were elected Fellows.—Prof. T. S. Cobbold exhibited specimens of Ligules from the Bream, the Minnow, and the Grebe to compare with those from man. The worm from the Bream is called *L. edulis* by Briganti, and is eaten under the name "macaroni piatti."—Mr. T. Christy called attention to experiments lately made, which show that the Kola nut possesses singular properties of clearing fermented liquors.—Mr. Thos. H. Corry read a paper on the development and mode of fertilisation of the flower in *Asclepias Cornuti*. R. Brown, 1809, J. B. Payer, 1857, and thereafter H. Schacht, have made Asclepias the subject of interesting study; Mr. Corry nevertheless has added new observations thereto. He finds that the petals and stamens, which in the early stage originate separately, become afterwards adnate; the stamens, moreover, by their broad filaments form a fleshy pentagonal ring, *i.e.* are monadelphous. The "*stigma-disk*" is not formed by the fusion of two stigmas, for the styles proper remain distinct throughout their entire extent. The greatest analogy of the flower to that of the Apocynaceæ is at this period; thereafter differences ensue. From a careful study of the different stages of the pollen in Asclepias it appears to exhibit a perfectly isolated and peculiar case of formation. The idea that self-fertilisation can take place with the parts *in situ* is shown to be impossible, and the need for insect or artificial aid rendered imperative. Cross-fertilisation is the great law in the Asclepiads.—Dr. F. Day read a paper, "Observations on the Marine Fauna of the East Coast of Scotland," founded on a recent survey by H.M.S. *Triton* off Aberdeen, Kincardine, and Forfar. As regards the herring and its migrations, they shift their locality for breeding purposes or in search of food, occasionally being driven from a spot where extensive netting or other causes disturb them. The herring seems of late years to take to deeper water off shore, but at times they appear to return to their old habitats in the comparatively shallow water. Although it is true that some fisheries—Wick, for instance—have decreased in plenty, at the same time other places, *e.g.* Fraserburgh, have proportionately increased. The fishery records prove that from the beginning of this century onwards there has been a steady annual increase of fish taken, though desponding fishermen aver to the contrary. At Wick, herring of different ages and conditions arrive and depart thrice yearly. Dr. Day recounts the results of his dredgings, and describes the Crustaceans and Mollusks obtained.—An additional report on the Echinoderms collected by Dr. Day, was made by Prof. F. J. Bell, and of the Zoophytes and Sponges by Mr. S. O. Ridley.—Mr. J. G. Baker afterwards read his second contribution on the Flora of Madagascar. In this paper, upwards of 150 new species of monopetalous dicotyledons are characterised. They were gathered chiefly by the Rev. R. Baron, F.L.S., of the London Missionary Society. Among others described are four new genera, one nearly allied to Cinchona, a second of semi-parasitic Scrophulariaceæ, and two of Acanthaceæ; besides these, many representatives of well-known European genera occur.—Prof. T. S. Cobbold read a description of *Ligula Mansoni*, a new human Cestode. He shows it to be extremely probable that the trout's ligule is the sexually immature state of the great broad tapeworm of man. Other interesting genetic relations are established, and several important generalisations discussed.—Additions to the Lichens of the *Challenger* Expedition was a short paper by the Rev. J. M. Crombie.—Mr. J. G. Baker made a second communication, being descriptions of about thirty plants from the Fiji Islands referred to by Mr. J. Horne in his recent work on the economic resources of Fiji.

Victoria Institute, January 1.—A paper upon "Design in Nature," was read by Mr. W. P. James. It was stated that Prof. Stokes, F.R.S., would read a paper at the next meeting.

PARIS

Academy of Sciences, January 2.—M. Blanchard in the chair.—M. Rolland was elected vice-president for 1883.—The Academy has lost four Members during 1882, viz MM. Liouville, Decaisne, Bussy (Free Academician), and Wohler (Foreign Associate); and six Correspondents, viz. MM. Plantamour, Lutke, Billet, Darwin, Cornalia, and Schwann.—Memoir on the vision of material colours, &c. (continued), by M. Chevreul.—Researches on hyponitrites; first part, chemical researches, by MM. Berthelot and Ogier. They study hyponitrite of silver, describing their analyses, and examination of the action of heat and oxidising agents, also calorimetric measurements. The formula $N_2O_5Ag_2$ agrees best with the results.—Ramifications of *Isatis tinctoria*, formation of its inflorescences, by M. Trécul.—It was announced that the U.S. Congress had invited the President of that country to convoke all nations to a conference with a view to adoption of a common initial meridian and an universal hour.—Reply to the objections presented by MM. Faye and Hirn to the theory of solar energy, by Dr. Siemens.—On a method of photographing the corona without an eclipse of the sun, by Dr. Huggins.—On geodetic circles, by M. Darboux.—On algebraic integrals of linear differential equations with rational coefficients, by M. Autonne.—On a communication of M. de Jonquières relative to prime numbers (continued), by M. Lipschitz.—Remarks on the subject of a note of M. Hugoniot, on the development of functions in series from other functions, by M. du Bois-Reymond.—Does oil act on the swell or on the breaker? by M. Van der Mensbrugghe. His theory applied only to two cases: where calm water, covered with oil, came to be acted on by wind, and where waves break. The relative calm of phosphorescent portions of tropical waters, he attributes not to increase of cohesion of the water (Admiral Bourgeois), but to the innumerable floating objects forming an obstacle to the slip of surface-layers over each other.—Decomposition of formic acid by the effluve, by M. Maquenne. The results are the same as those got by M. Berthelot in decomposing gaseous formic acid in a closed vessel, by heat alone, about 260°.—On the chloride of pyrosulphuryl, by M. Ogier.—On a vibrion observed during measles, by M. Le Bel. It is found in the urine in the early stages, and disappears with the fever: is a slightly curved, very refringent rod, moving very slowly; contains oval spores at one-third of its length, in a bag of dead protoplasm, which gradually disappears, the spore showing then a zone of mucilage around it. Another occurrence of spores on the thirty-fifth day was observed in an adult. The vibrion also may be got from the skin at the time of desquamation. M. Le Bel cultivated the vibrion, and injected it into a guinea-pig; which, on the tenth day, showed small vibrions in its urine, but did not seem incommoded. The urine in scarlatina and in diphtheria shows a microbacterium and a micrococcus, respectively, both quite different from the vibrion of measles.—Existence of zinc in the state of complete diffusion in dolomitic strata, by M. Dieulafait.—On the Marine Carboniferous of Haute-Alsace; discovery of culm in the valley of the Bruche, by MM. Bleicher and Mieg.—On the excitant property of oats, by M. Sanson. He has experimented with Du Bois Reymond's electrical apparatus on the neuromuscular excitability of horses, before and after ingestion of oats, or of an excitant substance, which he isolated from oats (from the pericarp of the fruit); this is called *avenine*, is quite unlike vanilline, is uncrystallisable, brown in mass, finely granular, and has the formula $C_{56}H_{21}NO_{18}$. All kinds of cultivated oats elaborate it, but in different quantity; as a rule the white varieties have less than the dark. The quantity seems also to depend on the place of cultivation. Crushing the grain weakens the excitant property. The total duration of the excitation (which grows to a point, then gradually disappears) seemed to be about an hour per kilogramme of oats ingested.

Errata in last week's report.—P. 236, top of second column, 7th line, for "Guimareo" read "Guimaraes" 9th line, for "argotised" read "azotised"; 13th line, for "usteria" read "Asteria"; 16th line, for "*pedunculus*" read "*pedunculatus*"; 16th line, for "suctocitiates" read "suctociliates."

VIENNA

Imperial Academy of Sciences, November 9, 1882.—The following papers were read:—K. Laker, studies on the hæmatic discs (Hayem's hæmatoblasts), and on the so-called dissolution of the white blood-corpuscles in the process of the purification of the blood.—E. Ludwig, note relating to the chemical composition of the damburite from the Scopi Mountain (Graubündteng).—T. Herzig, on guaiaconic acid and guiacic acid.—On the action of nitrous acid on guiacol, by the same.—A. Grunow, preliminary communication on the Diatomaceæ collected by the Austro-Hungarian North Polar Expedition.

November 16, 1882.—The following papers were read:—N. Polejæff, on the sperm and spermatogenesis of *Sycandra rajahamus Hæckelii*.—F. v. Hauer, new contributions to the knowledge of the elder tertiary Brachiuara fauna of Vicenza and Verona (Italy).—M. Margules, note on the dynamo-electric process.—A. Tarolimek, contributions to mechanical theory of heat.—K. Zelbr, on the comet Schmidt, October 9, 1882.—A sealed paper dated from November 6, 1882, was opened and read containing a short note by Josef Popper, on the transmission of power and the realisation of unused natural powers by electricity.

CONTENTS

PAGE
Geikie's Geology. By G. K. Gilbert, U.S. Geological Survey . . 237
Our Book Shelf:—
  Minchin's "Uniplanar Kinematics of Solids and Fluids" . . . 239
  "Die Kafer Westfalens" 239
Letters to the Editor:—
  Equal Temperament of the Scale – C. B. Clarke 240
  Animal Intelligence —Dr. Fritz Mueller 240
  The Inventor of the Incandescent Electric Light.—W. Mattieu Williams 240
  The Reversion of Sunflowers at Night.—C. A. White 241
  Pollution of the Atmosphere.—Joseph John Murphy 241
  A "Natural" Experiment in Complementary Colours.—E. J. Bles 241
Bairds' Hare and its Habits 241
Notes from the Letters of Captain Dawson, R.A., in Command of the British Circumpolar Expedition 242
The Swedish Expedition to Spitzbergen, 1882 243
The Increase in the Velocity of the Wind with the Altitude. By E. Douglas Archibald 243
Krao, the "Human Monkey." By A. H. Keane 245
Figure of the Nucleus of the Bright Comet of 1882 (Gould). By Prof. Edward S. Holden (*With Illustrations*) 246
Notes 246
Our Astronomical Column:—
  The Total Solar Eclipse on May 6 248
  The Minor Planets 248
  Comet 1882 *c* 248
The Education of our Industrial Classes. By J. Norman Lockyer, F R.S. 248
The Transit of Venus 253
Electric Railways. By Prof. W. E. Ayrton, F.R.S. (*With Illustrations*) 254
Societies and Academies 257

THURSDAY, JANUARY 18, 1883

GEIKIE'S GEOLOGY[1]

Geological Sketches at Home and Abroad. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London and New York: Macmillan and Co., 1882.)

Text-Book of Geology. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London: Macmillan and Co., 1882.)

II.

WE now come to consider the quality of the matter contained in the volume, the discrimination exercised in its selection, the validity of the theories presented, and the fidelity with which the science is portrayed. It is the function of a text-book to exhibit to the student an impartial and symmetric outline of the science. Its author is under obligation to present the views which are generally entertained by the great body of geologists, carefully withholding those which are peculiar to himself. From the great mass of available matter he must select that which will afford a well-balanced and comprehensive review, and he must sedulously avoid giving undue prominence to those matters which have special interest to himself by reason of his individual studies. In the work before us this has been accomplished in a manner which may truly excite admiration. Although the author is an original investigator in several departments of the science he delineates, he has permitted his own predilections to give little if any additional prominence to his special topics, and the wisdom he has displayed in the selection of material and the balancing of parts will commend itself to all professional readers.

It is useless to attempt an analysis of a work which is itself an epitome of a great science, but we may refer to the treatment of a few mooted points and to a few matters of novelty or of current interest.

The microscopical characters of rocks are treated more at length than in any other text-book. In the general account of rock characters they are accorded even more space than are the macroscopical, and they form part of the description of each specific rock. They are, moreover, illustrated by a series of cuts, showing the appearance of thin slices when highly magnified. A chapter is devoted to the subject of rock determination, and an analytical table is included therein.

The results of the *Challenger* exploration of the bottom of the ocean are given at some length, and the conclusion is drawn that the continental regions of the globe have been marked out from the earliest geological times. This is not treated as an hypothesis but as an established theory, and its logical consequences appear in numerous places.

In the taxonomic terms of stratigraphy, the convention of the Bologna Congress is not adopted. The terms *system*, *series*, and *stage* are used in the same order, but *group*, which by the congress was made more comprehensive than *system*, is by Geikie used as the equivalent of *stage*. He remarks, with propriety, that the attempt to alter the signification of a term so universally employed in English literature would produce far more confusion than can possibly arise from a failure to conform to continental usage.

One of the most conspicuous omissions of the book is with reference to the antiquity of man. The subject is treated with great brevity, because it is regarded as belonging more properly to archæology, but an account is nevertheless given of the earliest human vestiges. Mention is made of man's association with the Loess and with the inter-Glacial deposits of Europe, but the Californian claims to his pre-Glacial existence are ignored. It is true that these claims have been disputed, and it is true that the evidence in regard to each of the individual finds upon which they rest is incomplete; but since Whitney has assembled all the facts in his "Auriferous Gravels," it must be admitted that their cumulative force entitles them at least to recognition and consideration, however slow we may be to accept them as demonstrative.

In the section which treats of man as a geological agent, there are enumerated a great variety of ways in which he modifies the face of nature, but one of the principal, if not indeed the chief of all, is omitted, namely, the stimulus he gives to denudation by tilling the soil. The mat of vegetation, living and decaying, which naturally covers the soil in all humid regions, affords great protection against the erosive work of rain. Not only is the beating of the rain resisted, but the velocity of its outflow is retarded, so that from surfaces of gentle inclination it washes away very few particles. When this mat has been removed, and especially when the surface has been stirred by the plough, the conditions become exceedingly favourable to rain erosion, and the rain rills are charged with sediment. Moreover, cultivation and the cutting of forests increase the magnitude of river floods, and since rivers perform their chief work of erosion and transportation during flood-stage, the quantity of their work is thus augmented. It is safe to say that the rate of degradation of the surface by rains and rivers is increased several hundred per cent. by the removal of forests and the tillage of the soil, and it may be added that for this reason most attempts to measure the natural rate of denudation by means of the outscour of rivers have been abortive.

The unconformability between the Archæan and the Palæozoic is not mentioned in such way as to convey an impression of the profoundness of the chronological break. There is no known locality where a newer formation rests conformably upon the Archæan. There are few where the discordance of dip is not great. There are few where the superior formation is not relatively unaltered, and none where the inferior formation is not highly metamorphosed. So far as we know, the Archæan strata were both thrown in great folds and plicated in detail, were universally subjected to a metamorphism such as in later rocks seems to have been accomplished only at a depth beneath the surface, and were subsequently worn away upon a most stupendous scale before they received any sedimentary covering within the regions now accessible for examination. Compared with this, all other chronological breaks are trivial, and we may almost say that, compared with this, all other stratigraphical breaks are local.

In treating of the condition of the interior of the earth, Geikie concisely presents the prominent hypotheses, and then remarks that it is "highly probable that the substance of the earth's interior is at the melting-point proper for the pressure at each depth." In treating of the age of the earth, he sets forth the geological and the physical arguments with commendable brevity, but withholds all expression of individual opinion. In treating of the origin of orographical displacement he gives a brief history of opinion, and states that the contractional hypothesis is now generally accepted. A foot-note, however, refers to Fisher's "Physics of the Earth's Crust," which appeared while the text-book was passing through the press. The cause of ice motion is not discussed.

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[1] Continued from p. 239.

In the classification of formations there is nothing new. The Cambrian and Silurian are marked as independent and co-ordinate divisions, the latter beginning with the Arenig group in Great Britain and with the Calciferous in America, but the opinion is expressed that a subsequent revision of the subject may result in "throwing all these older Palæozoic rocks into one palæontological system." The pre-Cambrian rocks are designated by Dana's title of Archæan. The Rhætic is included with the Trias. In the table of formations the American Laramie is placed in the Tertiary; but this appears to have been done by inadvertence, for in the descriptive text which follows it is treated as Cretaceous.

In the classification of rocks the primary division is into crystalline and clastic. The crystalline are separated into stratified, foliated, and massive, and the clastic into sand rocks, clay rocks, volcanic fragmental, and organic fragmental. Of the massive crystalline rocks, the principal sub-group is indicated as *feldspar bearing*, and four small groups (the nepheline rocks, the leucite rocks, the olivine rocks, and the serpentine rocks) are indicated as co-ordinate with this.

The subject of geological climate is treated almost exclusively from the astronomical point of view, and the theory of Croll is the only one which receives more than passing mention. Its statement was prepared especially for the volume by Dr. Croll himself, and covers six pages. It is undoubtedly true that this theory has been widely accepted, that it is very generally entertained as a working hypothesis, and that it is the most probable one before the public; and it should for these reasons be given great prominence in a text-book; but I cannot help regretting that it has been presented with so little qualification. It deals with a series of physical laws and physical conditions which interact upon each other in an exceedingly complex way—in so complex a way that meteorologists, who have to deal with only a portion of them, do not claim and scarcely hope for a complete analysis of their combinations. The opportunities for arguing in a circle are most seductive, and the *a priori* probability that important considerations have been overlooked is not small. The only manner in which so comprehensive and intricate an hypothesis can be established is by stimulating inquiry which shall lead to corroborative evidence, and this is precisely what Croll's hypothesis after eight years of wide publicity has failed to do. If it is true, then epochs of cold must have occurred with considerable frequency through the entire period represented by the stratified rocks; and iceberg drift, if no other traces, should have been entombed at numerous horizons. It has not been found, however, and of the eight horizons claimed by Croll to show evidence of glacial action, the treatise under consideration mentions only two with confidence, and two others with doubt. In the two instances to which queries are not attached, the phenomena appear to indicate local and not general glaciation. If the hypothesis is true, the cold of the Glacial epoch must have been many times interrupted by intervals of exceptional warm, but little has been added to the evidence adduced by Croll for such an interruption, and in America, where there is now great activity in the investigation of glacial phenomena, the evidence of a *single* inter-glacial period is cumulative and overwhelming, while there is no indication whatever of more than one. If the hypothesis is true, submergence in polar and temperate regions should have been coincident with glacial expansion, and emergence coincident with glacial retreat, but the Quaternary history of Great Britain, as drawn in the new text-book, includes two periods of maximum ice-extension, *separated* by a period of maximum submergence. While these difficulties exist it appears to me unadvisable to convey to the student the impression that a satisfactory solution to the problem of glacial climate has been reached.

Because I have mentioned some points in which my individual judgment differs from that of Prof. Geikie, it must not be supposed for a moment that I undervalue his work, or that I regard it with anything short of enthusiastic commendation. It is broad and catholic, conscientious in detail, masterly in treatment. It is imbued especially with a spirit which for want of a better name may be called scientific modesty. The majority of our text-books, including all of our best text-books, have been written by teachers, and have been more or less affected by the peculiar mental attitude of the teacher. The investigator is under the constant necessity of holding his judgment in abeyance, and of treating every conclusion as an hypothesis, to be tested by future researches, and possibly amended or even abandoned. The teacher is under an equal necessity to formulate his knowledge so that he may communicate it in definite shape—he must not doubt, he must know; and under this compulsion he naturally and unconsciously acquires an undue confidence in results that have simply arisen from the weighing of probabilities. He is especially tempted to regard classifications as final, and not to recognise them as temporary presentations of temporary stages of knowledge. It is the especial merit of Prof. Geikie's book that it is untainted by this teacher's bias. It cautions the student against the confusion of geological synchrony with stratigraphical homotaxis; it cautions against the free use of palæontological evidence in the inference of geological climate; it cautions against deductions which may be vitiated by the imperfection of the geological record, and against negative evidence in general; it cautions against the impression that there are in nature any hard and fast lines separating epochs or formations or rock species; and, in addition, it heeds its own cautions. Its readers cannot escape the impression that the science of geology is in its youth, that it is developing at a rapid rate, that many of its results are tentative, and that its unsolved problems are as numerous and important as those it has successfully attacked.

It is only by a conscious effort that one gives attention to the literary style of Prof. Geikie's text-book. It is so direct and plain that it serves the purpose of conveying thought without leaving an impression of the manner of conveyance. As in the matter, so in the manner, his personality is not permitted to intrude. He says one thing at a time, and therefore his sentences are short; but he does not exaggerate, and therefore he never indulges in epigram.

A noteworthy feature of the illustrations is the reproduction of a large number of De la Beche's cuts, which are derived directly from the original blocks. All of these are good, and so are the majority of the remaining illustrations, but there is also a considerable number, especially in the chapters on stratigraphy, which are not so distinct as is desirable, and which probably owe their imperfection to the employment of some photo-mechanical process. The typography is excellent, and a page of errata is not called for.

The foot-notes contain a very large number of useful references. These are not mere citations of authorities in support of statements in the text, but are indications to the student of treatises in which he may find the fullest exposition of subjects to which the text introduces him.

G. K. GILBERT,
U.S. Geological Survey

SACHS'S TEXT-BOOK OF BOTANY

Text-Book of Botany, Morphological and Physiological. By Julius Sachs, Professor of Botany in the University of Wurzburg. Edited, with an Appendix, by Sidney H. Vines, M.A., D.Sc., F.L.S., Fellow and Lecturer of Christ's College, Cambridge. Second Edition. (Oxford, 1882.)

THERE are not wanting signs that the study of botany is steadily increasing in this country. An immense number of text-books or manuals have been published in English during the last thirty years on the subject, some of which have been very popular, to judge by the many editions they have passed through. Referring to these introductions to the study of botany in general terms, it was to be noted that they all, in a more or less complete manner treated of the vegetable kingdom from a morphological and classificatory point of view; but that the morphological portions were deficient in clear descriptions or conceptions of the origin or development of the members of the plant's body which they described, and the student who required instruction as to physiological, anatomical, or embryological details, had to look for such in the pages of the botanical periodical literature of the day. Most modern workers in biology will agree that the greater portion of this literature was derived from German sources, and it is scarcely to be denied that the first general compendium of note appeared in the German text-book of Sachs. This work had reached a fourth edition in 1874, but the previous editions had found their way into several of the centres of botanical teaching in Great Britain and Ireland, and had caused a considerable change in the older methods of teaching botany. Still it must have been a matter requiring some courage for the delegates of the Clarendon Press to undertake the costly work of translating, editing, and printing in English this work of Sachs', forming a large octavo volume of nearly 1000 pages, a text-book one would think far too large and expensive for most ordinary students. This work was, however, issued from the Clarendon Press in the spring of 1875, and it is not without interest to note that for the last two or three years it has been completely out of print, so that the edition must have been exhausted in the course of the first four or five years after its issue. It was most unfortunate that this edition, so ably translated by Messrs. Bennett and Thiselton Dyer, had not been based on the fourth German edition, which had been published nearly a year before the English translation made its appearance. The success of the translation may, however, be looked on as to a certain extent condoning this misfortune, and there can be doubt as to the revolution in the study of botany in these kingdoms, which has been brought about by its appearance. Instead of to an endless catalogue of under- and above-ground forms of stems, instead of a list as long as that of the ships in Homer of the forms of simple and compound leaves, the student has had his attention—at least in some schools—called to the important structures to be met with in these varied portions of a plant and to their peculiar functions and ontogeny. The subject of plant life and development seems to have become of more especial interest and to have fallen like a new story on many even old ears. It was not, under these circumstances, surprising that a new edition was called for, but it did excite some surprise that, having in a great measure made the demand, the Delegates of the Clarendon Press seemed unable for a time to supply it, and let several Long Vacations glide by without its appearance; even this new edition comes to us late in the autumn season of the year, when the year's fruits have been well garnered in. Still it is welcome as an important contribution to the study of a science that has of old and for long been fostered by the University of Oxford.

Welcome as this new edition is, it would, we firmly believe have been a much more complete text-book and have reflected more of credit on the Clarendon Press Series, if the present Editor had been given a fairer field to work on. Although the fourth German edition was in advance of the previous one, yet the half-dozen years that have elapsed since it made its appearance have been years during which botany has advanced with no tardy footsteps. Even Prof. Sachs himself could not be persuaded to face the torture of a fifth edition of the original, for he felt, as he tells us, that the expanded views of the present period would not even fit into the framework of his text-book, so that a faithful translation of the fourth edition is even more out of date in 1882 than the translation of the third edition was in 1875.

Hence it must have been distressing for the Editor of the volume before us to find, on entering on his task, that nearly the whole of Book I., which treats of the general morphology of the cell, the tissues, and the external conformation of plants had been for some time in type, and that consequently a number of recent discoveries had not been noticed in it. No one could have been better fitted than Mr. Vines to have brought this most important section up to date, and it is a pity that only 32 out of its 232 pages were reprinted, for there is a decided awkwardness in looking in an appendix for supplementary remarks which are often found to explain away or totally alter the meaning of the text.

As it is, we would emphasise the prefatory remark of the Editor, that "the Appendix has come to be an important feature" of the book, and is to be especially recommended to the notice of the reader. The student who will judiciously introduce the new material in this appendix into the older structure in the text will be afforded a tolerably clear insight into the present standpoint of vegetable morphology.

Book II. forms the largest third of the volume, and from a purely critical point of view, was the least satisfactory portion of the original. No doubt it was by far the most difficult portion to condense into any reasonable compass, and it bristled with unknown quantities and controverted points, and indeed it may well be doubted if the immense subject of "General Morphology and Outlines of Classification" could be fairly well mastered by any one botanist. That the editor has added a great deal of new material—no doubt assisted by some whose criticisms and suggestions he gratefully acknowledges—is, on the face of this second book, abundantly clear. That a good deal might have been still added is also, on a little examination, made apparent. Detailed criticism on this portion of the volume would be here to a large extent out of place, and serve no good end, but as a justification of these remarks we would observe that among the very first forms alluded to—the Protophyta arranged under the Cyanophyceæ—of which the Nostocaceæ form a highly interesting group—the description of the formation of the Nostoc filament, though amended, as noted within brackets, is, despite the warning of Bornet, founded on a misconception of Thuret's account. In a footnote, too, we read that Archer has described the occurrence of spores in *Nostoc paludosum*, as if this were something novel, but their appearance in many species has not only been known to but made even a factor in their classification by Bornet. It seems inexplicable to us why this distinguished author's works should be so little known to English writers, but so it is, and on turning over page 247 to see what would be said about the Rivulariaceæ—the Scytonemaceæ—we felt disappointed at not finding even a reference to show the student how much has been done by Bornet in recent years to add to our knowledge of these groups. To these remarks we will only add that the large and important groups of Palmellaceæ are dismissed in a paragraph of ten lines. In order that these remarks may not be mistaken, we may observe that we did not expect to find more than a sketch of the natural history of the forms to be found in these groups lying at the base of chlorophyllaceous life, but we did expect that what was narrated of these would be exact, and that a reference to the latest literature of the subject would be given. It would be easy, at least among the Thallophytes, to extend these criticisms. Such an excessively interesting algal form as Pithophora is nowhere alluded to, though its first birth-place seems to have been our Royal Gardens at Kew. Wittrock's paper on this form was fully as important as his on Mesocarpeæ. Nor is the student referred under Fucoideæ to the splendidly illustrated work on the group by Thuret and Bornet; but criticism is not our object, and we gladly pass from the notice of Book II. to notice Book III., from which, knowing the excellent work done by the Editor in vegetable physiology, we expected great things, nor have we been disappointed. It seems to us an excellent account of vegetable physiology, with all or most of all the modern discoveries alluded to, and we know of no compendium on the subject at all approaching to it.

Should in another four years this second English edition be sold out, let us hope that Mr. Vines will, like its author, cease trying to mend the old garment, but will of his own energy and knowledge give us an Introduction to the Study of Botany, which we doubt not would be worthy of appearing as one of the Clarendon Press Series, and which will wipe away the reproach, true to this of physiological botany as of the drama, that we are forced to fly with all too borrowed plumes.

E. P. WRIGHT

RECENT ELECTRICAL PUBLICATIONS

Electricity. By Robert M. Ferguson, Ph.D., F.R.S.E., of the Edinburgh Institute. New Edition, revised and extended by James Blyth, M.A., F.R.S E., Professor of Math. and Nat. Phil. in Anderson's College, Glasgow. (London and Edinburgh: W. and R. Chambers, 1882.)

Electric Illumination. By Conrad Cooke, James Dredge, M. F. O'Reilly, S. P. Thompson, and H. Vivarez. Edited by James Dredge. Chiefly compiled from *Engineering.* With Abstracts of Specifications, prepared by W. Lloyd Wise. Vol. I. (London: Office of *Engineering*, 1882.)

PROFESSOR BLYTH has done good service by the judicious additions which have to a great extent revivified Dr. Ferguson's well-known little manual, and brought it up to the level of the times.

The actual progress in electrical science since the original book appeared has not been anything extraordinary, but the amount of it which the public are willing to learn has undergone a prodigious increase, and the modern text-book is therefore expected to enter into details about a number of matters which a few years ago would have been scouted as altogether too difficult. It is these semi-advanced portions which Prof. Blyth has incorporated with the old stock of the work, the stock remaining about the same. There was very little to which one could object in the original; if it erred, it erred as a rule only by omission. In the new edition, however, we have information, and though concise it is mostly good and reliable information very intelligibly expressed, concerning Sir Wm. Thomson's electrometers, mariner's compass, and thermo-electric discoveries, also concerning electrostatic and electromagnetic induction, and other matters which had been but very lightly glanced at in the original edition. It also refers to Mr. Crooke's experiments, Mr. Spottiswoode's coil, Prof. Tait's thermo-electric diagram, and Dr. Kerr's discoveries. The operation of making a text-book may be compared to the operation of skimming, and the depth to which this operation may be safely carried depends, we suppose, mainly on the taste of the public at the time. Prof. Blyth has added to Dr. Ferguson's original skim a slightly deeper and more substantial layer; and fortunately neither of the authors have forgotten the very important preliminary operation of blowing aside the froth and scum which accumulates on long standing, and which an injudicious skimmer is very apt to obtain and exhibit as his sole result.

The book appears almost contemporaneously with Prof. S. P. Thompson's little work on the same subject, which we noticed some months back, and may be taken as complementary to it. Although both are of the same scope, yet the area open to them was so wide that it seldom happens that they both contain equally full information on precisely the same subjects.

The second volume which we here notice, viz. the compilation entitled "Electric Illumination," is of very different appearance and scope. It is a handsome large octavo, well printed, and with admirable illustrations. It is not addressed to students, but to engineers and practical men, and it is a most useful summary of notices which have appeared in the pages of *Engineering*, concerning dynamo machines, electric lamps, and the other paraphernalia connected with the practical applications of electricity. It aims, of course, more at completeness than at judicious selection; and it therefore naturally includes a number of contrivances which are hardly likely to come into any notorious existence.

While it is very useful as a book of reference, therefore it is scarcely calculated for ordinary reading, the style of the descriptions being not seldom tiresome, and giving one the usual dismal feeling of "letterpress" written up to a picture. Some of the sections are very full, as for instance that relating to the manufacture of Jablochkoff candles, where the account is so complete that the usual form of the Wheatstone bridge is depicted and carefully explained as if it were a specialty of the Jablochkoff system: while some other sections are distinctly meagre. At the same time it is only natural that some kinds of information should be easier of access than others, and that all that came to hand should be utilised. At the beginning of the book we have a sketch of the early history of dynamo machines, several admirable sketches of lines of force, and a very clear elementary exposition of the principles of magnetic induction. There are also very excellent and instructive skeleton figures of the Gramme and Siemens armatures, as designed by the late Antoine Breguet, though the writer of the article rather absurdly seems to take them as embodying researches which throw a new light on the action of the machines, instead of as useful and interesting illustrations of what was perfectly clear to every physicist.

Throughout the book, in fact, one comes across various curious statements, which, if read hypercritically and pressed, would be either annoying or misleading; but still more frequently one is in the presence of a cautious vagueness which conceals the want of exact knowledge by the turning of a phrase, and one notices a laudable desire to avoid the ascription of either praise or blame and to take the odiousness out of all comparisons.

But to say that some of the writers are often only half acquainted with their subject, and that they accordingly take precautions to avoid mistakes, is only to say that the book belongs to modern periodical literature; to that kind of literature, namely, which is written and read with the tacit understanding on both sides that in a few years at most it is sure to be out of date and forgotten, and that accordingly any serious labour expended on either its production or its assimilation would be labour misspent.

Taken for what it is, however, it is difficult to imagine a more complete and handy publication of information for which at the present time there is a great demand, and the book will be welcomed by all who take an interest, professional or otherwise, in those applications of electricity which are now so evidently imminent, and which must ultimately assume such vast proportions.

O. J. L.

OUR BOOK SHELF

Introductory Treatise on Rigid Dynamics. By W. Steadman Aldis, M.A. (London: G. Bell and Sons, 1882.)

THIS little work is truly characterised by its above title. The portions of the subject selected by the author will be best indicated by the headings of the several chapters. An introductory chapter on kinematics is followed by one on D'Alembert's principle: general equations of motion of a rigid body; impulsive forces. Chapter iii. treats of moments and products of inertia; Chapter iv. of motion round a fixed axis (centres of suspension, oscillation, and of percussion); Chapter v. of motion of a body with one point fixed; and Chapter vi. of the motion of a free body. Chapter vii. discusses certain general principles, as conservation of linear momentum, of moment of momentum, and of energy. In Chapter viii. miscellaneous problems are investigated, as of moving axes, initial motions, small oscillations, and "tendency to break." As might have been expected from so accomplished a teacher, the exposition of the general principles is most clear, and these principles are fully illustrated by a capital selection of exercises, many of which are solved, and for the solution of many others hints are given at the end. We know of no better introduction to this difficult branch of study. The text is most carefully printed.

Encyklopædie der Naturwissenschaften. Herausgegeben von Prof. Dr. G. Jager (and seven other gentlemen). Erste Abtheilung (Parts 16, 19, 20, 22, 24, 26, 27). (Breslau: E. Trewendt, 1880, 1881.)

THESE seven numbers form parts 6 to 12, *i.e.* the second volume of a "Handbuch der Mathematik," edited by Dr. Schlomilch, the several treatises being written by Dr. R. Heger, Professor at Dresden. The pagination is continuous (1-963 pp.), and there are 235 woodcuts.

The first treatise is on "Analytical Plane Geometry" (pp. 1-194). The first 164 pages are devoted to the conic sections: the mode of treatment, or rather the order of arrangement of propositions, is different from that of any English text-book with which we are acquainted, but approximates most closely to that of Dr. Salmon's classical work. It is a full, able, and interesting presentment of the properties of these curves. The remaining thirty pages are devoted to a rapid sketch of the principal known properties of curves of the third order, in which are embodied most, if not all, of the results of modern research.

The second treatise is on "Analytical Geometry of Three Dimensions" (pp. 195-380); the third is on the "Differential Calculus" (pp. 381-568); and the last is on the "Integral Calculus" (pp. 569-902).

This last work is broken up into three parts, of which the second treats of elliptic functions, the theta functions, and of elliptic integrals; the third is devoted to differential equations.

There are two smaller works, one (pp. 903-928) on the method of least squares (Ausgleichungs-rechnung), and the other (pp. 929-957) on insurances (Renten-, Lebens-, und Aussteuer Versicherung). A list of works on the different subjects treated of in the "Handbook" is given on pp. 961-963. These works are all in German, and the only English mathematician whose works are cited is Dr. Salmon, in Teutonic dress.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Pollution of the Atmosphere

In answer to Mr. Joseph John Murphy's letter in Nature, vol. xxvii. p. 241, stating that the radiation of marsh gas (from the incomplete combustion of coal) would be insignificant compared to that of vapour, I would like to say that their behaviour in the atmosphere is different; the moisture in the air is there, so to speak, on sufferance as long as the pressure and temperature allow it; that is, it is held in suspension by what might be called the capillary attraction of the air of a certain pressure and temperature. If you reduce the temperature you reduce the capacity of air for vapour, first by its reducing the capillary attraction, and secondly, by reducing one of the conditions that makes OH_2 a vapour. If you reduce the pressure, you enlarge the spaces between the molecules and reduce the capillary attraction, if I may apply this term to a gas. This is borne out by the balloon ascents of Mr. Glaisher. At 4 miles high the temperature was 8°, the dew-point was $-15°$, or a difference of 23°; at 5 miles it was 28°, and at 30,000 feet he states that there is no doubt that the dew-point is a difference of 50°; that is, the higher, the less vapour. But this will not be the case with marsh gas, as it is a permanent gas, and being of less density than even vapour, or about half the density of air, there is no reason why it should not be found in larger quantities at greater altitudes; and I think that its effect there would be that in the temperate zones and at the poles it would radiate its temperature of say from -8 at 30,000 to -30, and produce cold and rain, snow and floods as the storms on the Alps and the floods on the Continent, and in the Tropics to make the nights colder. In fact, it will have a tendency to do the reverse of vapour; vapour retains our heat and shields us from the cold of space. This radiator and absorber will tend to radiate the cold to us or to the vapour in the lower atmosphere, and produce rain and wind.

9, Bootham Terrace, York, January 13 H. A. Phillips

A "Natural" Experiment in Complementary Colours

Since I wrote to Nature last October (vol. xxvi p. 573) on the above subject, I have been both surprised and gratified to read no less than six communications on the same matter (vol. xxvi. p. 597, vol. xxvii. pp. 8, 78, 150, 174, 241). Only to-day I have received a letter from a German friend drawing my attention to Goethe's observation at Schaffhausen, he evidently being unaware of Mr. W. R. Browne's interesting letter (vol. xxvi. p. 597). My friend goes on to point out that "Gischt" is certainly foam, for in the context Goethe describes how he saw a rainbow in the "Dunst," or mist, thus enabling us to contrast the two words.

The special point of my communication was the excellent illustration, afforded us naturally, of the advantage of toning down the brightness of the white surface, upon which the complementary tint is to be looked, until that brightness is suitable to that of the exciting colour. In the experience related by me I was unable to see complementary tints in the foam, upon which full sunlight was falling; the glare of light was too strong.

Mr. C. R. Cross (vol. xxvii. p. 150) speaks of seeing them even in strong sunlight on the crests of waves; may not these crests have been in slight shadow, if the waves were just curling over? The example he gives of cloud *shadows* appearing purple on the ocean illustrates excellently my own observations. The letter from Mr. E. J. Bles (vol. xxvii. p. 241) gives a quotation from Sir C. Lyell, but without further detail I do not feel that much weight can be given to his observation from my special point of view. But I do not at all wish to say that complementary tints are not visible on a white surface in full sunshine; but theory and my own observations are certainly in favour of the advantage (and this is all I claimed) of a reduction of brightness to a level comparable with that of the exciting colour.

Chas. T. Whitmell

8, Maryland Street, Liverpool, January 15.

The Comet

In my letter relating to the September comet, published in vol. xxvii. p. 108, I was guilty of carelessness in copying from my notes the difference of micrometer readings instead of their value in arc. The value of one revolution of the screw is 15''·31075, and consequently the distances given in my letter should be as follows:—

$$\left.\begin{array}{l} a = 6''\cdot57 \\ b = 16\cdot90 \\ c = 8\cdot34 \end{array}\right\}\text{November 3}$$

$$\left.\begin{array}{l} a = 7\cdot36 \\ b = 16\cdot51 \\ c = 10\cdot15 \end{array}\right\}\text{November 6}$$

W. T. Sampson

U.S. Naval Observatory, Washington, D.C., January 2

The Transit of Venus

Will you be kind enough to make the following correction in your published report of the times of contact of phases of the transit of Venus. The third contact should be 2h. 39m. 57s. in place of 2h. 38m. 57s. These were both inadvertencies. From a comparison of Mr. Finlay's place at the Cape of Good Hope on September 8, I find that these elliptic elements satisfy the place within 7 seconds of arc in Right Ascension and 1·5 seconds of arc in Declination. Edgar Frisby

U.S. Naval Observatory, Washington, D.C., January 3

Early Coltsfoot

Last year I recorded (Nature, vol. xxv. p. 241) Coltsfoot in blossom on January 6, on the sides of the railway near here, probably an unprecedentedly early date. The mild weather lately prevailing induced me to suspect the former early blossoming might find a parallel *this* year. I saw the plant in flower *this morning*, near the same spot; one flower-stalk was fully four inches high, so it should have been observed some days previously had sunshine prevailed. Last year the winter was practically over (so far as hard frost was concerned) at the beginning of January. Will this be paralleled by the winter of 1882-83? R. McLachlan

Lewisham, January 12

Baird's Hare

Some of your readers may be interested in reading the following extract in which mention is made of a fact similar to that found in Nature vol. xxvii. p. 241, about *Baird's Hare.* The extract is from the Life of St. Francis Xavier, by H. J. Coleridge, S.J. In a letter written from Amboyna in May, 1546, Francis says:—

. . . "In the island of Amboyna I have seen what no one would believe . . . a *he-goat* giving suck to his young kids with his own milk; he had one breast which gave every day as much milk as would fill a basin. I saw it with my own eyes, for I would not believe it without seeing it. A respectable Portugese has the goat, and is taking it away, meaning to carry it to Portugal." T. Martyr

49, High Street, Clapham, S.W., January 15

The Projection of the Nasal Bones in Man and the Ape

The form and projection of the osseous frame-work of the human nose being considered by anthropologists of considerable value in a racial point of view, a close comparison has recently been made of the profiles of the external nose of man and the nose-case of the anthropoid apes. It has resulted in the conviction (1) that the absence of projection in the nasal bones of the chimpanzee, the gorilla, and the orang constitutes a distinction of more importance than has generally been assigned to it, and not the less so seems the fact (2) that a slight nasal elevation is observable in the skulls of some of the gibbons, and in the lower monkeys, as, for instance the baboons.

The distinction appears to me to be of the same kind as the erect position of man and the different order of the length of his toes as compared with the ape and many of the lower animals—as for instance the third toe in the lion, bear, dog, badger, and hare.

It should be remembered that the nasal bones in man form merely a bridge or back to the osseous structure of the nose, which is mainly due to the upheaval laterally of the pre-maxillary bones. These are less elevated in other animals, and there is no tilting of the nasals proper. In the chimpanzee and the orang the nasals are as flat as in the hippopotamus. On referring to Prof. Mivart's essay on the apes in the "Encyclopædia Britannica," I find he alludes to the transverse convexity of the bones of the nose, which he considers a marked character of man's skull, entirely absent in the chimpanzee. He adds: the nasals in the orang are exceedingly small and flat, "often even uniting in one bone."

In connection with the subject, it may be mentioned that in Quain's "Anatomy" the external nose is said to be due to the development of the frontal lappets in the fifth or sixth week of the human embryo. It is represented in a woodcut in Balfour's "Embryology" as well-formed and prominent so early as in the ninth week.

The existence of the nasal spine in the nostrils of man, but not in the ape or any of the lower animals, is a fact that has to be accounted for. It appears to have been overlooked, but is of some importance in connection with the development of the human nose.

J. PARK HARRISON

January 14

P.S.—The peculiarities of the human nose and the *rationale* of its formation are fully treated of in Prof. Humphry's "Human Skeleton," p. 220.

THE COMET

THE following communication from Dr Gould of Cordoba Observatory (Argentine Republic) appears in *Astronomische Nachrichten*, No. 2481:—

On September 6 I received information that a bright comet was visible in the east before sunrise. My informant had seen it on the morning of the 5th, and described it as being as bright as Venus and with a brilliant tail. Inquiry showed that it had been seen for several days by *employés* of the railroad and other persons whose duties required them to rise before daylight.

Not only was the morning of September 7 cloudy, but the eastern sky was overcast on every morning for a whole week. On one occasion it seemed that a part of the comet's tail could be distinguished, but not even an approximate position could be obtained for the head. On the morning of the 14th the comet was first seen at the Observatory, and an approximate position obtained from the circles of the equatorial telescope by pointings with the finder. It was then only 13′ south of the equator, and moving northwardly.

The telescope was equipped with the photographic lens and apparatus, and as my series of stellar photographs was nearly completed and its continuance for a few weeks demanded constant attention, I was reluctant to change the adjustments. It has been my uniform policy in Cordoba to confine our instrumental observations to the southern half of the sky, and, in general, to such regions as are not well visible from northern observatories. And as the comet had been conspicuous for more than a week, was on the equator, and the date of equinox was close at hand, it appeared unadvisable to sacrifice important and unique observations for the sake of determinations of the comet's position which I could not doubt were making under more favourable circumstances in the north. Consequently no micrometric observations were undertaken; but rude determinations of position were repeatedly made, from that time on, by use of the finder and the graduated circles, in order to follow the comet's course and deduce approximate elements and ephemeris.

On September 16 the brightness of the head was such, that it was visible with the finding-telescope throughout the day; and I prepared to observe it on the meridian, having followed it with the equatorial until within half an hour of the time of transit. Its declination was about + 0° 52′. But not more than five minutes before that moment a large cloud drifted across the meridian, making the observation impossible.

September 17 the comet was very bright and easily found in the full sunlight. At 10h. 40m. a.m. it was necessary to use a shade-glass, on account of its proximity to the sun; and at 11h. the sun and comet were in the same field of view. I again attempted to observe it upon the meridian, but was prevented by a new difficulty. The comet was hidden by the disc of the sun, and although I carefully scrutinised this and especially the preceding limb as it traversed the field of the meridian-circle, no token of the comet could be seen, nor could it be found during the afternoon. Although it must have passed in front of the sun, I then supposed it to have passed behind it and been occulted.

On Monday the 18th the comet was again on the preceding side of the sun and decreasing in declination at the rate of more than 2½′ hourly. Early in the day its brilliancy attracted popular attention throughout the country, and the "blazing star near the sun" was the one topic of remark. Telegrams came to me from all parts of the country, as well as from Chile and Uruguay, calling attention to the phenomenon. In the small telescope it presented the aspect of a brilliant nebulous mass, having at each end curved appendages like horns or wings, nearly large as the central body, and at their base quite as brilliant; the general form of the whole reminding one of the winged globes carved on ancient monuments. This appearance, unquestionably due to the outrush of glowing vapour from the nucleus, was also exhibited, although to less extent, on the two following days, during both of which the comet remained visible to the naked eye.

As soon as the elements of the orbit could be obtained, its similarity to that of the comet of 1843-1880 was manifest, and the suspicions regarding its identity and the hypotheses to which these gave rise presented themselves forcibly, as I am sure they must have done to astronomers in the northern hemisphere, where I doubt not they have long since been a theme of discussion. The perihelion-distance, although small, seems clearly larger than that of the orbits of 1843 and 1880; but how far such discordance is consistent with the hypothesis of identity must be decided by future investigation. The comparatively small amount of study which I have been able to give to the question leads me to think that the orbit deduced from observations before the perihelion may differ somewhat from that indicated by the observation since September 17; but as the Cordoba observations prior to that date were of a crude description, I have impatiently waited for tidings from other observatories. No. 2459 of the *Astr. Nachr.*, which has just reached me, leads me to fear that micrometric or meridian observations may not have been made before perihelion. In such case the rough positions obtained here with the finder and circles of the equatorial may possess a value far greater than was supposed possible at the time. Those previous to the perihelion are ten in number, and although I do not believe that their probable error can exceed a minute of arc in either coordinate, they are not represented within this limit by the elements deduced from observations made since the perihelion. Should no better positions have been obtained I will send these to you; but I cannot yet abandon the hope that some belated astronomer may have seen this brilliant object in season to secure a series of observations before the perihelion passage.

Micrometric determinations have been made here on various dates since October 17, and have now begun systematically, as the comet is growing lower for northern observers. But as it will not pass the limit of 30½°, S. Decl. the southerly observatories of Europe and all those of the United States will probably be able to follow it as long as it remains visible, and will find comparison stars in Argelander's Zones.

It will not have escaped your notice that all the elements differ from those of 1880 in the same direction in which these differed from those of 1843.

In the earliest observations made with the large telescope there appeared to be, in the place of the nucleus, a series of bright points following the axial line. The preceding and brightest of these seemed scarcely to exceed the tenth magnitude, and all were connected by intermediate material of somewhat less brilliancy which made it difficult or impossible to count them. Mr. Thome, who has made all the micrometric measurements, thinks that there were certainly not less than five or six, and perhaps more. The appearance was as though the original nucleus had been resolved into a series of ill-defined granules. These have gradually become less and less distinct, until the place of the nucleus now appears occupied by a line of irregular definition and unequal brightness, about 45″ in length, and of an average width of about 5″. All our determinations of position were made for the preceding and brightest of these nodules while they were clearly distinct; and, since then, for the anterior extremity of the bright line, where is a point which is still somewhat brighter than the remaining portions.

Since we are at present overloaded with work, in the preparation of the Zone-Catalogue, and observations are, without doubt, still making in Europe and North America, I will reserve the micrometric determinations until the reductions can be revised.

Those made on the meridian are as follows:—

	α	δ
	h. m. s.	° ′ ″
1882, Sept. 18	11 20 51·3	+0 16 39·3
19	11 14 31·0	−0 32 38·6
21	11 4 57·9	−1 59 30·2

Cordoba, November 14, 1882 B. A. GOULD

DESTRUCTION OF LIFE IN INDIA BY WILD ANIMALS

IN a recent communication I called attention to the loss of human and animal life in India from snake bites; I now proceed to describe the mortality due to wild animals, which, though much less than the former, is very considerable, and forms an important item in the mortuary returns.

The statement appended shows in detail for each province the number of persons and cattle killed by wild animals, and the number of wild animals destroyed, with the rewards paid for their destruction during the year 1881, as compared with the previous year. The figures are summarised in the following tables:—

Number of Human Beings and Cattle Killed by Wild Animals

	Persons killed.		Cattle killed	
	1880.	1881	1880.	1881.
Madras	223	238	8,667	8,668
Bombay	136	141	4,537	2,398
Bengal	1,295	1,367	14,567	8,423
North-Western Provinces and Oudh	561	470	8,140	7,971
Punjab	42	27	7,986	4,083
Central Provinces	289	248	3,711	2,929
British Burma	32	34	978	898
Coorg	Nil	Nil	219	191
Assam	234	211	3,269	2,802
Hyderabad Assigned Districts	24	18	3,560	3,013
Ajmere-Merwara	4	3	216	264
Total	2,840	2,757	55,850	41,640

Number of Wild Animals destroyed and Amount of Rewards Paid

	Destroyed.	Rewards			Destroyed.	Rewards.		
	1880.				1881.			
		Rs.	a	p.		Rs.	a.	p.
Madras	1,284	16,579	10	0	1,429	20,251	5	0
Bombay	1,717	4,775	1	0	1,367	4,965	13	0
Bengal	4,783	24,841	10	6	4,213	23,316	3	0
N.-W. Provinces and Oudh	2,924	7,295	4	0	3,037	8,434	14	0
Punjab	1,389	4,715	0	0	1,411	4,856	3	0
Cent. Provinces	1,408	17,887	8	0	1,351	15,842	0	0
British Burma.	639	3,468	0	0	1,059	4,260	8	0
Coorg	26	140	0	0	15	215	0	0
Assam	541	7,022	10	0	1,176	7,552	2	0
Hyderabad Assigned districts	167	1,590	0	0	216	2,156	0	0
Ajmere-Merwara	8	13	0	0	5	Nil		
Total	14,886	88,327	11	6	15,279	91,850	0	0

The resolution of Government, dated November 8, 1882, in dealing with this subject, gives the following details, which are so far satisfactory, as they show that organised measures are now being put in force for the destruction of wild beasts, and that already there has been diminution in the loss of human and domestic animal life. As in the case of venomous snakes, the prevention, or at all events diminution of loss of human and domestic animal life from the ravages of wild animals, is a question mainly of time, perseverance, and expenditure of money. The last consideration perhaps may have stood in the way of progress, not that expenditure of rupees either has been or would be grudged, were there certainty that it would overcome the evil, but that there may have been, perhaps is, a natural reluctance to spend public money for what seems an uncertain benefit, as some have regarded a system of rewards for destruction of snakes and wild animals. The Government of India has always evinced a desire to adopt any steps that might reasonably afford hope of relief, and many resolutions by the supreme and local Governments, and considerable expenditure of money with this object in view, proves that the authorities have been and are alive to the magnitude of the evil and to the importance of repressing it, and that they have taken measures which in some districts have been attended with a fair amount of success. But the absence of a thoroughly organised system of dealing with the evil, and the desultory and varying methods employed have prevented the attainment of the success that might fairly be expected and would be obtained under better arrangements; and it will not be until some complete organised system have been steadily and perseveringly prosecuted that the desired result will be accomplished. A few years ago (in 1878), when calling attention to this subject, I noted that the loss of life from wild animals in 1875 and 1876 had been as follows:—

Animals	Killed in 1875.		Killed in 1876.	
	Persons.	Cattle.	Persons.	Cattle.
Elephants	61	6	52	3
Tigers	828	12,423	917	13,116
Leopards	187	16,157	156	15,373
Bears	84	522	123	410
Wolves	1,061	9,407	887	12,448
Hyænas	68	2,116	49	2,039
Other animals	1,446	3,011	143	4,573
Total	3,735	43,642	2,327	47,962

Comparing these returns with that of 1880-81 it will be observed that the loss of life has not been materially diminished

	Persons killed.
1880	2,840
1881	2,757
1875	3,735
1876	2,327

though there is reason to hope that future yearly reports will be more favourable.

Registration is now becoming more accurate than it has been, and the returns are probably more reliable than they were, but they do not indicate any marked improvement on the whole. It is evident, however, from the terms of the resolution before referred to, that Lord Ripon is determined to deal vigorously with the evil, and, just as in the case of the poisonous snakes—only, perhaps, more surely—will the result, in time, justify the expenditure which must needs be incurred.

Of the wild animals and venomous snakes which destroy life in India, the wolf and tiger, it will be seen, are the chief offenders among the former, the cobra and bungarus (krait) among the latter. A list of the rewards that have been offered at various times and in different parts of India is appended, but I do not know the amount now offered for each animal, though it is probably much on the same scale. If these rewards be distributed regularly and systematically throughout India, they will probably suffice to insure a steady reduction in the number of noxious animals, and so will diminish a great evil.

"The figures quoted show a decrease during the year under review, as compared with the previous year, both in the number of persons and cattle killed; and, on the other hand, an increase in the number of wild animals destroyed. As was the case in the previous year, the mortality which occurred in Bengal and in the North-Western Provinces and Oudh, was far greater than in other provinces. Of the total number of deaths, 2757 were caused by wild animals, the figures for the previous year being 2840.

The number of persons killed in Bengal (747), and in the North-western Provinces, and Oudh (208) by wild animals other than those specifically named in the returns, was considerable. In future returns the animals which come under the general head "other animals," and which causes in all provinces a very large proportion of the mortality, should be specified in a foot-note, with the number of deaths caused by each kind.

The total number of cattle killed also decreased. This result is chiefly due to the exclusion from the Bengal return of sheep and goats, of which a large number were included in the figures of the year 1880. There has, however, been a marked decrease in the number of cattle killed by wild animals in the Bombay Presidency. In the Punjab, also, the number of cattle killed was considerably less than in the preceding year, but in this province, as in the case of Bengal, the decrease appears to be due to the exclusion of sheep and goats from the returns of the year 1881.

The number of wild animals destroyed was 15,279, against 14,886 in 1880. The number of tigers, leopards, bears, and wolves destroyed was 1557, 3397, 991, and 4538 respectively, as compared with 1689, 3047, 1100, and 4243 in the preceding year; and the number of human beings killed by these animals respectively, amounted to 889, 239, 75, and 256, against 872, 261, 108, and 347 in the year 1880.

Of the total amount of rewards paid during the year, Rs 91,850 were awarded for the destruction of wild animals.

In the review of the returns for the year 1880 a hope was expressed that endeavours would be made to induce men belonging to the Shikari class to devote themselves specially to the work of destruction in districts which are more than usually infested with wild animals, and Local Governments were authorised to make special arrangements for the experimental employment of such men. From the present reports it appears that the Government of Madras has decided that the employment of a paid corps of Shikaris is undesirable, as the cost of supervision would be excessive, while the employment of such a corps would discourage local Shikaris. On this point the Governor-General in Council desires to remark that where local Shikaris exist it is very desirable that every encouragement should be held out to them, and that in such cases it is preferable to trust to fixed, certain, and prompt payments according to results, as the most effective way of inducing the Shikaris to devote themselves to the work. At the same time certain tracts of country exist in which the special and temporary employment of men from outside may be very useful and expedient, and the reports show that the adoption of this plan has in some cases been followed by satisfactory results. For instance, in the Futehpore district, in the North-Western Provinces, the entertainment of a body of special Shikaris resulted in the destruction of a considerable number of wolves with which that district was infested. In Dinapore, in the Lower Provinces, also, professional hunters were engaged during the closing month of the year for the destruction of tigers.

"In the Central Provinces the ravages committed by tigers in the Balaghat and Seoni districts necessitated the offer of enhanced rewards for their destruction, and the district officer of Seoni has endeavoured to organise a special expedition of shikaris for the purpose of hunting down the animals, and has provided the shikaris with ammunition. Licenses under the Arms Act appear to have been more freely given than hitherto to persons who require arms for protecting themselves and their cattle and crops from the attack of wild animals, but the Governor-General in Council desires to take the opportunity of expressing a hope that this matter will be carefully kept in view by Local Governments and Administrations in order that every possible facility may be offered to cultivators and others for obtaining such licences in districts in which wild beasts are more than usually abundant."

Wild Animals destructive to Life in India

CARNIVORA	
Felidæ	
Felis—F. leo	Lion
F. tigris	Tiger
F. pardus	Leopard
F. jubata	Hunting Leopard
Hyænina	
Hyæna—H. striata	Striped Hyæna
Canidæ	
Canis—C. pallipes	Wolf
C. aureus	Jackal
Ursidæ	
Ursus—U. isabellinus	Brown Bear
U. tibetanus	Black Bear
U. labiatus	Sloth Bear.
UNGULATA	
Elephantidæ	
Elephas—E. indicus	Elephant
Rhinoceros—R. indicus	Rhinoceros
Suidæ	
Sus—S. indicus	Wild Boar
Bovinæ	
Gavæus—G. gauri	Bison, gaur
Bubalus—B. arni	Buffalo, arna
SAURIA	
Crocodilidæ	
Crocodilus—C. palustris	Crocodile
C. biporcatus	,,
C. pondicerianus	,,
Gavialis—G. gangeticus	Gharial
PISCES	
Carcharidæ	
Carcharias—C. gangeticus	Groundshark of Ganges

Poisonous Snakes of India

Those marked with an * are most deadly.
Those marked with a † are most common among the most deadly.

POISONOUS COLUBRINE SNAKES

Elapidæ

1. Naja	N. tripudians †, cobra, several varieties
2. Ophiophagus	O. elaps *, hamadryas
3. Bungarus	B. cæruleus †, krait
,,	B fasciatus, sankni
4. Xenurelaps	X. bungaroides
5. Callophis	C. intestinalis and several other species

Hydrophidæ, or Sea Snakes (all deadly)

1. Platurus	P. scutatus, P. Fischeri
2. Hydrophis	H. cyanocincta, and several other species
3. Enhydrina	E. bengalensis
4. Pelamis	P. bicolor

VIPERINE SNAKES

Crotalidæ, or Pit Vipers

1. Trimeresurus	T. gramineus and several other species
2. Peltopelor	P. macrolepis
3. Halys	H. himalayanus
4. Hypnale	H. nepa

Viperidæ, or true Vipers

1. Daboia	D. russellii †, Chain Viper, Tic-polonga
2. Echi	E. carinata †, Phoorsa snake, Afae, Kuppur

The following is a scale of the rewards offered in different parts of India, at different times, for wild beasts and snakes:—

TIGERS

	Rupees
Bengal	12½ to 50
Berar	10 ,, 20
Bombay	6 ,, 60
Burmah	5 ,, 20
Central Provinces	10 ,, 100
Hyderabad	20
Madras	50 to 500
Mysore	35
North-West Provinces	10
Oudh	None
Punjab	None
Rajpootana	10 to 15

LIONS

The only record of which I find official mention, is 25 rupees in Kotah.

PANTHERS, LEOPARDS, CHEETAHS

	Rupees.
Bengal	2½ to 10
Bombay	3 ,, 12
Burmah	5 ,, 10
Hyderabad	10
Madras	25
Mysore	15
North-West Provinces	5
Rajpootana	8 to 10
Central Provinces	5 ,, 12

WOLVES

	Rupees
Bengal	5 to 20
Berar	3 ,, 5
Bombay	4
Central Provinces	2 to 5
Madras	5
North-West Provinces	5
Oudh	1 to 6
Rajpootana...	5

HYÆNAS

	Rupees.
Bengal	1 to 2
Berar	5
Central Provinces	½ to 2
Madras	3¼

BEARS

	Rupees.
Bengal	1¼ to 2½
Berar	5
Bombay	3 to 12
Burmah	5 ,, 12
Hyderabad	5
Madras	5
Central Provinces	2 to 5
North-West Provinces	3
Rajpootana	5

SNAKES (Species not reported)

Bengal	4 annas
Berar	—
Bombay...	6 pie to 4 annas
Burmah...	—
Central Provinces	1 rupee
Hyderabad	8 annas to 2 rupees
Madras	1 anna
Mysore	8 annas
North-West Provinces... ...	2 rupees
Oudh	—
Punjab	2 annas
Rajpootana	1 to 8 annas.

No rewards appear officially proclaimed for elephants, buffaloes, or bisons. In cases of notorious rogue elephants rewards have been specially given. In Burmah 5 to 20 rupees offered for alligators; in special cases, more has been given in Bengal and Madras.

The difference in the amount of the rewards appears to indicate that higher sums were offered in special cases, probably when the creature was a notorious man or cattle-slayer.

Now I cannot help thinking that if Government made it part of the duty of district officers, not only to proclaim these rewards but to encourage the destruction of wild animals and snakes, by means of an organised establishment, which should be supplied in these districts, much benefit might result. The money rewards already offered would probably suffice for wild animals, but those for venomous snakes should be increased; if, at the same time, the people were encouraged to work for the rewards, and were aided by persons acting under properly selected superiors, the result would soon show a diminution of the wild animals and snakes. But, I repeat that not until some organised establishment is formed, to be worked steadily throughout the whole country—not dependent on the will or subject to the caprice of individuals, but under local officers subject to one head—will any real or progressive amelioration of the evil be effected. Such a department under a selected officer, would, as was the case with the Thugs and Dacoits, soon make an impression on a death-rate which, so long as it continues in its present condition, must be referred to a defect in our administration.

J. FAYRER

PALÆOLITHIC IMPLEMENTS OF NORTH-EAST LONDON

IN 1855 Prof. Prestwich published in the *Quarterly Journal of the Geological Society* an account of a fossiliferous deposit in the gravel of West Hackney. The precise locality of the excavation is given, and from 1855 to now many neighbouring excavations have been made. They almost invariably exhibit the "Palæolithic Floor." In 1855 only little was known of palæolithic implements, yet it is a remarkable thing that none of these objects, so common and well-made as they usually are at West Hackney, arrested Prof. Prestwich's attention. It is also remarkable that although a list of twenty-three land and freshwater shells is given in that paper, yet it does not include the only two of especial interest, viz. *Corbicula fluminalis*, Müll., and *Hydrobia marginata*, Mich.; the first of which is extremely common, and the latter frequent. The branches of trees, "sharply broken into short pieces," and the fossil bones, "showing no trace of wear or fracture," are frequent in all the West Hackney pits. One may be sometimes very near a curious discovery and yet miss it.

In NATURE, vol. xxvi. p. 579, I described and illustrated the West Hackney, or Stoke Newington, palæolithic gravels as understood by me, confining myself to the geological aspects of the situation. I now approach the subject of the weapons and tools contained in the drift of that place. Of the stone implements there are three distinct varieties, each belonging to a different geological time. The implements of these ages are not confined to the valley of the Thames, as they are marked with almost equal distinctness in other places as at Canterbury, Bedford, Southampton, and elsewhere.

In looking for the oldest human works, it would be unreasonable to expect symmetrical implements. The very earliest weapons and tools used by our most remote precursors must have been natural or accidentally broken stones:—naturally pointed stones and stones with a naturally suitable cutting or chopping edge; the first attempts at implement making must have been at the time when the primæval savages "quartered" a stone by smashing it, and then selected pointed and knife-like pieces of this stone for tools.

None of the following rules are without exceptions, for amongst the implements which are usually very large, a very small specimen may now and then occur; and amongst those which are usually very small, there may be at times a large example. The lustre and deep ochreous tints may at times vary a little. Notwithstanding exceptions, when all the characters are taken together, the distinctness of the three classes will hold good.

The oldest known tools are the rarest, and, according to my estimate, can be recognized by the following characters:—they are generally lingulate, or club-shaped, with a heavy butt, often rudely ovate, never acuminate, generally large and very rude, frequently with a thick, ochreous crust, and always greatly abraded, as if they had been tossed about for ages in the sea. Some of these implements are so much abraded that they have lost almost every trace of flaking. These old implements acquired their ochreous crust before they were buried in the gravel, as they occur amongst sub-angular lustrous flints and even chert gravel, where only the implements and a few stray stones exhibit the ochreous crust. I have seen no trimmed flakes or scraping tools belonging to this older age. In London, these old implements are generally found near the bottom of the twenty feet (or even thirty feet) excavations. At Canterbury they occur in thin seams of distinct ochreous material where all the contained flints have an ochreous surface. All these older tools were made at a long distance from where they are now found. Two Canterbury examples are illustrated, half actual size at Fig. 1 and 2, Nos. 100 and 126 in my collection. A very important point has now to be especially noticed: when these ochreous instruments were originally tossed about and buried in the gravel some of them became chipped and even broken. Now, the chipped and broken surfaces of these older implements, as at A A, Fig. 1, are *never ochreous*, but invariably of the natural colour of the flint and lustrous. This lustre has been acquired since the gravels were laid down, and it exactly agrees with the lustre of the sub-abraded lustrous implements of medium age found from 8 feet to 10 feet above the ochreous ones. It follows, therefore, that the lustrous implements, although enormously old, can only be as old as the time when the ochreous ones were bruised and broken in the gravels where they are now found.

Another fact must be mentioned here: the men who used the oldest known tools sometimes broke them in two whilst they were at work with them; the accidentally fractured surfaces of this class are of course as old as the tools, and therefore always ochreous. Points and butt ends wholly ochreous are of common occurrence: these pieces of tools must have been shattered for long ages before the gravels of middle age were laid down.

The men who made and used the rude ochreous tools were to a great extent a "whole handed" race—they had not learned the full use of their fingers but held the weapons as one would now hold a heavy stone for smashing. It is probable that the more pointed end of the club-shaped implements was sometimes grasped in the hand and the butt used as a club or hammer. The absence of scrapers indicates that the men probably knew nothing of dressing skins, and were unclothed.

In and near London lustrous and sub-abraded tools of medium age are commonly found at a depth of 12 feet; these tools show a distinct improvement in workmanship over the older ones. Most of the examples are lingulate and acuminate, and the butt, and sometimes the umbo, shows signs of hammering, the ovate form is not uncommon, but the cutting edge all round I have not yet seen. A few chisel ended implements occur. Rude choppers and somewhat large scraping tools are common. All the artificially chipped stones of this medium age are sub-abraded and lustrous. They were not made where now found, but have been carried by the drift for a short distance. A pointed weapon and chopper of medium palæolithic age are illustrated half real size at Figs. 3 and 4, Nos. 588 and 482 in my collection. A scraper of the same age is illustrated at Fig. 5, Scraper No. 9 in my collection.

When the lustrous sub-abraded tools were made the men had by that time acquired the habit of holding their weapons in a lighter fashion,—still in the palm, but more lightly held with the thumb and two next fingers. The frequent presence of horse-shoe and side scrapers now indicates that the men had possibly learned to rudely dress skins for clothing. Sometimes unfinished implements are found; one of medium age from Lower Clapton, London, is illustrated at Fig. 6. The dotted line shows where the point would have been, if the maker had finished it. Implements roughly blocked out to form, and without any secondary trimming, are common: it would appear that the men sometimes first blocked out a number of implements rudely with a heavy hammer-stone, and afterwards finished with neater fabricating tools. An implement in a preparatory stage, of which I have many similar examples, is illustrated in Fig. 7, from Bedford. Many implements were accidentally shattered in the course of manufacture, and the shattered failures are common in all implementiferous gravels.

Long after these two classes of tools were buried by floods of water deep in the gravel and sand, there lived a third race of palæolithic men, as far removed from the men who made the lustrous sub-abraded implements as these latter men were from the makers of the ochreous and highly abraded instruments. These newer tools are found at Stoke Newington at about 8 feet above the lustrous examples, and generally about 4 feet from the present surface. In some places so much top material has been taken off for brickmaking that the stratum containing the newer implements is almost exposed on the surface. Denudation since palæolithic times has considerably altered the contours round north London, and the "Palæolithic Floor" at South Hornsey, close to Stoke Newington, is 14 feet below the surface instead of 4 feet—this 10 feet has been removed in some places by the rains of centuries, in others by modern brickmakers and nurserymen.

The newest palæolithic implements are as a rule not highly lustrous as in the last, but sub-lustrous, and often even dull; not abraded or sub-abraded, but as sharp as on the day they were made. As a rule they are very much smaller and lighter than anything belonging to the two previous periods. An example is illustrated half real size at Fig. 8, No. 403, in my collection. Other characteristic specimens are illustrated at Figs. 9 and 10. Fig. 9 is a thin and exquisitely manipulated trimmed-flake, No. 47 in my collection, weighing only $1\frac{3}{10}$ ounces. Fig. 10 is an implement worked on both sides, the natural crust of the flint being left untouched on the butt, weight only $1\frac{1}{6}$ ounces, No. 627 in my collection. Oval implements with a cutting edge all round now appear; a few examples, as in the last period, occur where the broad end (as in neolithic celts) appears to have been designed for cutting or chiselling; scrapers are common, not large and rough, but as a rule small and extremely neat. One is illustrated at Fig. 11, half actual size, scraper No. 22 in my collection; small knives, *i.e.* flakes, with the edge or edges showing very neat secondary trimming, are common, and hardly to be distinguished from neolithic "knives." As a rule every object is now neat, small and fine.

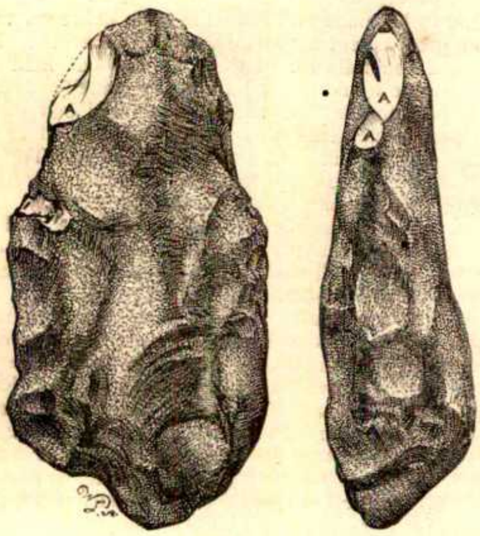


FIG. 1

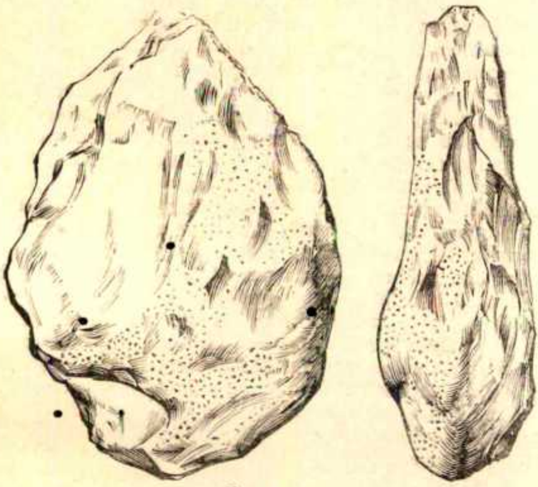


FIG. 2.

That these later implements are of a different age from the last is proved by the curious fact that the newer implements are sometimes re-made from older ones, *i.e.* re-trimmed after the lapse of a vast period of time. I have several such examples, one a scraper belonging to the "Palæolithic Floor": it is made from an old lustrous flake of medium age, all the more recent work being dull and sharp. At Fig. 12 is illustrated, half real size (No. 452 in my collection), an implement of later palæolithic age from Bedford. It is an old implement that was "found" after a lapse of time by a newer palæolithic man and re-pointed. The finder had probably sense enough to know that the thing he found was really a human-made implement, only wanting a little fresh work to make it "as good as new." This man stands in contrast with the very few individuals (still extant) who say they can see no evidence of design in drift tools. The original form of the implement is indicated by the dotted lines, C, C, C; the natural crust of the flint is present at the base on both sides, shown by dots in the illustration. The original mid-age flaking is shown at B, B, B, B, and the work of the newer palæolithic man is exhibited at D, D, D. The old finder of the implement gave two new edges and a new point to the tool, and improved the shape of the butt; the newer work is creamy white and lustrous, and in distinct contrast with the older work. When this implement was thrown out of the pit by the workman, the newer point got accidentally injured, at E, F. This injury, by exposing the interior of the flint, shows that the tool was originally a greyish-black one, and that since it was last pointed, it has acquired a thick, white bark by the decomposition of the flint. Now, neolithic flints at Bedford (where the example under examination was found) remain blackish-grey to the present day; the thousands of years (say from two to ten) since they were chipped have been insufficient to cause even the thinnest conceivable while film of decomposition to appear, but this palæolithic example has acquired a white bark of a sixteenth of an inch in thickness. How much older then must this *new point* be than the neolithic flints from the same place. The new point being inconceivably old, how much older must the old butt be! The implement, how-

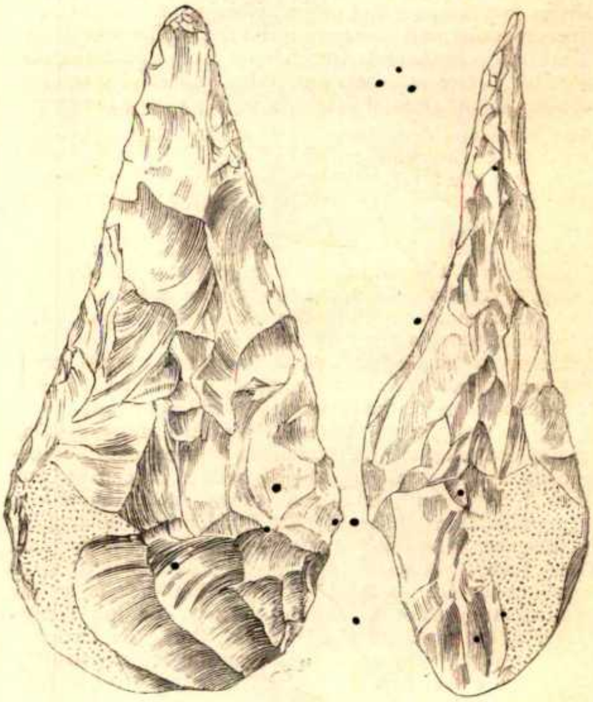


FIG. 3.

ever, that was "found" (as proved by the flaking of medium age) was new as compared with the older, highly-abraded examples. There is other evidence of the extreme antiquity of these things. They are *all* beneath the "trail and warp." Now the "trail" belongs to geological time; and the period of its deposition is so remote that one can only guess at its age in years. The newest palæolithic implements are every one beneath and older than the "trail," how very much older, then, must be the oldest implements. The proofs that they are really older I have given.

The tools of the later palæolithic period show a marked development of the hand in the makers, for the chippers of these later tools had learned to hold small instruments with the fingers, much as we now hold a small pen, pencil or knife. From the rude and heavy bludgeon the men had advanced to beautiful oval and ovate forms almost perfect in geometrical precision. The progress from the large and rude, to the extremely small and neat scraper, shows that the men had probably progressed in the art of dressing skins, and in every way did finer and neater things. That these men and women now wore necklaces, and possibly bracelets, seems proved by the fact of specimens of *Coscinopora globularis*, D'Orb., occurring with the natural orifice, artificially enlarged. I have several specimens thus enlarged from a horde of more than two hundred, examples all found together near Bedford. Mr. James Wyatt, F.G.S., noticed a similar fact, as recorded by him in the *Geologist* 1862, p. 234. These later palæolithic men lived in large and probably peaceful companies, and their living places stretched in unbroken lines on the old river banks. The "Palæolithic Floors" are not little isolated patches, but places extending for many miles, how large they are it is impossible to say from paucity of excavations.

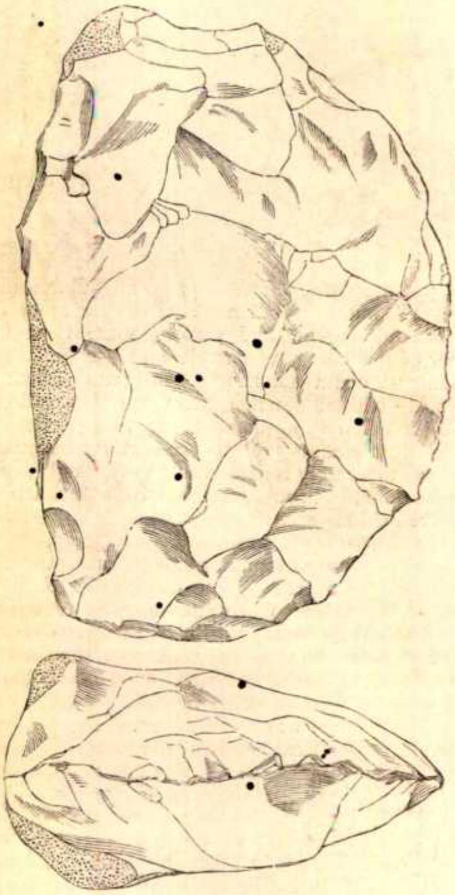


Fig. 4.

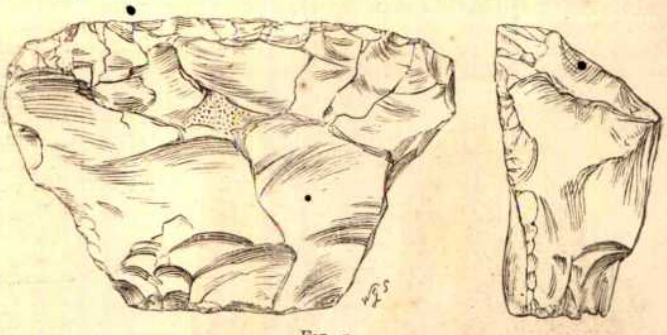


Fig. 5.

They are not confined to the valley of the Thames, but they occur in many places.

The newer implements and those of middle age are innate with, and have belonged to the gravel from the first. The older implements are distinctly "derived" like the cretaceous fossils commonly found in the gravel. We know whence the fossils have come because they are so common, the abraded ochreous implements on the other hand are very rare, and this rarity makes it difficult to say whence they have been derived, they possibly belong to none of our existing rivers. As in 1868 (*Journal of Anthropological Institute*, Feb. 1879), I recorded my discovery of flakes and implements in the so-called middle-glacial gravel of Amwell, Ware, and Hertford, I have little doubt that the older implements found at North-East London have been derived from these positions. Whether the above-mentioned gravels are really glacial or not, I am not prepared to decide. How the implements got into the gravel I cannot say. I found them in the ballast thrown out of the pits and in the pits themselves. If the gravel is glacial could not glaciers have swept up flakes and tools from old surfaces in the same way as the "trail" has undoubtedly done?

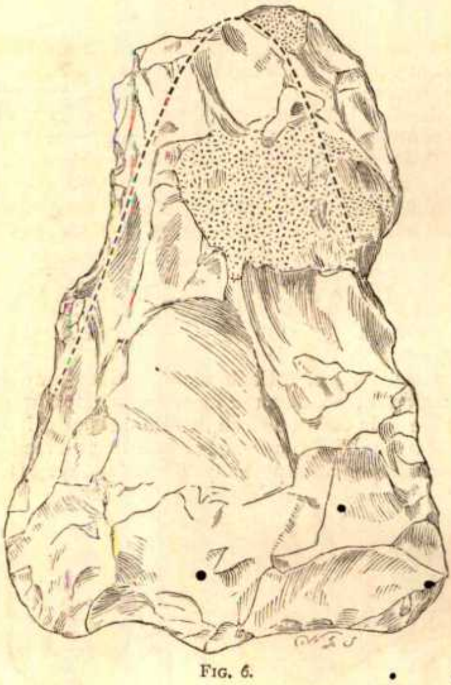


Fig. 6.

Great caution must be exercised in the acceptance of implements as of glacial age, even if found on the surface of glacial gravels. Men of the later palæolithic age lived only seven miles south of Ware, and there is no reason why they should not have strayed over those high positions. Some of the later tools have glacial striæ on the original crust.

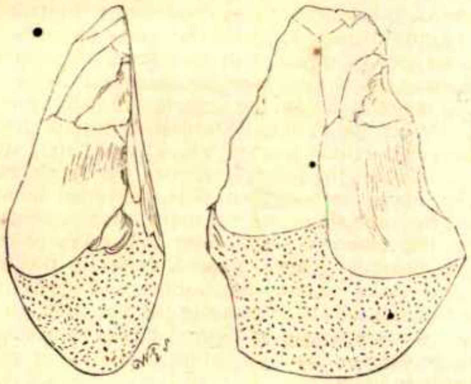


FIG. 7.

There is apparently, but perhaps not really, a gap between each of these three palæolithic periods, as there is apparently a gap between palæolithic (in its vague general sense) and neolithic times. Each older period however, has forms which foreshadow the forms which follow in succeeding periods even down to neolithic times. No doubt the fossil bones, if a good series could be obtained, would show a succession of, or possibly different groups, of animals in the different deposits, but the bones, antlers, and teeth met with by me, are at present insufficient to define any such groups with distinctness.

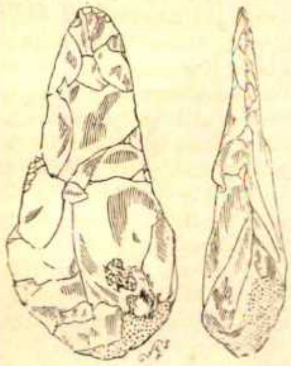


FIG. 8.

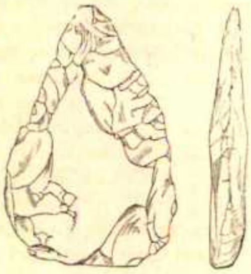


FIG. 9.

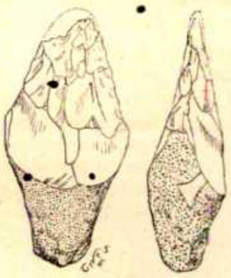


FIG. 10.

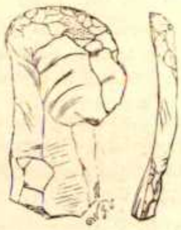


FIG. 11.

The day will come when we shall know much more of palæolithic men than we know now. At present we only know that such men once existed and made weapons and tools of stone during long periods of time. How or where they first appeared as human creatures we can only guess. When we know more we shall modify our use of such terms as "River Drift Men," "Cave Men," &c., and we shall probably be able to mark out more or less distinctly a succession of men, a succession of geological events, and a distinct succession of progressive steps in the men from the lowest savage to the barbarian. Some of our ignorance is undoubtedly caused by the undue attention that has been bestowed on the collection of ornate implements and to the employment of gravel-diggers for their collection. No greater mistake can be made than the mere getting together of the more highly-finished and perfect implements. We only learn from them that certain makers, at first few and far between, common at last,—acquired extraordinary skill in the manufacture of stone tools and weapons. For one perfect example, twenty have their points, butts, or edges injured either by peaceful or warlike work. Collectors will not put the damaged examples and failures in their "cabinets;" but every damage tells some story of the use of the implement, and throws some light on the character of the being who made and used it.

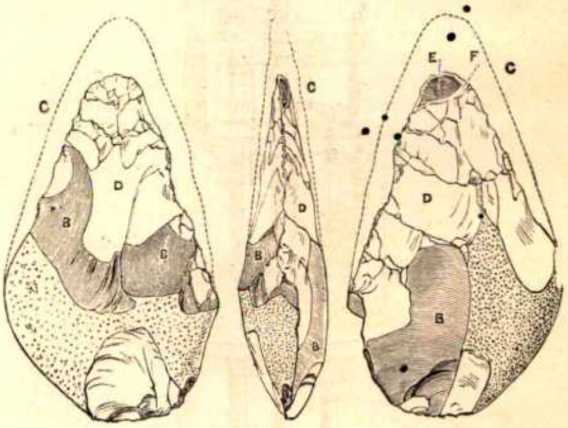


FIG. 12.

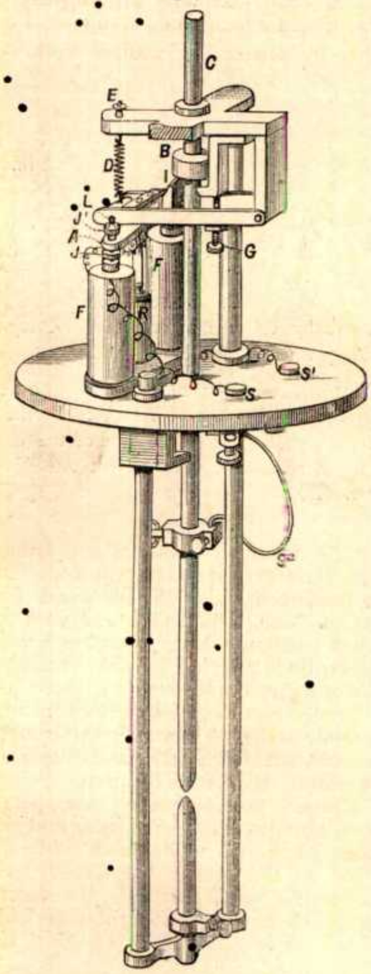
Implements could not have been made without fabricating tools—without punches, hammer-stones, and anvils; —where the ordinary implements are, these latter things also are. Implements such as are seen in museums are only fit for moderately rough work; very rough work was sometimes done, but rough and massive stones artificially worked are seldom seen in collections.

Knives, scrapers, wedges, heavy choppers, punches, anvils, cores, abraded hammer-stones, and other things have all been recovered by me from Stoke Newington, London; but as this paper has already exceeded the limits set apart for similar articles, the description and illustration of these less-known objects had better be deferred.

WORTHINGTON G. SMITH

LEVER'S ARC LAMP

SO many rival forms of lamps have recently been devised for regulating the electric arc light that even specialists in this branch of applied science have some difficulty in keeping up a knowledge of all the various systems. Amongst those, however, there is a tolerably well-defined class of lamps in which the movements of the carbon-holder are regulated by a clutch or kindred device, which grips the holder and raises it, lowers it, or releases it when required. Clutch lamps date back, indeed, to the year 1858, when a lamp of this type invented by Hart, the instrument maker, received a prize from the Royal Scottish Society of Arts. Amongst the more modern forms of clutch lamp those which have hitherto found favour with the public are the well-known inventions of Brush and Weston. Though the clutch device is in itself simple and efficient, the difficulty which has beset the action of such lamps has been that of arranging suitable electric mechanism to work the clutch. In Hart's lamp an electro-magnet through the coils of which the main current passed on its way to the lamp, lifted the clutch, and again released it when the increasing resistance of the arc interfered with the strength of the current. In the lamps of Weston and of Brush a much more complicated arrangement was adopted, the magnets which worked the clutch being in both these patterns of lamp wound "differentially," that is to say, with a coil of fine wire connected as a shunt to the lamp, acting in opposition to another coil of thick wire through which the main current flowed. This differential principle was originally applied in the Siemens' lamp, wherein, however, no clutch was used. In the Pilsen lamp, and in many others, combinations of shunt magnets and main-circuit magnets have been similarly applied. The lamp which we illustrate in the figure, the invention of Mr. Charles Lever, of Manchester, is a clutch lamp, but of remarkably simple, yet efficient construction. And as it possesses sundry points worthy of notice from a scientific aspect, we will briefly describe it. The upper carbon is clamped in a holder or carbon rod C, which consists of a tube of brass sliding smoothly through the upper framework of the lamp. Fitting accurately, but not tightly to it, is a brass washer, or collar, B, which is supported from below or on one side by an adjustible screw, G, and on the other by a metal piece, I, projecting from the jointed framework below. This framework is held up by a spiral spring, D, which, when the lamp is not in action, keeps the piece, I, pressed up under the washer, B, and tilts it. When thus tilted it clutches the carbon-holder, C, and raises it. Attached to the under-side of the jointed framework alluded to is an iron bar, A, bearing two broad-ended iron screws, J, below which, again, are seen the two limbs of an electro-magnet, F F, with the poles upward. This electro-magnet is wound with fine wire, and connected as a shunt to the lamp. Now, as described above, when the lamp is not in action, the carbons are held apart by a spring. When the current is turned on it must therefore pass through the shunt magnet, which immediately attracts the bar, A, lowers the piece, I, releases the clutch-washer, B. The upper carbon then falls, and the current is diverted from the shunt-magnet to the lamp itself, passing through the carbons. But when this takes place, the spring, D, being no longer opposed, draws up the framework, and picks up the clutch, thus raising the upper carbon through the space requisite for the production of the arc. A more simple or efficient mechanism would be difficult to devise; and its action is extremely regular and steady in practice.



NOTES

PROF. HUXLEY has been appointed to deliver the Rede Lecture (Cambridge) this year.

MR. G. H. DARWIN, M.A., F.R.S., has been elected to the Plumian Professorship of Astronomy and Experimental Philosophy at the University of Cambridge, vacant by the death of the Rev. James Challis. This was the first election to a professorship since the approval of the new University Statutes by Her Majesty in Council. By the new statutes the election to certain professorships is vested in a Board nominated by the Special and General Boards of Studies and by the Council of the Senate, the persons so nominated being elected by the Senate. The members of the Board appointed to elect to the Plumian Professorship are the Vice-Chancellor, Prof. H. J. S. Smith, of Oxford, Mr. W. H. M. Christie, the Astronomer-Royal, Mr. W. Spottiswoode, President of the Royal Society; Professors Adams, Stokes, Cayley; Dr. Ferrers, Master of Gonville and Caius; and Mr. Isaac Todhunter, of St. John's.

THE subscription for the Darwin Memorial has awakened so much enthusiasm in Sweden that the local committee there formed has received subscriptions from no less than 1400 persons, including "all sorts of people," writes Prof. Loven in a letter to the English Committee, "from the bishop to the seamstress," the sums varying from five pounds to twopence. The English Committee, which has its head-quarters at the Royal Society, London, has now received (inclusive of subscriptions from abroad), 4000*l.*, but the number of subscribers in the United Kingdom is only about 600. From this it would seem that an interest in science is not nearly so widely spread in Britain as it is in the more thinly peopled land of Sweden.

IN announcing the death of Mr. Darwin to the American Philosophical Society at its meeting on April 21, 1882, Dr. Le Conte stated the general bearing of Darwinism in a striking and unusual way:—"To no man more than to Darwin does the present age owe as much, for the gradual reception of the modern method of close observation over the scholastic or *a priori* formulæ, which, up to a brief period, affected all biological investigations. To him, above all men, we owe the recurrence to the old Aryan doctrine of evolution (though in those ancient times promulgated under the guise of inspiration) as preferable, by reasonable demonstration, to the Semitic views, which have prevailed to within a few years, and are still acceptable to a large number of well-minded but unthinking men. The doctrine of evolution, in its elementary form, means nothing more than that everything that exists has been derived from something that pre-existed; that the former is related to the latter as effect is to cause. And it is most pleasing evidence of the acceptability of this doctrine, that it is now heard from many pulpits in the land, and is a strong illustration of the instructions which are thence given."

LETTERS have been received from Mr. Forbes dated from Shonga, on the Niger, at the end of October last. Shonga is a small trading-station a short distance up a creek on the right bank of the main stream some fifty miles below Rebba. Mr. Forbes had been there three weeks, and was expecting to remain about three more, when the steamer would call for him, and try to get up to Sokoto—an excursion that would occupy at least six weeks. After this Mr. Forbes would return direct to England. Having been pulled down by fever and the want of good food, Mr. Forbes had not been very successful in his collections at Shonga. His list of species of birds obtained at the date of his letter was only 105, and the difficulty in obtaining spirit had interfered with the preservation of fishes, of which many species were abundant.

IN a collection of birds and insects just received from Mr. Andrew Goldie by Messrs. Salvin and Godman are specimens of a fine new Bird of Paradise, obtained in the D'Entrecasteaux Islands, south-east of New Guinea. This species, which belongs to the restricted genus *Paradisea*, is shortly characterised by Messrs. Salvin and Godman in the last number of the *Ibis* as *Paradisea decora*, and will be fully described and figured in the next number of the same journal.

THE *Lancet* is happy to be assured that the rumours respecting the infirm state of health of Prof. Owen are unfounded. The large circle of the professor's friends will share with us in the hope that his valuable life will be prolonged many years beyond the seventy-nine which it has already reached.

A GROWING want has for some time been felt by lecturers on biological subjects, and especially by those whose lot it is to address large audiences or classes, of a good series of lantern slides, which would do for biology what has been so well done for physical science by York's series of slides. The ever increasing use of the oxyhydrogen lantern as a means of illustration, especially with popular audiences, renders this need more apparent. Arrangements have, however, now been made with Messrs. York and Son, 87, Lancaster Road, Notting Hill, London, W., to issue such a series, under the supervision of Dr. Andrew Wilson and of Mr. Wm. Lant Carpenter, to whom, at 36, Craven Park, Harlesden, London, N.W., or to Dr. Wilson, 110, Gilmore Place, Edinburgh, any communications on the subject may be addressed. It is intended that, in the first instance, the series shall comprise some of the principal types and life-histories of the lower forms of plant and animal life, and the elementary facts of animal and vegetable physiology. It is believed that the knowledge that these are in preparation, may save the construction of diagrams by some lecturers, and may lead others to make valuable suggestions as to sources of illustration, &c., to one of the above named gentlemen.

PROF. COOK, of Canterbury College, New Zealand, points out in the new number of the *N.Z. Journal of Science* that while the colony is remarkably well provided with museums, it is entirely without a public astronomical observatory. It is a fact that some years ago about 250*l.* were collected for such an observatory, but it came to nothing. We heartily endorse Prof. Cook's able advocacy for the foundation of an observatory in New Zealand, which, if perfectly equipped and directed could not fail to do good work. Out of a total of ninety-five observatories in the *Nautical Almanac* only eight are in the southern latitudes.

AT the Guildhall last week Dr. Siemens and Dr. Percy were each presented with the freedom and Livery of the Worshipful Company of Turners. The honour was conferred upon Dr. Siemens in recognition of his eminence as an engineer, his successful application of physical science to valuable practical purposes, especially electricity and metallurgy, and his personal support of technical education. The new member made a suitable reply in returning thanks for the honour conferred on him, an honour which was specially precious to him, and of which he should ever be proud. Referring to electricity, he said it was a new science, the applications of which had all to be developed, and in the development of which wonderful results had been produced. In the case of Dr. Percy, the honour was conferred in recognition of his distinguished scientific attainments, especially in connection with metallurgy, the great value of his researches, and his teaching not only to turners, but to all workers in metal.

THE German Fishery Society has petitioned the Reichstag to make a grant of 10,000 marks, chiefly to enable Germans to take part in the approaching London Fishery Exhibition. It is desired that an official delegate should represent this Empire in London in connection with the enterprise.

THE astronomical observatories of Greenwich, Kiel, Pulkova, Vienna, Milan, Paris, Utrecht, and Copenhagen have fixed on Kiel as the centre for astronomical telegrams. For an annual payment of five pounds each of the above-mentioned observatories will receive by telegraph information of every fresh astronomical discovery wherever made.

DR. SCHLIEMANN is desirous of commencing a new series of excavations in the North-West of Athens. In the neighbourhood of the old Academy was the site of the official burial-ground, and there were buried the ancient Athenians who had fallen in battle. Dr. Schliemann hopes in this spot to find the grave of Pericles. At a subsequent period it is his intention to begin fresh excavations in Crete.

IN an address on education at Birmingham on Monday, Mr. Mundella said: "They were asked if they were not over educating; he said no, and he would tell them why. Our idea of education was the lowest, certainly, on this side the Alps. Those who had the longest experience in education, those nations which had spent the most on it, were at this moment making the greatest efforts. The educational impulse throughout Europe was something they could hardly believe, and it was so because the people on the Continent had found that knowledge was power, not only military power, but industrial power. Whereas in Birmingham, last year, the expenditure on education was 2*s.* 3*d.* per head, in Paris it was 12*s.*"

THE following gentlemen have kindly promised to deliver popular lectures, with lantern illustrations, at the Royal Victoria Coffee Hall, Waterloo Road, on Friday evenings at 9 o'clock. January 19, Mr. Wm. Lant Carpenter, B.A., F.C.S., on "The Telephone and how to talk to a man 100 miles away." January 26, Mr. C. A. V. Conybeare on Pompeii. On February 2, instead of a lecture a magic lantern entertainment, entitled "Here, There, and Everywhere," will be given by Major George Verney. February 9, Mr. E. B. Knobel (Sec. R.A.S.), "The Sun and his Family, with a glance at other Suns."

ACCORDING to the *Journal* of the Russian Physico-chemical Society, the priority in photographing with the electric light belongs to the well-known St. Petersburg photographer, M. Lewitski, who obtained such photographs in the winter of 1856, on the following occasion:—To produce the electric light during the celebration of the coronation of the Czar Alexander II. at Moscow, a Bunsen battery of 800 elements had been constructed.

The following winter this battery was taken to St. Petersburg, and Prof. Lenz demonstrated its action to a distinguished auditory, formed of members of the Imperial family and generals of the army. It was during this lecture that M. Lewitski obtained a photograph of the professor. A positive of this portrait was presented by M. Lermantoff to the Russian Physical Society at the *séance* on December 14, 1880. It is by no means a poor photograph, but full of detail in the shadows and half tints.

In a recent report of the Berlin Physical Society (p. 95) we referred to some valuable observations by Dr. Kœnig with Prof. Helmholtz's new instrument, called the *leukoscope*. We observe that a detailed account (with illustration) of the instrument and of the results obtained with it, appears in *Wiedemann's Annalen*, Nos. 12 and 13 of last year.

Prof. F. W. Putnam has concluded a very successful course of lectures at the Peabody Museum, Boston, on some of the most interesting of American antiquities. The *Boston Evening Transcript* in an article on the lectures says:—"It is to be hoped that the curator will not again be retarded in his work from the want of means for its prosecution, when he has shown, as he has in this course of lectures, how much can be done at comparatively little expense under proper methods of research. As he said in his lecture, what is to be done must be done at once, and it would be a great pity to have the opportunities now open to him lost to science. The ancient city known to the present inhabitants of the Little Miami Valley, thirty-five miles east of Cincinnati, as 'Fort Ancient,' would be worth to American scholars for study as much as any of the old Greek cities that have been so thoroughly dug over by European explorers and students. Certainly American scholars should lead in American archæology and ethnology. The restoration or preservation of these wonderful remains of a comparatively enlightened prehistoric American people would be a glorious monument for any American Institution of learning and science."

Shocks of earthquake have been felt in the province of Murcia, in Spain. Seven shocks occurred at Archena on the 11th inst. Shocks have also been felt at Fortuna, Muta, Ricotel and other towns in Murcia. Eleven distinct shocks were felt on Tuesday morning at Archena, between the hours of three and six. Some lasted fifteen, and others lasted two seconds. An earthquake of a few seconds duration was experienced at Kultorp, near Kalmar, in Sweden, at 8.50 p.m. on the 12th inst. A slight shock of earthquake was felt at Monmouth at five o'clock on Tuesday evening, accompanied by a light, rushing noise. The wave seemed to pass from south-east to north-west.

A remarkable discovery of the elder *Runic* inscriptions has just been made in Ryfylke in Norway. The characters have been made on a stone, the arrival of which in Christiania is awaited with great interest by *savants*.

The French Minister of Postal Telegraphy in France has established at the central office a special course of lectures on Wheatstone's automatic apparatus, to which sixteen competent operators, from different parts of the country, have been admitted. The course of lectures and experiments has lasted two months. The pupils are now passing an examination, and a special certificate will be issued to the successful candidates, which will greatly help them in their future promotion in the postal telegraphic service.

The Parc Montceau, placed in one of the most fashionable parts of Paris is now lighted by Jablochkoff candles with success.

Admiral Mouchez has issued his invitation for the Soirées de l'Observatoire, at which as usual will be exhibited all the scientific novelties of the year.

M. Chevreul has been unanimously nominated once more President of the French Société Nationale d'Agriculture.

It is expected that the French Government will take in hand the celebration of the centenary of the discovery of balloons. The two committees which had been formed by several aeronautical societies have been amalgamated, and M. Gaston Tissandier has been appointed president. The scheme of an international exhibition for balloons and instruments used in aërial investigations has been adopted by M. Herrisson, the Minister of Public Works, and will be carried into effect by M. Armengaud Jeane, the well-known civil engineer.

In his speech on laying down his office, previous to being admitted Vice-Chancellor for the year 1883, Dr. Porter, Master of Peterhouse, Cambridge, referred to the endowments of the new Professorships of Physiology and Pathology, increased grants to the museums and lecture-rooms, and a chemical laboratory on an adequate scale, as among the more urgent claims on the new funds available to the University.

Prof. Frisby writes from the U.S. Naval Observatory, Washington, that in the circular he lately sent (Nature, vol. xxvii. p. 226), giving elliptic orbit of great comet, $\phi = 89° 7' 42''\cdot70$ should be $\phi = 89° 13' 42''\cdot70$.

The additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus* ♂) from India, presented by Mr. C. James; a Common Otter (*Lutra vulgaris*), British, presented by Mr. E. P. Squarey; a Black-necked Hare (*Lepus nigricollis* ♂) from Ceylon, presented by Mr. W. Bowden Smith; an Indian Antelope (*Antilope cervicapra*) from India, presented by Capt. R. Brooke Hunt; a Bohor Antelope (*Cervicapra bohor* ♀) from India, presented by Mr. W. J. Evelyn; a Black-backed Jackal (*Canis mesomelas*) from South Africa, presented by Mr. J. S. Crow; a Larger Hill Mynah (*Gracula intermedia*) from India, presented by Mrs. M. R. Manuel; three Passenger Pigeons (*Ectopistes migratorius*) from North America, presented by Mr. F. J. Thompson; a —— Horned Lizard (*Phrynosoma* ——) from California, presented by Mr. Martin R. de Selincourt; a Common Adder (*Vipera berus*), British, presented by Mr. J. Harris; an Indian Black Cuckoo (*Eudynamys orientalis*) from India, purchased; an Axis Deer (*Cervus axis* ♂), born in the Gardens.

APPROXIMATIVE PHOTOMETRIC MEASUREMENTS OF SUN, MOON, CLOUDY SKY, AND ELECTRIC AND OTHER ARTIFICIAL LIGHTS[1]

Sir William Thomson pointed out that the light and heat perceived in the radiations from hot bodies were but the different modes in which the energy of vibration induced by the heat was conveyed to our consciousness. A hot kettle, red-hot iron; incandescent iron, platinum, or carbon, the incandescence in the electric arc, all radiate energy in the same manner, and according as it is perceived through the sense of sight, by its organ the eye, or by the sense of heat,[2] we speak of it as light or heat. When the period of vibration is longer than one four-hundred-million-millionth of a second, the radiation can only be perceived by the sense of heat; when the period of vibration is shorter than one four-hundred-million-millionth of a second, and longer than one eight-hundred-million-millionth of a second, the radiation is perceived as light, by the eye.

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[1] Abstract of lecture at the Glasgow Philosophical Society, by Sir William Thomson, F.R.S.

[2] Sometimes wrongly called the sense of touch. The true list of the senses, first given, I believe, by Dr. Thos. Reid, makes two of what used to be called the sense of touch, so that, instead of the still too common wrong-reckoning of five senses, we have six, as follows:—

Sense of Force.
" Heat.
" Sound.
" Light.
" Taste.
" Smell.

Pouillet, from a series of experiments, deduced a value of the energy radiated by the sun, equal in British units to about 86 foot-pounds per second per square foot at the earth's surface, or about 1 horse-power to every 6½ square feet of the earth's surface. We may estimate from this the value of the solar radiation at the surface of the sun. The sun is merely an incandescent molten mass losing heat by radiation, and surrounded by an atmosphere of incandescent vapour, so that the radiant energy really comes out from any square foot or square mile of the sun's surface, as from a pit of luminous fluid which we cannot distinguish as either gaseous or liquid. Take, however, instead of the sun, an ideal radiating surface of a solid globe of 440,000 miles radius. The distance of the earth being taken as 93 million miles, the radius of the sun is equal to, say in round numbers, one two-hundredth of the earth's distance, hence the area at the earth's distance corresponding to one square foot of the sun's surface, is equal to 40,000 square feet. The radiation on this surface is (40,000 × 86, or) 3,440,000 foot-pounds, which is therefore the amount of radiation from each square foot of the sun's surface. This amounts to about 7000 horse-power, which, according to our brain-wasting British measure, we must divide by 144, if we wish to know the radiation per square inch of the sun's surface, which we thus find to be 50 horse-power.

The normal current through a Swan lamp giving a 20-candle light is equal to 1.4 amperes with a potential of 40 to 45 volts. Hence the activity of the electric working in the filament is 61·6 ampere-volts or Watts (according to Dr. Siemens' happy designation of the name of Watt, to represent the unit of activity constituted by the ampere-volt). To reduce this to horse-power we must divide by 746, and we thus find about 1-12th of a horse-power for the electric activity in a Swan lamp. The filament is 3½ inches long, and ·01 of an inch in diameter of circular section; the area of the surface is thus 1-9th of a square inch, and therefore the activity is at the rate of 3-4ths of a horse-power per square inch. Hence the activity of the sun's radiation is about sixty-seven times greater than that of a Swan lamp per equal area, when incandesced to 240 candles per horse-power.

In this country the standard light to which photometric measurements are referred is that obtained from what is known as a standard candle. Latterly, however, objections have been raised against its accuracy. It has been said that differences of as much as 14 per cent. have been found in the intensity of the light given by different standard candles, and that serious differences have been observed in the intensity of the light from different parts of the same candle in the course of its burning. The Carcel lamp, the standard in use in France, has been regarded as the only reliable standard. It is, no doubt, very reliable and accurate in its indications, but it should be remembered that its accuracy is greatly owing to the careful method and the laborious precautions taken to secure accuracy. If something akin to the precautions applied to the Carcel lamp by Regnault and Dumas were applied to the production and use of the standard candle, there is little doubt but that sufficient accuracy for most practical purposes could also be obtained with it; probably as good results as are already obtained by the use of the Carcel lamp.

At the Conference on Electrical Units which met in Paris lately, a suggestion was made to use as a standard for photometric measurements the incandescence of melting platinum, and very interesting results and methods in connection with the proposal were presented to the meeting. According to experiments by Mr. Violle, which M. Dumas reported to the Conference, a square centimetre of liquid platinum at the melting temperature gives of yellow light seven, and of violet twelve times the quantities of the same colours given by a Carcel lamp. The apparent area of the Swan filament, being one-ninth of a square inch, is ·23 of a square centimetre, and when incandesced to 20 candles must be about as bright as the melted platinum of Mr. Violle's experiment, as the 7 carcels of yellow and 12 of violet must correspond to something like 10 carcels or 85 candles, in the ordinary estimation of illumination by our eyes. The tint of Mr. Violle's glowing platinum cannot be very different from that of the ordinary Swan lamp incandesced to its "20-candles." Thus both, as to tint, and brightness, it appears that melted platinum at its freezing temperature is nearly the same as a carbon filament in vacuum incandesced to 240 candles per horse-power.

For approximative photometric measurements the most convenient method is certainly that of Rumford, by a comparison of the shadows cast by the sources of light on a white surface. The apparatus necessary are only a piece of white paper, a small cylindrical body, such as a pencil, and a means of measuring distances. Ordinary healthy eyes are usually quite consistent in estimating the strength of shadows, even when the shadows examined are of different colours, and with a reasonable amount of care photometric measurements by this method may be obtained within 2 or 3 per cent. of accuracy. The difference in the colours of the shadows is of course due to each shadow being illuminated by the other light.

Arago has compared the luminous intensity of the sun with that of a candle, and estimates it as equal to about 15,000 times that of a candle flame.

Seidel, as Sir W. Thomson had been informed by Helmholtz, estimated the luminous intensity of the moon as about equal to that of grayish basalt or sandstone. An experiment on sunlight made in Glasgow on the 8th of this month (since this paper was read), compared with an observation on moonlight, which he made at York during the meeting of the British Association there in 1881, had led him to conclude that the surface of the moon radiates something not enormously different from one-quarter of the light incident upon it. It would be exactly this if the transparency of the Glasgow noon atmosphere of December 8, 1882, had been exactly equal to that of the York midnight atmosphere of September, 1881, referred to below, for the respective altitudes of the sun and moon on the two occasions. The observation on moonlight referred to above showed the moonlight at the time and place of the observation (at York early in September, 1881, about midnight, near the time of full moon) to be equal to that of a candle at a distance of 230 centimetres. The moon's distance ($3·8 \times 10^{10}$ cm.) is $1·65 \times 10^8$ times the distance of the candle. Hence, ignoring for a moment the loss of moonlight in transmission through the earth's atmosphere, we find $(1·65 \times 10^8)^2$, or 27 thousand million million as the number of candles that must be spread over the moon's earthward hemisphere painted black, to send us as much light as we receive from her. Probably about one and a half times as many candles, or say forty thousand million million would be required, because the absorption by the earth's atmosphere may have stopped about one-third of the light from reaching the place where the observation was made. The moon's diameter is $3·5 \times 10^8$ centimetres, and therefore half the area of her surface is 19×10^{16} square centimetres, which is nearly five times forty thousand million million. Thus it appears that if the hemisphere of the moon facing the earth were painted black and covered with candles standing packed in square order touching one another (being say one candle to every five square centimetres of surface), all burning normally, the light received at the earth would be about the same in quantity as estimated by our eyes, as it really is. It would have very much the same tint and general appearance as an ordinary theatrical moon, except that it would be brightest at the rim and continuously less bright from the rim to the centre of the circle where the brightness would be least.

The luminous intensity of a cloudy sky he found about 10 a.m. one day in York during the meeting of the British Association to be such that light from it through an aperture of one square inch area was equal to about one candle. The colour of its shadow compared with that from a candle was as deep buff yellow to azure blue, the former shadow being illuminated by the candle alone, the latter by the light coming through the inch hole in the window shutter.

The experiment on sunlight of last Friday (December 8) showed, at 1 o'clock on that day, the sunlight reaching his house in the University to be of such brilliancy that the amount of it coming through a pinhole in a piece of paper of ·09 of a centimetre diameter produced an illumination equal to that of 126 candles. This is 6·3 times the 20-candle Swan light, of which the apparent area of incandescent surface is ·23 of a square centimetre, or 3·8 times the area of the pin-hole. Hence the sun's surface as seen through the atmosphere at the time and place of observation was 24 times as bright as the Swan carbon when incandesced to 240 candles per horse-power. By cutting a piece of paper of such shape and size as just to eclipse the flame of the candle and measuring the area of the piece of paper, he found about 2·7 sq. centims. as the corresponding area of the flame. This is 420 times the area of the pin-hole, and therefore the intensity of the light from the sun's disc was equal to

(126 × 420) about 53,000 times that of a candle-flame. This is more than three times the value found by Arago for the intensity of the light from the sun's disc as compared with that from a candle-flame; so much for a Glasgow December sun!

The ·09 cm. diameter of the pin-hole, of the Glasgow observation, subtends, at 230 centimetres distance, an angle of 1/2556 of a radian; which is 23·7 times the sun's diameter (1/108 of a radian). But at 230 cm. distance the sunlight through the pin-hole amounted to 126 times the York moonlight (which was 1 candle at 230 cm. distance). Hence the Glasgow sunlight was [$(23{\cdot}7)^2 \times 126$ times or] 71,000 times the York moonlight. We cannot therefore be *very far* wrong in estimating the light of full moon as about one-seventy-thousandth of the sunlight, anywhere on the earth. This, however, is a comparison which, because of the probably close agreement of the tints of the two lights, can probably be made with minute accuracy: and we must therefore not be satisfied with so very rough an approximation to the ratio as this 70,000. A lime light, or magnesium light, or electric arc-light, carefully made and remade with very exactly equal brilliance, for each separate observation of sunlight and moonlight, might be used for intermediary.

THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION[1]

In our days the student of the biological sciences may look forward towards his life-task with sincere gratitude. Gratitude not only for what has already been achieved, and for the ends that have been attained in this domain, but more especially for all that which the future promises, since the sage whose mortal remains were lately deposited in Westminster Abbey has thrown the light of his genius over regions which hitherto were shrouded in deepest obscurity and has opened new vistas on old problems, of which man has been seeking the solution for many thousands of years.

It is to him we have to give thanks that the dawn of a new life has commenced for those sciences; to him, moreover, we owe it that the twilight has only lasted a short time, and that the full light of day has shone so soon upon an extensive field. And if by this light we perceive numerous new problems, the existence of which was not even dreamt of before, and which cover the field of our work as far as the horizon reaches, still we notice that their shapes have obtained definite outlines. In future they may serve as milestones on our way onwards, before, when we were still groping in the dark, they were as many stumbling-blocks which prevented us from advancing.

If to-day I call before your mind the image of this great reformer, it is not to give you an eulogy of Darwin, whose sudden death some months ago has filled with grief the whole civilised world. He is before my mind, because I belong to the generation whose youth coincides with that of the "Origin of Species;" a generation deeply filled with gratitude towards this great master. A gratitude bursting forth with doubled intensity in him who enters upon a career in which he will have ample opportunity to continue work in that field of science to which he has become more and more attached through the inspiring influence of Darwin.

It is not only by the contents of his work that Darwin takes hold of us, it is also his personal character which leaves such a forcible impression. The history of his life, his method of work, his amiable individuality, have excited our enthusiasm over and again, and always in an increasing measure. Similar to other grand figures in the history of the world, who by their life and their example have perhaps wrought more than by their teaching—which at the hands of less eminent adepts soon took a dogmatic, *i.e.* a degenerate shape—this reformer of biological science has left behind him a remembrance which will be kept and transmitted by his followers with quite as much care and piety as the writings he has left.

What strikes us most and all at first in everything emanating from him is his passionate honesty,[2] which has already become proverbial. Never did he pass over in silence, in the interest of his argument, a point which might eventually appear to be in favour of the opposite plea. In the enumeration and refutation of such points he was always quite as careful as in the collection of positive proofs. He was never biassed, unless biassed in the good sense of the term, *i.e.* enabled, when once he was of opinion that it was necessary to choose a decided side with respect to any dubious point, to devote to the careful consideration of this point not only hours, but if necessary months and years of his life,—months and years of daily returning observations concerning what appeared to be unimportant facts, which, however, when they were afterwards brought together, permitted him to draw highly important conclusions.

Unlimited veracity and undaunted patience, two principal requirements of the true naturalist, thus found their most perfect incarnation in Darwin, and with these two for his guides, he brought together, from far and near, building stones for the completion of the grand structure which his mind had conceived. The quarries from whence he excavated those building stones were very different from those to which the scribes in biological science habitually resorted. It must be understood that since the appearance of Cuvier's "Le Règne Animal distribué d'après son Organisation," a reaction had sprung up against descriptive zoology which in many cases went further than Cuvier himself would ever have acknowledged. The numerous volumes of his excellent "Histoire naturelle des Poissons" furnish ample proof that Cuvier had always endeavoured to combine careful description of the species and conscientious sifting of all the material concerning its life history, its geographical distribution, and its synonymy with the study of the comparative anatomy of the group to which it belonged. Several of his followers have, however, concluded that since researches upon the internal organisation of so many classes of animals allowed him to make most important deductions, it was from similar researches only that anything could be expected for the future. Their ambitious aspirations could not manage to forget that a combined investigation by Cuvier and Geoffroy St. Hilaire was once described by one of the two in the following words:—"Nous ne déjeûnions jamais sans avoir fait une découverte."

And so a period was opened up in which our knowledge of the internal organisation of animals was not only increased on all sides and firmly based upon facts by zealous workers, but in which this knowledge was gradually pushed into the foreground as the pre-eminent, as the only true zoology. The careful study of the species and its life history was left with a smile and a shrug of the shoulders to dilettanti and museum zoologists. In order further, to indicate how the results of researches of these men were looked upon as popular and unimportant, this new school invented the well-sounding name of "scientific zoology."

The eminent researches of von Siebold on parthenogenesis and on the freshwater fishes of Germany; Kolliker's important monograph of the Pennatulids, &c., show that even its founders were subject to impulses which drove them back into this very field, or rather that it was not they, but their less gifted followers from whom the contemptuous meaning which that combination of words gradually attained has emanated.

Thus for a certain lapse of time the wind blew from a different quarter, and attempts have repeatedly been made to call into life classifications which were based upon certain points in the internal organisation, points which were considered to be of the more importance the less they were visible. Fortunately the great masters to whom we owe comparative anatomy, and who have made it such as we know it in the present day, have not joined in this movement. Johannes Muller's "System der Plagiostomen" stands side by side with his "Comparative Anatomy of the Myxinoids," showing that this one-sided exaggeration would never have been encouraged by himself. Gegenbaur, Huxley, &c., have similarly kept aloof from the "scientific zoologists" in the stricter sense, whose narrow-minded doctrines are still pullulating, be it in a somewhat modified form. At the present day it is not so much the internal organisation which forms the shibboleth by which entrance is obtained to the holy circle of self-styled orthodox zoologists, but now it is the history of development, embryology, that gives the pass-word. This important branch of biological science has made gigantic strides of late; it counted in its foremost ranks, among the most promising and large-minded, the man whom a cruel fate had doomed to find his death in the Alps of Switzerland, the talented Balfour. He never overvalued in a petty way the labours of the select batallion of which he was one of the leaders. In the rear of this army, however, voices are heard claiming infallibility for embryology, and the splendid generalisation: "the development of the individual is a repetition on a reduced scale of the development of the race," must often serve to hide unripe attempts at classifications deduced from the developmental stages of eggs and larvæ of questionable origin, and applied to groups of animals of which the adventurous embryologist would certainly not be able to distinguish the different members specifically.

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[1] By Prof. A. A. W. Hubrecht. Inaugural Address delivered in the University of Utrecht. September, 1882.

[2] Cf. Huxley, NATURE, May, 1882.

But enough of this distressing partiality, knowing that we find a complete reaction against it, in Darwin's word and example, which will be our strongest antidote against similar influences. We are thus carried back to our starting-point, where it was observed that the value of the sources from whence Darwin has drawn so much valuable information, was scarcely recognised up to his time. He entered into connection with cattle-rearers and bird-fanciers, and gladly availed himself of the remarks of trustworthy observers who were acquainted with animals and plants in their daily life, even if they had always remained outside the pale of science.

And what far-reaching results may be obtained by careful study of the habits and life-history of animals is shown by the last volume which we owe to Darwin's hand. Here it is apparent, upon almost every page, that from conscientious observations on the habits of an animal so common as the earthworm, conclusions follow which furnish us with new and quite unexpected views about the formation and the changes of a large area of the earth's surface.

The most striking example of Darwin's all-embracing genius is obtained when his Monograph of the Cirripedia is compared with the chapters in which he enunciates and discusses his hypothesis of pangenesis. The one, the most scrupulous study of details, the comparison of slight differences both between individuals of the same species and between specifically distinct specimens; the evaluation of these distinctive characters one against the other; in one word, pure systematic zoology with all its appurtenance of patience, scrupulousness, and nearly painful conscientiousness. The other—one of the most daring hypotheses which the human understanding has ever wrought, upon which only a very limited number of observed facts can be brought to bear. A hypothesis which boldly penetrates into the most hidden secrets of organic nature; which brings the marvellous effects of heredity on a level with the reproduction of lost parts, yea even with the healing of wounds. A hypothesis which no longer looks upon the cells as the morphological units of the living organism, but which postulates the existence of a continual flow of separate minute gemmulæ, feeding and reproducing themselves, and being derived from all the cells and all the tissues in all the consecutive periods of their existence. These gemmulæ, in the individual being we have before us, circulate along paths which remain wholly unknown to us, and finally reunite in millions in every ovum, in every spermatozoön, in every bud, and in every pollen-grain.

The laws by which these inscrutinable processes are governed, do not lose anything of their mysteriousness when we glance at the disparate and incomprehensible phenomena which they have to explain: atavism, in which heredity takes a sudden leap backwards into the grey mists of the past; the transmission to the child of the effects of an increased or decreased use of a limb by the parents: the reproduction of a lost limb or tail; the growth of an entire plant out of a severed portion of a leaf; the change which pollen and sperma may occasionally call forth not only in the ovules but also in the tissues of the mother-form; the hybridisation in the vegetable kingdom by the union of the cellular tissue of two plants independently of the organs of generation; the appearance of a complex metamorphosis in the course of the development of certain animal forms, the nearest allies of which are entirely devoid of anything like it; &c.

Nevertheless this hypothesis was put forward by the very same Darwin whom we have to thank for the monograph of the Cirripedia. It is clear that the frame of mind required for completing the one is widely different from that in which he enunciated the other. There is, however, a common link uniting the two. In the specific description of the Cirripeds we find him ever and again in collision with the opinion then generally accepted of the definite boundaries limiting the species, and thus this work cannot have remained without influence on the later development of his ideas. On the other hand, he looked upon the hypothesis of pangenesis as a necessary sequel, to a certain extent as "le couronnement de l'édifice" of his theory of evolution by means of natural selection.

We have not here to enter into a discussion concerning the hypothesis of pangenesis, nor to inquire into the different attacks to which it has already been exposed. I must, however, observe that with it Darwin has entered the domain of physiology, a field upon which all the questions into which the great problem of evolution may be subdivided, as heredity, influence of use and disuse of organs, adaptation to modified circumstances, must find their solution.

Whereas the physiology of man and the higher animals is developing and growing with rapidity, and what has been thought and wrought in Utrecht has largely influenced this development, Comparative Physiology, which has to track all the different problems just mentioned all through the animal kingdom down to their simplest form in the lowest organised beings, is only in its infancy. And yet this branch of science will shortly come abreast of morphology further to secure the basis of the theory of evolution and to contribute to its harmonious development. It was not by mere chance that the legislature specially mentions Comparative Physiology as a branch of science which will have to be cultivated and taught by him who is called to the chair I am about to occupy.

Although the greater part of this territory is still wrapt in obscurity, still it is at the University of Utrecht that the prospects for Comparative Physiology are promising in the highest degree, be it by the efforts of others than the legislature had in view. It must for certain be acknowledged that researches concerning the phenomena of life in the very smallest organisms, investigating their reaction towards light and oxygen, and even penetrating into the effects of hunger and thirst as manifested by these lowly-organised beings, eminently belong to the domain of Comparative Physiology. The vicinity of a laboratory in which such excellent results have already been obtained is a strong stimulus for us all towards further labour in this field.

Venturing to-day along that road, I may hope to claim your attention, because in so doing, I wish to make an attempt to weaken one of the chief arguments against the theory of evolution, an argument which was termed by Huxley "the stock objection."[1]

I wish to speak to you about the hypothesis of accelerated development by primogeniture and its place in the theory of evolution.

I must begin with calling to mind that provisionally it is not upon the firm basis of proved facts, but more upon the quicksands of theoretical conjecture that we shall be moving. Our track first leads us into the domain of a science which is of such an exceptional value for the theory of evolution, because this science only, the science of palæontology, can furnish us with direct evidence towards the truth of that theory.

If, indeed, living organisms form one continuous chain with those that have already become extinct; if these organisms have not been called into life in successive periods by repeated creative acts but if they are in direct blood-relationship to each other—a relation which as we penetrate further into the past must be accompanied by a simplification of organisation—then palæontology must furnish us with the evidence of this process. Then, indeed, the superposed strata which have been deposited since the cooling of the earth's crust under the combined influence of internal vulcanism and external atmospherical influences, must contain the archives in which the most trustworthy and direct proof for the validity of the theory of evolution are to be found. Moreover, the material which we find heaped in these archives must show—if we place confidence in it—that gradual increase of complication which accompanies the development of the more highly differentiated forms out of simpler types by the aid of natural selection, in a succession exactly corresponding to that of the deposition of the strata. We know how far palæontology had advanced in 1859, we understand how it was that Darwin insisted on the imperfection of the geological record in the first edition of his "Origin of Species." He diligently collected arguments to explain this incompleteness and to oppose the objection against his doctrines which it might furnish. I cannot at present enter into details concerning this refutation. Still it is quite as valid to-day. So many deposits are wholly devoid of animal remains, it is so obvious that of other animal forms, fossils can hardly ever have been formed, and lastly, only such a small portion of the earth's surface has been adequately searched, that we have indeed more reason to be astonished at the quantity of facts that have already come to our knowledge, than at the much larger quantity which yet remains hidden from our view.

This is especially present to our minds when we remember the invaluable deposits that have of late years been opened up in North America, where not only the successive periods of the tertiary epoch form extensive deposits, but where they moreover contain perfectly preserved animal specimens which have lived in these successive periods, and which indeed show in the most irrefutable way that a direct connection accompanied by an increase of differentiation undeniably exists. We here find a very remarkable page of the book thrown open upon which nature has written down for us the history of the development of the horse, and whoever has learnt to read this handwriting is brought to the inevitable conclusion: this development has started from an older form of a less specialised organisation, and has proceeded along successive steps which are entirely in accordance with the theory of evolution.

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[1] T. H. Huxley, American Addresses.

Similarly the numerous remains of the fossil group of the Ornithoscelidæ are only known since a recent date, and a gradually increasing knowledge is thus attained of those interesting animals which link together reptiles and birds, two classes of animals which were formerly looked upon as amongst the most thoroughly separated.

Together with these irrefutable proofs that evolution has indeed taken place, starting from the simpler, more generalised types, and tending towards the more complicated and more specialised forms, palæontology acquaints us with certain other facts. I allude to the persistence of the same form, of the same genus, sometimes even of the same species in all successive strata and periods. Thus, for example, among Mollusks, Chiton and Pleurotomaria have persisted from the Silurian down to the present period; Dentalium from the Devonian; Pinna and Cyprina from the Carboniferous period. Amongst the Foraminifera certain genera occur in the Carboniferous epoch, which at the same time are members of the living fauna. Amongst Brachiopods our living Lingulas, Rhynchonellas, and Terebratulas are very ancient types; representatives of the osseous fishes lived in the Cretaceous period, which cannot be generically distinguished from their living relatives, whilst certain genera of cartilaginous fishes reach even into a much farther distant past.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Oxford.—Owing to the early occurrence of Easter, the term has begun a week earlier than usual this year. The first University business of importance will be the constitution of the New Boards of Faculties. These will consist of the professors as *ex-officio* members, and of members elected by the College lecturers in the various faculties, the number to be so elected being first decided by vote. The first step towards the new state of things has been the appointment of Mr. Lockhart, of Hertford College, as General Secretary to the Board of Faculties.

In the department of Physics at the University Museum, Prof. Clifton continues his course on the Electricity Developed by the Contact of different Substances; Mr. Stocker lectures on Mechanics, and Mr. Heaton on Problems on Mechanics and Physics. Prof. Price continues his course on Optics, and also gives a course on Hydro-mechanics.

Prof. Pritchard is absent in Egypt completing his measurements of the magnitude of the stars. The observatory will be open on Tuesday and Thursday evenings under the charge of Mr. Plummer.

In the Chemical Department of the Museum, Prof. Odling will give a course on Elementary Facts and Doctrines. Mr. Fisher will lecture on Inorganic Chemistry, and Dr. Watts on Organic Chemistry. The laboratory will lose the services of Mr. F. D. Brown in the middle of the term, as he has been elected to the Professorship of Chemistry and Physics in the New University at Auckland, and leaves for New Zealand in March.

In the Biological Department Prof. Moseley continues his course on Comparative Anatomy (followed by practical work). Mr. Hatchett Jackson lectures on the Fundamental Principles of Embryology, Mr. Poulton on the Geographical Distribution of Animals, Mr. Lewis Morgan on the Teeth of Vertebrata and on Human Osteology, and Mr. Hickson on Histology. Mr. Barclay Thompson has been obliged to give up lecturing on account of ill health.

Prof. Prestwich gives a course of lectures on Stratigraphical Geology.

The following courses will be given in the private College laboratories:—At Christchurch Mr. Vernon Harcourt lectures and gives practical instruction in Quantitative Analysis, and Mr. Baynes on Thermo-dynamics. At Balliol Mr. Dixon lectures on Organic Chemistry, and Elementary Electricity. At Magdalen Mr. Yule gives a course of demonstrations on the Chemical and Physical Properties of the Blood, Circulation, Respiration, &c.; and Mr. Chapman gives a practical course on Elementary Vegetable Morphology.

A scholarship in Natural Science will be offered at Keble College of the value of 80*l.* per annum. The examination will be in Biology and Chemistry; a scholarship will also be offered for competition at Queen's College in Physics, Chemistry, or Biology.

Cambridge.—Mr. J. E. Marr, M.A., Fellow of St. Johns College, is the Sedgwick Prizeman this year.

Science lectures commence on the following days: Prof. Liveing, General Principles of Chemistry, January 23; Prof. Dewar, Organic Chemistry, January 23; Prof. Newton, Geographical Distribution of Vertebrate Animals, January 31; Mr. Caldwell, Morphology of Invertebrata, Advanced, February 1; Dr. Hans Gadow, Morphology of Vertebrata, Advanced, January 30.

The names of Messrs. Casey (Trin.), Harvey (King's), A. R. Johnson (St. John's), Turner (Trin.), and Welsh (Jesus) appear in alphabetical order in the First Division of the List for the Third Part of the Mathematical Tripos, to which only the Wranglers were admitted. One name is in the second division, and eight in the third.

Mr. F. J. M. Page, B.Sc., F.C.S., of University College, London, was elected, on January 11, Lecturer on Physics at the London Hospital Medical College.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, December, 1882.—An improved dynamometer, by W. P. Tatham.—The isochronal Worthington pumping engine, by J. K. Maxwell.—Explosive and dangerous dusts, by T. W. Tobin.—Economical steam power (continued), by W. B. Le Van.—The universality of vibrations, by C. C. Haskins.—Report on European sewerage systems, &c. (continued), by R. Hering.

Annalen der Physik und Chemie, No 13 (December 2, 1882).—Absolute measurements with bifilar suspension, and especially two methods for determining the horizontal intensity of terrestrial magnetism without time measurement, by F. Kohlrausch.—The reduction of the Siemens unit to absolute measure, by E. Dorn.—On electric vibrations with special regard to their phases, by A. Oberbeck.—Experimental researches on galvanic polarisation, by F. Streintz.—On M. A. Guebhard's representation of equipotential curves, by E. Mach.—The electromotive force of the Daniell element, by E. Kittler.—On amalgamation-currents, by H. Haga.—Explanation of electric shadows in free air, by P. Riess.—On the material parts in electric sparks, by F. Wachter.—On the magnetic screening action of iron, by J. Stefan.—On tone-vibrations of solid bodies in presence of liquids, by F. Auerbach.—A small alteration of the pyknometer, by E. Wiedemann.—Remark on Herr Galn's memoir on the density of the luminiferous ether, by the same.—On the true cohesion of liquids, by the same.—On the condensation of liquids on solid bodies, by the same.—The leucoscope and some observations made with it, by A. König.—Contribution to the theory of diffraction in telescope-tubes, by H. Struve.—On the elliptical polarisation of reflected diffracted light, by W. Kœnig.—On the Poggendorff fall-machine, by K. L. Bauer.—Contributions to the history of natural sciences among the Arabs, viii. and ix., by E. Wiedemann.

Bulletin de l'Academie Royale des Sciences de Belgique, Nos. 9 and 10.—Notice on a peculiarity in the aurora borealis of October 2, 1882, and on the increase in intensity of scintillation of stars during auroræ, by C. Montigny.—Some theorems of elementary geometry, by E. Catalan.—On curves of the third order, by C. Le Paige.—Aspect of the great comet of 1882 (Cruls) observed at Louvain, by F. Terby.—Note on the aurora borealis of October 2, 1882, by the same.—Action of chlorine on tertiary butylic chloride, by Baron d'Otreppe de Buvette.

No. 11.—Note on some bones of the Biscay whale at the

Museum of La Rochelle, by P. J. Van Beneden.—On some uniform geometric transformations, by C. Le Paige.—Second notice on the comet, by F. Terby.—On the functions of M. Prym and M. Hermite, by A. Genocchi.—On glycogen in Mucorineæ, by L. Errera.

Journal de Physique, December, 1882.—Remarks on timbre, by M. Kœnig.—Remarks on the critical state, by M. Stolatow.—Experimental study of the reflection of actinic rays; influence of specular polish, by M. de Chardonnet.—Note on the theory of the Laurent saccharimeter with white light, by M. Dufet.—Notes of science in *Il Nuovo Cimento* and the *Journal* of the Russian Physico-Chemical Society.

Rivista Scientifico-Industriale e Giornale del Naturalista, October 31, and November 15 and 30, 1882.—Inconveniences of the usual pluviometer, and a few words about the pluvio-pulverometer, an apparatus for rain, dew, and atmospheric dust, by P. Lancetta.—Non-sensitive mercury thermometer; demonstration of the principle of the telephone, by G. Govi.—A doe with hairy horns.—Review of a prize memoir by G. Poloni, on the permanent magnetism of steel at different temperatures, by A. W.—Double-action mercury air-pump, by G. Serraville.—Fundamental principle of electrostatics, by S. Mugna.—Male genital armatures of saltatory Orthoptera, by A. Tozzetti.—Brief notice of the fluoriferous volcanoes of Campania, by A. Scacchi.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xxv, fasc. xviii.—On a recent landslip near Belluno, by T. Taramelli.—On drunkenness in Milan, II., by A. Verga.—Jacobi's theorem regarding periodicity, and the illegitimacy of a part of the consequnces that have been deduced from it, by F. Casorati.

Nouveaux Mémoires de la Société Helvetique des Sciences Naturelles, vol. xxviii., 2nd part.—The Diluvium round Paris and its position in the Pleistocene, by A. Rothpletz.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, January 11.—Prof. Henrici, F.R.S., president, in the chair.—Messrs. H. T. Gerrans and W. L. Mollison were elected members.—Dr. Hirst, F.R.S., spoke on the resolution of congruences into systems of quadric reguli; Mr. Glaisher, F.R.S., discussed the solution of a differential equation allied to Riccati's; and Mr. Tucker communicated a paper by Prof. Cayley, F.R.S., on the automorphic transformation of a binary cubic function.

Zoological Society, December 19, 1882.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—Mr. Sclater exhibited some photographs of a new Zebra, from Shoa, lately named *Equus grevyi*, by M. A. Milne-Edwards, F.M.Z.S., which had been sent to him by that gentleman, and pointed out the difference which separated this animal from the nearly allied *E. zebra*.—The Rev. H. H. Slater, F.Z.S., exhibited and made remarks on the skin of a Shrike (*Lanius* sp. inc.) which had been obtained near Spurn Point, Yorkshire.—The Secretary exhibited, on behalf of Lord Lilford, the skin of a young male *Emberiza rustica*, which had been taken at Elstree Reservoir on November 19 last. Only one other example of this bird had hitherto been recorded as having been met with in Great Britain.—Dr. Günther exhibited, on behalf of Sir Campbell Orde, Bart., a specimen of a Charr (*Salmo alpinus*), obtained in a loch in North Uist, being the first example ever obtained in this loch.—Mr. P. H. Carpenter exhibited and made remarks on some microscopical preparations of *Antedon eschrichti*, in which a nervous plexus derived from the fibrillar envelope of the chambered organ was visible at the sides of the ambulacra of the disk.—Prof. Flower exhibited a photograph (presented to the Society by Mr. James Farmer, F.Z.S.) of Seal Point, Farallone Islands, off the coast of California, showing the immense number of Seals (*Otaria gillespii*, M'Bain) frequenting that locality.—Prof. Flower read a paper on the whales of the genus *Hyperoodon*, in which he pointed out that one of the most important points in the history of these animals yet unsolved was whether the large-headed form, with great development of the maxillary crests, called by Dr. J. E. Gray *Lagenocetus latifrons*, was a distinct species, or whether, as suspected by Eschricht, it was the adult male of the common form known as *Hyperoodon rostratus*. The author had asked Capt. David Gray to avail himself of his exceptionally favourable opportunities of observing these animals in their native haunts, to solve this question, with the result shown in the next communication.—A communication was read from Capt. David Gray, ss. *Eclipse*, called "Notes on the Characters and Habits of the Bottlenose Whale (*Hyperodoon*)," in which it was stated that he had killed 203 of these animals last season, and had traced in the males every gradation of development between the two forms, and had therefore conclusively proved that *Hyperoodon* or *Lagenocetus latifrons* had no existence as a distinct species. The communication was illustrated by sketches and photographs, showing the external characters and cranium in various stages of growth.—Mr. P. H. Carpenter read a paper on the classification of the *Comatulæ*. He criticised the method of formulation recently proposed by Prof. F. J. Bell, and pointed out its disadvantages for the purposes of classification, owing to its being inapplicable to those *Comatulæ* which have irregular arm-divisions. He explained his own system of formulation and classification, and stated that he believed it to be capable of dealing with all possible variations of *Comatula* structure.—Mr. F. Day read a paper on the identity of *Arnoglossus lophotes*, Gthr., with *Pleuronectes grohmanni*, Bonap. A second paper by Mr. Day contained remarks on some hybrids between Salmon and Trout.—A paper by Messrs. Godman and Salvin was read, describing some Butterflies from New Ireland, received from the Rev. G. Brown and Mr. E. L. Layard. Among these were examples of two new species, named respectively *Prothoe layardi* and *Danaius adustus*.—Mr. Oldfield Thomas read a paper containing descriptions of two new species of Fruit-Bats of the genus *Pteropus* from the Caroline Islands. The author proposed to call them *Pteropus phæocephalus* and *Pt. breviceps*.—A communication was read from Major G. F. L. Marshall, F.Z.S., containing some notes on Asiatic Butterflies. A species of *Amecera* was mentioned as new to the Beluchistan fauna, and three species were described as new to science.—Mr. G. A. Boulenger read the description of a new species of Lizard from Dacotah, based upon some specimens lately presented to the Society's collection by Mr. S. Garman, of the Museum of Comparative Zoology, Cambridge, Mass., and proposed to name it *Sceloporus garmani*.—Mr. Arthur G. Butler read a paper in which he gave an account of a collection of Spiders made by the Rev. Deans Cowan in Madagascar. In addition to many intesting and singular forms were specimens of the curious tailed species *Arachnoura scorpionoides* from Central Madagascar. Six new species were described.—Mr. W. N. Parker read a paper on some points in the anatomy of the Indian Tapir.—Mr. Herbert Druce read a paper descriptive of new species of Moths chiefly from Western Africa and New Guinea. Fifteen new species were described, as was also a new genus of *Chalcosiidæ* from New Guinea.

Geological Society, December 20, 1882.—J. W. Hulke, F.R.S., president, in the chair.—Percival Fowler, Alfred Eley Preston, and Robert Blake White, were elected Fellows of the Society.—The following communications were read:—On generic characters in the order Sauropterygia, by Prof. Owen, C.B., F.R.S. After referring to the subdivision of De La Beche's group of Enaliosauria into the orders Ichthyopterygia and Sauropterygia, the author indicated that the latter showed differences in the proportional length of the neck and the number and form of its vertebræ bearing relation to the size of the head, together with modifications of the teeth, of the sterno-coraco-scapular frame and of the paddle-bones, leading to the formation of two genera, namely, *Plesiosaurus*, and *Pliosaurus*, the latter so-called to indicate the nearer approach made by it to a generalised Saurian type. In Crocodilia the crowns of the teeth show a pair of strong enamel ridges, placed on opposite sides of the teeth, and these occur also in *Pliosaurus*; while in *Plesiosaurus* they are not present. *Pliosaurus* further approaches the fresh-water Saurians by the large size of the head and the shortness of the neck.—On the origin of valley-lakes, mainly with reference to the lakes of the Northern Alps, by the Rev. A. Irving, B.A., B.Sc., F.G.S. The author, having given reasons for considering this question, still an open one, proceeded to criticise Prof. Ramsay's theory as it was expounded by him in 1862. The author proceeded to show that the lakes of the Northern Alps are found, as a rule, just among those strata where subsidence would be most likely to occur. In this way it was shown that we are *not shut up*, by Prof. Ramsay's reasoning, to the hypothesis of *glacial excavation*. Further, other agencies than those discussed by Prof. Ramsay may have co-operated to form lakes, such as (*a*) *Alterations in the relative levels* of different parts of a floor of a valley, connected with movements of parts of a mountain-system on a large scale. The effects of (1) lines of flexure crossing older lines of valley-erosion; (2) of lateral thrusts closing in a valley (partly), were here considered. (*b*) *Upthrust* of the more yielding strata (as in the "creeps" of coal-mines) by resolution of forces due to pressure of the mountain-masses at the side of a valley. (*c*) The *dead weight of the huge glaciers* which filled the Alpine valleys, and *crushed in the floor,* in places where extensive underground erosion had gone on in preglacial times. (*d*) The partial *damming up of valleys,* (1) by *diluvial detritus,* (2) by *moraines,* (3) by *Bergstürze* (recently investigated by Prof. Heim of Zurich. (*e*) *Faults.* (*f*) *Chemical solution,* by Alpine waters derived from the melting of the snow, which has undergone long exposure to the atmosphere. It was shown that the very situation of the great majority of the lakes of the Northern Alps is distinctly favourable to the operation of one or more of these agencies. The Königsee was mentioned as a special instance of *subsidence;* the Achensee of a lake lying in a *faulted* line of dislocation; Lake Alleghe and Lake Derborence as lakes formed by Bergsturze during the last century; the prehistoric delta of the Arve as the most conspicuous instance in the Alps of the partial damming-up of a valley by diluvial detritus; the *quondam* Lake of Reutte as an instance connected with violent inversion of strata; and the ancient lakes of the Grödner and Oetz Thals as instances of the action of moraines. The common fact of observation that lakes are more numerous in glaciated than in non-glaciated countries, the author thought, was partly explained by some of the foregoing principles, partly by the better preservation of lake-basins in glaciated countries from silting up and from becoming thus obliterated, while in some glaciated regions lakes are wanting.

Victoria (Philosophical) Institute, January 15.—Prof. Stokes, F.R.S., Lucasian Professor of Mathematics at Cambridge, read a paper on the Absence of Real Opposition between Science and Religion.

EDINBURGH

Royal Society, December 18, 1882.—Mr. Robert Gray, vice-president, in the chair.—Prof. Tait read a paper on the laws of motion, in which an attempt was made to express the fundamental principles of dynamics without introducing the idea of "force." The conservation of energy forms of course the basis. The region of space, in which a given particle is, is mapped out by its equipotential surfaces. Newton's First Law is expressed, then, by saying that the potential of the space is the same from point to point, so that the kinetic energy of a moving particle suffers no change. If the potential varies, then the kinetic energy must vary. As a simple case, consider two regions separated by a plane, the potential function being constant throughout each region. Then the velocity of a particle approaching the plane may (since motion is purely relative) be referred to a point moving parallel to the plane, so as to make the velocity of the particle wholly perpendicular to the plane. It thus appears that the component of the velocity at right angles to the plane only is altered, so that if the direction of motion is originally inclined to the plane, the direction as well as the speed is altered. This, in fact, is the well known problem of refraction according to the corpuscular theory of light; and the principle of least action thus appears under the form of the conservation of velocity at right angles to the direction of greatest potential slope. In expressing Newton's Third Law, Prof. Tait extended the second interpretation as given in the now well known Scholium to include vector as well as scalar quantities.—Mr. George Seton, in a paper on illegitimacy in Scotland, gave a careful analysis of the returns for the last decade, which showed a decrease of ·9 per cent. as compared with the returns of the previous decade. The counties in which the percentage was under the average for the whole country all lay to the west of a line drawn from the north coast to Loch Ryan, down the eastern boundaries of Sutherland, Inverness, Perth, Argyle, Renfrew, and Ayr. That this difference could not be referred as altogether due to difference of race was proved by the fact that amongst the pure Scandinavian population of Orkney and Shetland the rate was much below the average. The results pointed to a low moral tone in the agricultural districts of Elgin, Banff, Aberdeen, Roxburgh, and Galloway.—Prof. Tait communicated an account of Prof. J. E. MacGregor's experiments on the absorption of low radiant heat by some gaseous and vaporous bodies. The apparatus was a gigantic form of that which has already been described in these columns (see NATURE, vol. xxvi. p. 639). Air saturated with water vapour at 12° C. behaved almost exactly like air mixed with ·06 per cent. by volume of olefiant gas.—Prof. Tait, in a note on the compressibility of water, stated that water seemed to be less compressible at higher than at lower pressures, and more compressible (as compared with steel or glass) at lower than at higher temperatures. This latter result was obtained by comparison of his own laboratory experiments with the experiments carried out by Mr. Murray and Prof. Chrystal in their deep-sea sounding expedition last summer on the north-west coast of Scotland. Both series of experiments were made with Prof Tait's steel and glass gauges.—Prof. Crum Brown communicated a note by Mr. A. P. Laurie on an application of Mendeljeff's law to the heats of combination of the elements with the halogens. Laying off as abscissæ numbers representing the heats of combination of different salts of a given halogen, and measuring as ordinates the corresponding atomic weights of the other element in the compound, Mr. Laurie obtains a succession of points which show a remarkable periodic arrangement. The curves so drawn for the different halogens are strikingly similar.

PARIS

Academy of Sciences, January 8.—M. Blanchard in the chair.—The following papers were read:—Observations on the last communication of Dr. Siemens concerning solar energy, by M. Faye.—On the ice-plant (*Mesembrianthemum crystallinum*), by M. Mangon. This plant (which is covered with transparent vesicles filled with liquid, like frozen dew-drops) is formed of a weak solution of alkaline salt, kept in the solid state by a vegetable tissue, whose weight reaches less than two per cent. of the whole mass. The ashes, formed of salts of soda and potash, constitute nearly half (43 per cent.) the weight of the dried plant (recalling seaweed). M. Mangon notes the plant's elective power, suggests that its cultivation, as a potash-plant, might be useful in some cases, and in any case, it might do good service in removal of alkaline salts in excess from ground on the Mediterranean coast.—Researches on hyponitrites; second part; calorimetric measurements, by MM. Berthelot and Ogier.—On the natural formation of bioxide of manganese, and on some reactions of peroxides, by M. Berthelot.—Experiments relating to disorders of motility caused by lesions of the apparatus of hearing, by M. Vulpian. He describes a series of disorderly movements produced in rabbits by pouring a few drops of hydrate of chloral solution into one ear or both ears. The same experiment with dogs and guinea-pigs gave much less marked effects.—On complex units, by M. Kronecker.—Examination of the analogy between electrochemical and hydrodynamical rings, and the curves $\Delta V = 0$; Best process of discussions in the experimental method, by M. Ledieu.—Experiments on the motion of current waves in various passages, contracted either in the interior or at the extremity of a canal debouching into a reservoir, by M. de Caligny.—Report on a memoir of M. de Salvert on conic umbilici.—On the precision of longitudes determined with use of the new chronometric method, by M. de Magnac. He shows by a list of chronometric longitudes obtained in the *Jean Bart,* on the coast of Brazil and Montevideo, compared with the telegraphic longitudes, deduced from observations by three American officers, that the difference is remarkably small.—A case of damage to a building from lightning was reported; the effect was attributed to breaks in certain metallic parts (there was a good lightning-rod and there were trees near).—The periodicity of comets, by M. Zenger. He finds the origin of comets intimately connected with the rotation of the sun. Dividing the intervals of times of perihelion by various whole numbers, he obtains a mean value of 12·56 days, which is exactly that of a demi-rotation of the sun. Thus, between successive formations of comets there must have elapsed an even or odd number of solar demi-rotations. He supposes enormous explosions driving far out the matter of protuberances; large meteorites near the outer border of the corona may thereby be enabled to agglomerate coronal matter round them and form a comet. The general law of motions of planets applies equally to comets, but the duration of revolution of comets must be a multiple of that of a half-rotation of the sun.—Addition to a note on prime numbers, by Mr Lipschitz.—Influence of cooling on the value of maximum pressures developed in closed vessels by explosive gases, by M. Vieille.—Remarks on the expression of electric magnitudes in the electrostatic and electromagnetic systems, and on the relations deduced from it, by MM. Mercadier and Vaschy.—

Phosphorography of the infra-red region of the solar spectrum; wave-lengths of the principal lines, by M. Becquerel. He gives a determination of new lines of the solar spectrum and their wave-length, [and he has observed in the infra-red spectrum, maxima and minima of extinction proper to different phosphorescent substances manifested by various luminous sources, and similar to phosphorogenic maxima and minima in the other end of the spectrum.—On solar photometry, by M. Crova. Correcting a numerical error, he obtains 8,500 carcels for the sun's luminous intensity in a clear sky, and so removes the inexplicable discordance of his former figures with those of Bouguer and Wollaston.—Manganese in dolomitic strata; origin of the nitric acid, which often exists in natural bioxides of manganese, by M. Dieulafait. There are two classes of ores of manganese; those of the first class are directly derived from action of sea-water on primordial rocks, and they are deposited but a short distance from their place of extraction. Those of the second class have been, since the origin of seas, in complete solution in their waters, and have been deposited at all epochs, where chemical conditions have been favourable.—On the existence of the genus *Todea* in Jurassic strata, by M. Renault.—On a trombe observed at sea, by M. de Tromelin.—A work by Prof. Inostranzeff, of St. Petersburg, "On the prehistoric man of the stone age of Lake Ladoga," was presented by M. Daubrée.

BERLIN

Physical Society, January 5.—Prof. Helmholtz in the chair.—Prof. Spörer, of Potsdam, first communicated the results of an investigation of the sun-spot observations in the twenty years, 1861 to 1880, with a view to settlement of the question whether movements of the spots indicated surface-currents on the sun. It appeared that such currents, towards the pole, or towards the equator, were not demonstrable. Herr Spörer further spoke at length on a quite peculiar phenomenon he had noticed on observing the transit of Venus on December 6. He premised that the phenomenon might be explained in two ways; either it might be regarded as an effect of fatigue or over-stimulation of the eye (though he had not marked other signs of such exhaustion), or it might be connected with a very cloudy atmosphere of Venus, whose presence is supposed to be indicated by the glow which some astronomers have seen to extend along the Venus-crescent over the whole planet. The phenomenon itself was as follows: the transit of Venus was observed in Potsdam with a 10-foot telescope; the sunlight was reduced to a degree of brightness bearable by the eye by means of a polarising arrangement (two pairs of parallel mirrors). The sun's limb was much agitated, and the first contact could not be observed. When the planet had made a distinct indentation on the sun, it was considerably blacker than the ground of the heavens; and the part of the planet lying outside the sun was invisible. After more than half the disc had entered the sun, it was observed that the borders of the black-planet disc was still at right angles to the sun's border, and the two sun-points were absent. Later, when Venus was further advanced, the whole disc, and even the small part lying outside, appeared brighter than the ground of the heavens, and with a dull grey light. Another minute later a small interrupted line having previously been seen outwards and upwards from the planet's disc, on the ground of the heavens, there appeared, out from the grey disc, a dark crescent-shaped segment, which, above and below, was distinctly defined, and in the middle merged indefinitely in the similarly coloured ground of the heavens. The grey disc with the dark crescent advanced on the sun, so that it was not possible to distinguish precisely the planet's disc. At about 3h. 11¼m. the outer border of the grey disc was in the connecting line of the two solar horns, and so in the first internal contact; one minute later, an alteration (not more exactly describable) had occurred in the aspect of the planet's disc, and about 3h. 13¼m., at the time of the previously-calculated first internal contact, the outer border of the dark segment had entered; one saw a fine luminous line on the sun without black drop. One minute later the sun disappeared behind a bank of clouds.—Dr. Herz communicated the results of calculations he had made with a view to answering the question, whether the tidal action of the moon is capable of producing currents of water-masses on the earth of such an order of magnitude, that the ocean-currents observed might be explained by this cause. Proceeding on the assumption of a water channel running round the equator, he found for liquids with friction, that the tide must indeed produce a current; and for a whole series of such channels reaching from the equator to the pole there appeared currents, which in their co-operation would present the form of the great ocean currents, but their order of magnitude was such, on the assumption at the outset, that the actual ocean currents cannot be due to this cause. Herr Herz then, conversely, calculated from the astronomically-proved retardation of the earth's rotation, the tangential force, which can produce such a retardation, and determined the differences of the water-levels on the east and west coast of a very narrow dividing ridge of land, which would produce such a pull; these differences of level were deducible from the tidal action of the moon.—Prof. Ostwald, from Riga, reported, on his experiments for measurement of the chemical forces of affinity. As is usual in physics, he measured these forces by mass and velocity of the reactions, or by the force with which equilibrium is maintained. Two acids were each brought into contact with a base, and the salts formed were determined; in all cases, the affinities were found proportional to the reacting mass and the square of the velocity of the reaction. The formula construtecd a few years ago by Herren Guldberg and Wage for affinity, has been confirmed by the author by numerous experiments.

VIENNA

Imperial Academy of Sciences, November 30, 1882.—V. Hausmaninger, on the variability of the coefficient of diffusion between carbonic acid and air.—P. Kowalewsky, on the relation of the nucleus lentiformis to the cortex of brain in man and animals.—V. Hilber, on recent land-snails and land-snails found in the loess from China (part 1), containing a description of Helix species collected by L. v. Loczy during the Asiatic Expedition of Count Szecheniji. E. Stefan, on the experiments made by Boltzmann on sound-vibrations.

December 7, 1882.—V. v. Lang, on his capillary balance.—H. Niederriss, on trimelhene-glycol and the bases of trimelhene.

December 14, 1882.—F. Streintz, on the usefulness of the method of Fuchs.—Tg. Klemencic, on the capacity of a plate-condenser.—A Wassmuth, on the internal connection of some electro-magnetic phenomena resulting from the mechanical theory of heat.—V. Gruber, fundamental experiment on the cutaneous sight of animals.—G. Vortmann, on the separation of nickel from cobalt.—R. Canaval, on the earthquake at Gmünd (Austria) on November 5, 1881.—E. Weiss, communication on the observations of the transit of Venus in Austria.—H. Weidel and M. Russo, studies on pyridine.

CONTENTS

PAGE
GEIKIE'S GEOLOGY, II. By G. K. GILBERT, U.S. Geological Survey 261
SACHS'S TEXT-BOOK OF BOTANY. By Prof. E. P. WRIGHT . . . 263
RECENT ELECTRICAL PUBLICATIONS 264

OUR BOOK SHELF:—

Aldis's "Introductory Treatise on Rigid Dynamics" 265
"Encyklopædie der Naturwissenschaften" 265

LETTERS TO THE EDITOR:—

Pollution of the Atmosphere.—H. A. PHILLIPS 266
A "Natural" Experiment in Complementary Colours.—CHAS. T. WHITMELL 266
The Comet.—W. T. SAMPSON 266
The Transit of Venus.—Prof. EDGAR FRISBY 266
Early Coltsfoot —R. MCLACHLAN 266
Baird's Hare —T. MARTYR 266
The Projection of the Nasal Bones in Man and the Ape.—J. PARK HARRISON 266
THE COMET. By Dr. B. A. GOULD 267
DESTRUCTION OF LIFE IN INDIA BY WILD ANIMALS. By Sir J. FAYRER, F.R.S., K.C.S.I. 268
PALÆOLITHIC IMPLEMENTS OF NORTH-EAST LONDON. By WORTHINGTON G. SMITH (*With Illustrations*) 270
LEVER'S ARC LAMP (*With Illustration*) 274
NOTES 275
APPROXIMATIVE PHOTOMETRIC MEASUREMENTS OF SUN, MOON, CLOUDY SKY, AND ELECTRIC AND OTHER ARTIFICIAL LIGHTS. By Sir WILLIAM THOMSON, F.R.S. 277
THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION. By Prof. A. A. W. HUBRECHT 279
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 281
SCIENTIFIC SERIALS 281
SOCIETIES AND ACADEMIES 282

THURSDAY, JANUARY 25, 1883

THE THIRST FOR SCIENTIFIC RENOWN

FEW students of science can fail to feel at times appalled by the ever-increasing flood of literature devoted to science and the difficulty of keeping abreast of it even in one special and comparatively limited branch of inquiry. Were merely the old societies and long established journals to continue to supply their contributions, these, as they arrive from all parts of the country, and from all quarters of the globe, would be more than enough to tax the energy of even the most ardent enthusiast. But new societies, new journals, new independent works start up at every turn, till one feels inclined to abandon in despair the attempt to keep pace with the advance of science in more than one limited department.

One of the most striking and dispiriting features of this rapidly growing literature is the poverty or worthlessness of a very large part of it. The really earnest student who honestly tries to keep himself acquainted with what is being done, in at least his own branch of science, acquires by degrees a knack of distinguishing, as it were by instinct, the papers that he ought to read from those which have no claim on his attention. But how often may he be heard asking if no means can be devised for preventing the current of scientific literature from becoming swollen and turbid by the constant inpouring of what he can call by no better name than rubbish!

Some sciences seem to be specially exposed to inundation of this kind. Geology lies exposed to it in an unusual degree. Popular in its subject, and capable of ready apprehension as to its general principles, this department of science allures the outsider into its precincts, where he too frequently soon arrives at the belief that to have read a geological book or two is to become a geologist. This belief would be harmless enough, did it not speedily bear fruit in "papers" communicated to scientific journals, and stamped with all the enthusiasm and crudity of a beginner. On no account should any check be placed on the legitimate ambition of the youngest aspirant after scientific renown. But we venture to think that the common precipitate publication of his earlier efforts is not a legitimate ambition; but on the contrary is really an injury to himself and a positive hindrance to the progress of the science which he no doubt loyally desires to serve. It too frequently happens, moreover, that his first efforts are directed to the pleasant task of discovering flaws in the work of those who have preceded him. And of course the more eminent these predecessors, the greater his credit in setting them right. Let him take to heart the old maxim, *Festina lente*. The longer he delays his appearance as an author, and the wider he meanwhile extends his practical experience of nature, the more tolerant will he become of the work of others and the less overweeningly confident of his own. In no department of natural knowledge can a real acquaintance with the subject be gained save as the result of prolonged study.

These reflections have been suggested on the present occasion by the perusal of a pamphlet which exhibits in the most glaring way the tendency on which we have animadverted. It is devoted to the announcement of a brand-new theory of the origin of Fingal's Cave.[1] Curiously enough this is not the first time that the basaltic colonnades of Antrim and the Scottish Isles have furnished the text for teaching the most arrant nonsense. Nearly forty years ago a sailor, familiar with tropical bamboo jungles, started the idea that columnar beds of basalt are neither more nor less than petrified growths of bamboos. After vainly trying by occasional newspaper letters to find supporters, he seems to have given up the struggle against the blind prejudices of geologists. In 1864, however, his views were taken up by another writer yet more outrageous, who published a pamphlet of nearly 100 pages, entitled "The Giant's Causeway once Bamboos," and in supporting his dogma ran a tilt at religion, science, tradition, history, in short at everything that happened to suggest itself in the course of his incoherent and erratic pages.

The author of the pamphlet cited below, Mr. F. Cope Whitehouse, M.A., &c., is obviously a man of original genius, and is resolved that the world shall know it. In the summer of 1881 he seems to have come with the mob of tourists that annually makes a pilgrimage to the coast of Antrim. But instead of merely submitting to be led through the usual route by the inevitable and inexorable guide, he boldly separated himself from the gaping crowd, and proceeded to meditate. To his rapid mental vision it was soon apparent that the caves of that coast-line, instead of being the work of the sea, as ignorant mankind has hitherto believed, have been hollowed out by human hands. At once he could perceive the intimate relationship of Gothic doorways, ancient civilisation, mediæval castles, Irish manuscripts, and the "Kelto-Iberian, Wend or Phœnician" race. The narrow sea-worn gullies of Antrim being thus shown to have been ancient harbours, his eye looked northwards to the dim blue Scottish Isles, and his venturous imagination at once demanded whether that world's wonder, Fingal's Cave, might not after all be merely a piece of man's handiwork. To state the question was in effect to answer it affirmatively. Nevertheless that no unsympathetic geological Philistine might blaspheme, our courageous hero sailed for those far islands of the west, saw Staffa with his mortal eyes, and found how well his prophetic intuition had divined the secret of that weird place. We almost envy the thrill of satisfaction that must have vibrated within him as he proudly felt that scientific observers for a century past, from the days of Sir Joseph Banks down to our own time, had one and all missed the true meaning and history of the Caves of Staffa, and that it was reserved for him, casual visitor as he was, to lift the veil and reveal the mystery to our astonished gaze.

Knowing well the type of which Mr. Whitehouse is a fresh and most characteristic example, we hardly require his assurance that as soon as his eye lighted on Staffa his "conjecture received strong and unexpected confirmation. It was subjected to rigid examination; it was strengthened by opposition." Of course it was. Then, like all similar enthusiasts, his soul could find no rest until he had proclaimed the truth to the nations. Returning to America, he found an opportunity of enlightening the darkness of the American Association for the Advancement of Science, at its meeting in Montreal in August last. A few weeks later he proclaimed the great discovery to the Academy of Sciences of New York. But these were limited audiences, though composed, no doubt, mainly of scientific men on whom as yet the true light had never shone. It was absolutely necessary to appeal to a wider, and possibly more sympathetic public. Accordingly he published his views in the December number of the *Popular Science Monthly*. But he was still unsatisfied, till at last he conceived the noble idea of combining the spread of truth with promoting the erection of the Statue of Liberty enlightening the World. He hired a theatre in New York, gave an account of his astonishing observations, charging a dollar a head for admission, and stated that the proceeds of his "matinée" were "to be devoted to the pedestal of the colossal statue." Let us hope that the sum realised was worthy at once of the great truths proclaimed by the lecturer; and of the national object to which it was to be given. Future pilgrims to the colossal Statue of Liberty will piously scan the pedestal, searching for the stone that shall hand down to the far future the name of the illustrious seer who could brush away the tangled cobwebs spun by a century of scientific babblers, and pierce into the true meaning of the Cave of Fingal.

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[1] "Is Fingal's Cave Artificial?" A paper by F. Cope Whitehouse, M.A. (New York: Appleton and Co., 1882.)

Mr. Whitehouse has published so far only an abstract of his address, but he has had it well printed with good illustrations, and seems to have generously scattered copies of it broadcast over this country. It was not of course at all necessary that he should communicate the steps of the reasoning by which he was led up to his great discovery. And he has considerately refrained from troubling the world with such unprofitable details. Still one cannot help trying to follow the mental process by which an epoch-making deduction has been reached. We get from the abstract glimpses of the way in which the received explanation of the caves of Staffa collapsed at the touch of Mr. Whitehouse's genius. He visited the scenery in calm summer weather. From Staffa he could see the great sweep of the Mull cliffs to the east and the broken rampart of islands all round the rest of the horizon. As the smooth sea mildly heaved along the base of the basaltic colonnade, he could easily persuade himself that Staffa must be a "singularly sheltered," "land-locked" island; and that "the force of the breakers is inconsiderable." How absurd then must it have appeared to him to attribute to that placid lake-like water the power of hollowing out caves in a rock so obdurately stubborn as basalt! Moreover, he could see no reason why the sea, supposing it gifted with such power of erosion, should have chosen the places where the caves actually occur. And his inability to find this reason satisfactorily disposed of any possible action of the waves. Not only so, but from his stand-point at Staffa his clear vision could take in the whole coast-line of Scotland, and he made the further important announcement, which will doubtless for ever silence our northern geologists, who believe in the geological power of the waves, that "there are very few hollows worn by the sea in the Scotch coast!" Having cleared away the fictions of so-called scientific observation, he could apply the much more reliable conjecture which his glance at the Giant's-Causeway had evoked in his own mind. To his trained eye the caves of Staffa were obviously artificial. Oracularly he tells us that they are "strikingly Phœnician." "No such Gothic arch was ever formed by nature. No natural cave has an entrance higher than the interior." (!) Lastly, from the end of Fingal's Cave you can see the Hill of Iona rising against the sky, consequently the cave must have been excavated by men who lived on Iona. This final argument must be regarded as a crushing answer to those who have recklessly talked of the power of the waves in these regions. On what conceivable grounds can we suppose that the sea would make a tunnel, from the end of which the Dun of Iona would be visible?

Perhaps some unconvinced outsider may be tempted impertinently to ask for what object such stupendous excavations could have been devised by any civilisation, whether ancient or modern—excavations always swilled with the surge, often unapproachable for weeks together, and in which, even in calm weather, unless care be taken, a boat is liable to have its bottom knocked in. Of such questions Mr. Whitehouse very properly takes no notice. Again, we were in the belief that the religious community of Iona had been an eminently peaceable folk, liable to invasion by pirates from the sea or marauders from the mainland, but with little more to oppose in the way of defence than the prestige of their sanctity. But this conception also is now found to be false. We learn, on the same reliable authority, that they were a warlike race, quite able to look after themselves. It appears that Mr. Whitehouse has shown "the strategic importance of Staffa, and the probability that the wealth and refinement of Iona were due to the protection it afforded." He will have no difficulty in further proving that the traditional picture of the saintly Columba is a mere myth, and that the abbots of Iona possessed an army and navy, made war on heathen Pict and savage Scot, curbed the fury of fiery Norseman, and employed their gangs of prisoners in tunnelling the caves of Staffa!

There is a hollow among the rocky knobs that rise inland from the summit of the cliffs above Fingal's Cave. We would fain place Mr. Whitehouse there on a day when the gathering clouds have blotted out Ben More, when thick mists are driving along the opposite precipices of Gribon, when the Treshnish Isles grow fainter every moment in the western sky, when even Iona, that lies so near, is fading into the general gloom, and when the wild moan of the rising south-western gale among the crags around is answered by the hoarse clamour of the surge below. We should like to keep him there while the gale rapidly increases, breaker after breaker careering madly forwards with foaming crest from under the pall of driving rain that hides the sea, dashing into every creek and cave, rushing in sheets of green water up the face of the crags, and pouring back in hundreds of yeasty torrents into the boiling flood. We would ask him still to stay till the storm has reached its height, that he might feel the solid island shake under his feet, that he might see the sheets of water, foam, and spray thrown far up into the air, that he might hear the cannon-like thunder of the shock as each billow bursts into the Cave of Fingal. He would return a wiser (and a wetter) man, and would regret that in a rash moment he had published some childish nonsense about man having excavated sea-worn caves, and had expressed an opinion about the power of the sea of which he would then feelingly admit that he had been profoundly ignorant.

The pamphlet here noticed did not in itself deserve consideration in these columns. We have made use of it as a type of publication painfully frequent in the literature of science. If in exposing its characteristics we deter any rash and immature aspirant for fame from at once rushing before the world with what he conceives to be his discoveries, we shall have done a service at once to him and to science.

CINCHONA PLANTING

A Handbook of Cinchona Culture. By Karel Wessel van Gorkom. Translated by Benjamin Daydon Jackson, Sec.L.S. (London: Trübner, 1883.)

Die Chinarinden in Pharmakognostischer Hinsicht dargestellt. Von F. A. Flückiger. (Berlin: R. Gaertner, 1883.)

THE rapid extension of cinchona planting in India, Ceylon, and Jamaica will make a translation of Van Gorkom's account of the methods of cultivation and harvesting pursued by him, as Director of the cinchona plantations belonging to the Dutch Government in Java, useful to many who propose to turn their attention to this profitable industry. At present intending planters in British possessions have had little beyond Dr. King's Manual of Cinchona Cultivation (1876) to serve as a guide. In Ceylon the planting community includes many men of first-rate ability, and the singularly energetic journalism of the island speedily ventilates for the common good any fresh idea or point of practice in planting procedure.[1] Indian planters share the benefit of this, while Jamaica has the advantage of possessing in Mr. Morris, a director of its botanical department, who has carried to the West Indies an intimate knowledge of all that is being done in Ceylon. It is not very probable that those who are at present occupied in cinchona enterprise in British possessions will glean much from Van Gorkom's book. Still such a manual will not be without its use for those who have everything to learn about the matter, and, as will be seen, it cannot fail to be interesting to those who watch from an independent point of view the economics of the subject.

The book is handsomely printed and got up—too handsomely, indeed, for workmanlike use, for which its size, that of a small folio, seems particularly unsuited. We must too make a serious protest as to the style of the translation, which, we think, cannot be considered tolerable, even with every allowance for "seeming inelegancies" which Mr. Jackson pleads for in his preface. Take, as a sample, the first sentence which caught our eye:—

> "If we trust that this excellent opportunity for fruitful comparisons shall lead to unfettered judgment, still more do we look for, from the impressions received and the enlarged field of view, the scientific work carried on, which has so long been in hand, and most certainly with great completeness and undisputed knowledge of material, will indicate our present standpoint in the domain of quinology" (p. 264).

Now it is quite certain that this is not English, and we have some doubts whether it really conveys any meaning at all. But at any rate we would ask what is the use of translating in this way a work the purpose of which is not literary but essentially utilitarian. There seems, in fact, to be a deep-rooted superstition about the value of so-called fidelity in translating books of mere information. In rendering a foreign language as a philological undertaking, it is often desirable to sacrifice, to some extent, style and form, in order to convey as nearly as may be, the exact force of each word and of each turn of expression. But where, as in a technical treatise, it is only the context we care about, it is exasperating to find the translator exhibiting a would-be scholarly care over the exact reproduction of the vehicle. All we want him to do is to master the meaning and give it to us in clear, straightforward English.

Having said so much by way of criticism we may indicate a few points which we think will be interesting even to some who are not colonial readers of NATURE. A hundred of the three hundred pages of which the volume consists is given up to historical matter regarding the history of *Cinchona* and the development of its culture in Java and in British possessions. All this is an oft told tale, and contains little that will not be found in Mr. Markham's Peruvian Bark (reviewed in NATURE, vol. xxiii. pp. 189-191). An exception must be made, however, as to the interesting account of the commencement of cinchona cultivation in Bolivia. The existence of this enterprise was known, but we have not met with any previous account of it. The Dutch Consul-General reported to his Government:—

> "The great event in the agricultural region of Bolivia is the planting of the Bolivian cinchona forests, of which an earnest beginning was made in 1878. . . . The river Mapiri, in the province of Larecaja, department La Paz, has been the centre of the movement, and already the young trees of two years' growth, may be reckoned at from four to five hundred thousand" (p. 17).

Doubt is, however, expressed whether the planting will be maintained in the face of labour difficulties and a possible fall of prices in consequence of increasing exports from the East Indies.

Modern cinchona enterprise in Java has aimed at the production of barks rich in quinine. With the lucky purchase from Mr. Ledger in 1865 of a packet of seeds of the now well-known *Cinchona Ledgeriana*, the Dutch "cinchona culture of the future has entered upon an entirely new phase" (p. 79). About 20,000 of the seeds germinated in Java, and first and last Mr. Ledger received about 24*l.* from the Dutch Government, and "was therewith well content" (p. 91). Fortunately the greater part of the seed originally imported was purchased by a well-known Indian planter, Mr. Money, and some of it seems by private channels to have found its way to the Government plantations in Sikkim. The Dutch having got this valuable kind seem to have managed it with extraordinary intelligence and skill. Men like De Vrij, Moens, and Van Gorkom were well-trained European scientific men and competent chemists. Their object was by continuous selection, controlled by repeated analyses of bark made on the spot to obtain races of *Cinchona Ledgeriana* richer and richer in quinine, and it is a matter of general notoriety how well they have succeeded.[1] It is the part of Van Gorkom's treatise dealing with this matter which cinchona planters will be grateful to Mr. Jackson for putting within their reach. Two conditions of success in harvesting good seed are insisted upon.

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[1] Of T. C. Owen's Cinchona Planter's Manual, published at Colombo, we know nothing beyond the name.

> "For seed saving, the handsomest strongest trees are selected, and especially amongst those whose superior value has been ascertained by chemical examination. Disappointment is inevitable where the eye and botanical characters alone are made use of and trusted to; *the whole issue depends upon the certainty that varieties rich in quinine are exclusively propagated.*
>
> "The choice being made there is something else which must not be neglected; it further behoves us to be perfectly sure that the tree is not fertilised with foreign pollen, that is to say, pollen of an inferior tree or variety" (p. 136).

The last condition cannot be insisted upon too forcibly, notwithstanding that competent botanical opinion can be quoted against it. In their home in South America the different species of *Cinchona* are localised at different points of the Andine chain. Geographical isolation keeps them uncrossed. But where they are brought together in one plantation they hybridise freely. *Cinchona robusta*, which is now widely diffused in India, undoubtedly first originated in Ceylon as a cross between *C. officinalis* and *C. succirubra.*

The aim of the Dutch Government being to produce a commercial bark of high quinine-producing quality, in which they have met with extraordinary success, Van Gorkom is somewhat disposed to criticise the different policy which has been pursued in British India:—

> "The Bengal Government . . . makes its cinchona culture serviceable before all things to the wants of its population, and thus only asks itself, how the people and army may be provided with febrifuges on the most advantageous terms" (p. 229).

He sets against this the "well-known fact that not one half of the alkaloids possessed by the raw material are obtained, the greater part being lost.' Even supposing, however, that things are as bad as this, and not susceptible of improvement, it is still arguable whether, looking at the cheapness with which red bark can be grown and converted into a febrifuge—the usefulness of which is incalculable—the theoretical waste is a matter for the present of much consequence. But it is unreasonable to suppose that the Bengal methods of extraction are not susceptible of improvement, though they will probably never reach the standard practicable by more expensive methods in Europe. But the objection of wastefulness must be measured by the circumstances. The proprietor of an estate in England who, with a view of bringing a portion of his park into tillage, began by burning the timber upon it, would be considered a madman. But this is habitually done in clearing a piece of tropical forest for cultivation, and as it is not easy to see what else could be done, a complaint as to the waste would not be much to the purpose. It might have been expected that Van Gorkom's sympathies would have centered in the quinine-producing yellow barks which are for the moment most in favour. This, however, is largely due to the unreasonable importance which is attached to quinine over other cinchona alkaloids. Van Gorkom does not share this prejudice:—

> "The conviction has more and more gained ground, that good cinchona barks judiciously applied, frequently do not merely rival quinine, but even surpass it in useful effect" (p. 212).

This point of view is exceedingly important with regard to red bark (*C. succirubra*), which is the easiest of all species to cultivate.

> "There is no cinchona bark richer in alkaloids, and though *C. succirubra* is not suitable for the preparation of quinine, because it can only be treated with trouble and much expense, yet it has a preponderance of the secondary alkaloids. No better material for pharmaceutical purposes is known, and on that account its propagation is desirable from every point of view" (p. 100).

High class yellow barks are by no means free in their growth or particularly easy of cultivation. It has been found useful to graft them on *succirubra* stocks, and the practice has been adopted in Sikkim and Ceylon; Van Gorkom gives a useful account of the method adopted in Java.

We must refrain from pursuing many other points which these pages suggest. Two of the concluding chapters deal with the possible synthesis of quinine and the commerce of the barks. As to the former the author has little doubt of success. Two isomerous bodies, chinoline and chinoleine, are known, of which the former is obtained by the distillation of coal tar, the latter by that of quinine This is thought then to be the clue by which the construction of quinine from coal-tar products will be eventually achieved. But he takes comfort for cinchona planters from two considerations. One is that the synthesis of a vegetable substance when effected does not always result in its practical commercial replacement. The synthesis of alizarine it is found after all does not give the dyer quite what the madder plant gives him. Artificial quinine then may—if ever produced—prove only of interest to the chemist. His other consolation is based on what is said above—that pharmacy can never dispense with the total aggregate extracted products of bark, and the day may be regarded as indefinitely distant when the chemist will be able to replace these any more than such complexes as the contents of our tea- and coffee-pots.

As to commerce it is interesting to learn that London is the most important market for bark, and Paris next. We fear, however, from statistics obtained from another source, that this country has no corresponding lead in the production of the manufactured products, only about 10 per cent. of the quinine of the world being made in England. Yet Van Gorkom states emphatically that "the consumption at the present day of cinchona and its alkaloids, merely represents a paltry fraction of the quantity which will be required to satisfy the prescription of humanity in every country, and among all classes and races of men" (p. 236).

We have left ourselves but little space to notice Prof. Fluckiger's handy and concise work, which, though of importance to cinchona planters, is primarily a pharmaceutical study of the subject. The bark of *Cinchona succirubra* has been recently adopted as the official bark of the German Pharmacopœia—a fact of no small importance to planters in British possessions, when it is remembered how enormous is the extent of its cultivation in their hands. It is this fact which has won it its official status, as though poor in quinine its quality is tolerably uniform, and being easily grown its supply can always be depended on. Prof. Flückiger gives a figure of the plant as well as of *Cinchona Ledgeriana*—the quinine bark *par excellence*—and of *Remijia pedunculata*, one of the sources of the *Cinchona cuprea* which has of late years been poured into European markets from South America.

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[1] Acknowledgment must be made of the striking liberality with which the Dutch Government officials have always placed what they could spare of their selected seed at the disposal of planters in other countries.

MARINE SURVEYING

A Treatise on Marine Surveying. Prepared for the use of younger Naval Officers, by the Rev. J. L. Robinson, B.A., Royal Naval College. (London: Murray, 1882.)

THIS book has been written apparently with the view of enabling young naval officers to cram themselves sufficiently to pass the examination in surveying at the Royal Naval College, and it must be conceded displays considerable industry on the part of Mr. Robinson, who has evidently taken pains to go through the examination papers on surveying from their commencement, to see what questions are usually asked, and in what form they could be best answered; and has besides consulted a large number of works bearing on surveying, a list of which he gives at the commencement of his treatise; but we confess we are much disappointed that with such excellent materials, so poor a result should have been produced, for, with the exception of the chapter on tides, which in its way is excellent, the work is of very little value, and rather reminds us of that treatise of—

> "The young lady of Buckingham
> Who wrote about geese and stuffing 'em,
> But found out one day
> She'd neglected to say
> A word in her book about plucking 'em."

Mr. Robinson says in his preface "he has had no intention to write a handbook for the use of the practical surveyor," and that "such an intention might fairly be regarded as an impertinence in one who has never been engaged in the practical work of the profession," but that he has had rather "the examination room and its requirements before him." But did it not strike Mr. Robinson that the practical surveyor selected to examine the candidates might ask questions upon which he has neglected to touch, and that consequently his treatise might fail to ensure success in the "examination room," notwithstanding the valuable hints he has received from Staff-Commander Johnson and his friend of great experience as a first-class surveyor?

The first chapter consists of extracts from Admiralty publications, but we recommend the officers at the college to consult those publications for themselves, more especially the Admiralty list of abbreviations, as the illustrations in this work give a poor representation of the symbols and signs used by the draughtsman and engraver.

The second chapter, on the Construction and Use of Scales, and the sixth, on Instruments, are derived principally from Heather. Here again we prefer the original to the copy.

The third chapter, on Laying off Angles, merely contains a brief description of the methods of plotting angles by chords with a small radius. On this we would remark that the real value of plotting angles by chords consists in their being plotted with long radii, as any practical draughtsman could have informed the author.

The fourth chapter is a most elaborate analysis of the method of Fixing a Position by Angles, &c. Surveyors take sextant angles, principally, to fix their positions when sounding, and invariably use the station pointer for that purpose; this chapter therefore seems to us to be firing a 12-ton gun at a sparrow.

The fifth chapter, on Charts and Chart Drawing, is rather a description of the method of map construction, and contains some mis-statements. Evidently Mr. Robinson is not well acquainted with the mode of constructing charts at the Admiralty or by surveyors, as he states in one paragraph that circumpolar charts are usually constructed on the gnomonic projection, whereas we are not acquainted with one Admiralty circumpolar chart on this projection. It is true a diagram is published to facilitate the practice of great circle sailing but no circumpolar chart.

The fact is all marine surveyors project their work on the gnomonic projection, and as the smallest scale in use is an inch to a mile, it is evident that the errors of this projection are very slight, as the largest sheet of paper that can be worked at conveniently is about six feet square. When the original surveys arrive at the Admiralty the Hydrographer decides in what form they shall be engraved and published. If the surveys are plans of harbours, they are usually published on the gnomonic projection (as they were originally drawn); if the survey is of a coast, or to be incorporated in a coast, or general sheet, it is transferred to the mercatorial projection, for which the meridional parts of the spheroid are used. Charts of the circumpolar region are however published on an arbitrary projection, in which the parallels of latitude are drawn as concentric circles at equal distances from the pole.

Chapter seven is on Base Lines. Now base lines are principally of use to the marine surveyor as the quickest method of starting his work, which, when it extends over a large area, almost invariably depends eventually for its scales on astronomical observations.

Mr. Robinson states that it is impossible to fix the position *exactly* by means of a sextant. Here we must differ from him, and will give one instance to the contrary. When the question of the boundary between the United States and British North America was decided, and the 49th parallel was fixed on, Admiral Sir George Richards then in command of H.M. surveying vessel *Plumper*, at Vancouver's Island, was directed to ascertain the position of this boundary line on the western seaboard of North America. This he did with a sextant, and buried a mark in the ground on the position of the 49th parallel as ascertained by himself. The Americans sent a party for the same purpose with a zenith sector and altazimuth and when they had fixed the position of the 49th parallel by these means, the difference between the two results was found to be less than 100 feet! It is of course as well that nautical surveyors should know the various methods employed in obtaining accurate bases for geodetical measurements, but for marine surveying the same nicety is not required as in measuring the arc of a meridian, and it cannot be too often impressed on the mind of the aspirant in surveying that over accuracy (that is such minuteness as cannot be represented on paper) is loss of time.

The eighth chapter is on Triangulation, and is more worthy of attention than those preceding. If we remember rightly, there was only one three-feet theodolite, used in the Ordnance Survey of Great Britain, which instrument is the property of the Royal Society. In fact, so far as we are acquainted, there are only two three-feet theodolites in existence, Ramsden's, and another used in the Great Trigonometrical Survey of India.

The observation that very distant stations are generally observed at night is now subject to correction, as the heliostadt has rendered it quite as easy to observe by day, in fact, in some of our marine surveys, triangles whose sides were 60 miles in length, have been obtained with these instruments, and an eight-inch theodolite, with the greatest ease. With the eight-inch theodolite, and by means of repeating the main angles round the circle, very accurate results may be obtained; and the spherical excess has to be allowed for, and deducted, in order to make the triangles plane, for in all nautical surveys the chord of the arc is used both for calculation and plotting.

The ninth chapter, on Levelling, contains not only an account of levelling, but also of obtaining heights by means of the barometer and thermometer, but totally neglects the method in general use by surveyors, viz. by angles of elevation and depression with a theodolite. For travellers the barometer and thermometer give an approximation of the elevation, which is exceedingly useful in an unsurveyed district. For work requiring extreme accuracy careful levelling is required, but for nautical work the principal use of the level is to ascertain the exact difference between the zero of the tide gauge and some permanent mark on shore, so that a fixed datum can always be referred to for reduction of soundings in future. The heights of hills are almost invariably obtained by angles of elevation and depression, and the results so closely approximate to the truth that it is waste of time to do more, unless results less than five or six feet in error are absolutely requisite.

The tenth chapter is on Tides, and is, as has been before mentioned, well worthy of perusal, in fact, it is the most complete popular description we remember to have seen; and if compiled entirely by Mr. Robinson from the books he has consulted reflects great credit on him, and we can but wish he had paid the same attention to the other parts of his treatise.

We think, if we remember rightly, that it was in one of the Arctic voyages, that of Sir John Ross, that the influence of atmospheric pressure on the rise of the tide was first observed, but the fact is well known now, and is always allowed for by surveyors when ascertaining the mean level of the sea. This subject is of considerable practical importance, for it is sometimes the only guide we possess by means of which we can reduce our soundings to the same depth as those obtained at previous epochs. For instance, if the datum-mark to which the soundings have been referred, has, in the course of time, disappeared, the surveyor's first work is to ascertain the height on his gauges of the mean level of the sea. This he does by obtaining day and night observations for five or six consecutive high and low waters, carefully registering the barometer at the same time. Then meaning the results and allowing for the atmospheric pressure, it is astonishing how closely they agree. The mean level having been found, it is very easy to reduce the soundings to the former datum. For instance, if the soundings are reduced to low water ordinary springs, and their rise at springs is 16 feet, it is evident that the soundings must be reduced to 8 feet below mean level of the sea, to enable them to be compared with those previously obtained.

In his paragraph (201) on tide-gauges, Mr. Robinson recommends a string from a float over a pulley. It must be either a chain or wire, as a string is far too subject to contraction and expansion from atmospheric changes.

Self-registering tide-gauges are, we are glad to say, becoming much more common than they were, and we trust to see them established at every important point in the United Kingdom.

The eleventh chapter, on Soundings, may do very well to enable a sub-lieutenant to answer some questions in the examination-room, but is useless in practice.

Few surveyors think it necessary to accurately protract on a chart the position of the objects they use for sounding transits (Art. 210). Often the back mark is too far off to appear at all on the sheet, and the farther off it is the better, provided the atmosphere is clear; for a front mark the first conspicuous object in the foreground is seized—a conspicuous tree, the chimney of a house, the angle of a hedge, a boat hauled up on the beach, &c. If the back object is sufficiently far off, the lines of soundings are practically parallel, and the same mark may be used for the whole survey. It is also requisite to cross the lines of soundings, to avoid any chance of error. The sounding on the chart depends (1) on the leadsman, (2) on the officer fixing and registering, (3) on the tidal register, and (4) on the reductions being correctly made. Now there may be a mistake in either one of these, and consequently it is advisable to always cross the lines of soundings as a check. We have found that as good a method of checking the correctness of the results as any, is by running along the contour lines, as defined by soundings obtained at right angles to those lines.

Another remark on soundings we must also take exception to, viz. that (Art. 213) it is usual to make a reduction of a couple of feet below low water in doubtful cases

With respect to under-currents (Art. 221), Mr. Robinson appears not to be aware of the methods pursued by Sir G. Nares in the *Challenger*, and Capt. Wharton in the *Shearwater*.

Chapters twelve and thirteen, on Chronometers, and Meridian Distances, are principally derived from Admiral Shadwell's notes on the management of chronometers, and here we recommend the student to the original rather than the copy.

The method of calculating meridian distances, expounded by Dr. Tiarks, and fully explained by Sir Chas. Shadwell, is invariably used by nautical surveyors, and the results thus obtained have hitherto closely approximated to the later determinations by means of the electric telegraph.

Chapter fourteen, on the method of Plotting a Survey, deals almost exclusively with the small plans required by the examination papers at the Naval College, and as it instructs the student to plot *out* from the base line (which is never done in practice) cannot be recommended.

The method of plotting adopted in practice is to calculate out from the base line to the extreme points of the survey, or to the extreme points that will appear on any one sheet of paper, and then to plot *in*. Every practical draughtsman knows that it is far easier to say, "draw a straight line" than to do it, and that an infinite amount of trouble is saved by plotting in towards small triangles from large, as the errors of the plotter are then being constantly reduced, whereas in plotting *out* they are being continually enlarged. In fact we venture to say that no one is competent to write an article on plotting who has not been in the habit of projecting surveys for no one else can understand the extreme nicety required to make three lines from three stations to the same object coincide in one point.

It is possible that Mr. Robinson has compiled this work in hopes the Admiralty may order it to be accepted as the text-book on surveying at the College. We trust, however, that their Lordships may be better advised on the point. Already we have one book, ordered to be used, which contains a theory on winds, not by any means accepted by meteorologists, and this theory has at present to be learnt by all the younger naval officers. Now we have no objection to any one theorising on wind, or any other subject, but what we do object to is that a book containing such theories should be ordered to be the standard work at the colleges, simply because the gentleman who wrote it holds, and worthily holds, a prominent position there. We think that although theories should not be absolutely excluded from textbooks, they should deal principally with well-ascertained facts, leaving the student to develop for himself a theory from those facts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

A PERUSAL of Dr. Romanes' article on Natural Selection and Natural Theology, in the *Contemporary Review* for October, 1882, suggests a few remarks upon one or two points, which may not be out of place.

One would quite agree with Dr. Romanes in "insisting on the essentially distinct character of natural science and natural theology as separate departments of human thought." True as that is, in a just sense, how does it follow that there "is no point of logical contact between" the two? Does this mean that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum?* It would seem so, and that it must be so to justify his very thoroughgoing conclusion: (1) That Darwin's theory explodes *particular design* (which he chooses to identify with special or independent creation); and (2) that it does not allow us rationally to introduce the conception of "an ultimate cause of a psychical kind pervading all nature," the theory having "no point of logical contact with the theory of design even in the larger sense." That is, a *raison d'être* in particular is proved to be absurd; behind all secondary causes, one such may possibly exist, but it is not to be legitimately thought of!

Or does he mean only that Darwin's theory need not, and legitimately should not, concern itself with philosophy and natural theology? Very well: then let the disciples practise what they preach, and imitate their revered master, who was content to maintain that species became what they are by descent with modification, instead of by independent creation, leaving untouched the question whether or not they were designed to be what they are. If there be "no logical contact" between Darwin's theory and the theory of design, then this renowned investigator preserved more logical consistency than some of his followers: if he refrained because of "the essentially distinct character of natural science and natural theology," and because of his determination to consider only the former, he was no less consistent.

But after all, such questions may be consistent enough, and moreover they are inevitable; and so it is not wonderful that they are raised—and not rarely prejudged—on the scientific about as freely as on the theological side.

Anyway Darwin did not prejudge the question of design, while declining to discuss it, as is done, for instance, by the *dictum* that if the species of animals and plants were slowly evolved, the evidence of design has been utterly and for ever destroyed. That has been affirmed over and over, formerly in the main by the theologians, but now, when these have seen what it comes to, mainly by the anti-theologians; by both, seemingly, under a misapprehension of the real character of the evidence for design.

Dr. Romanes' view is fairly presented in his denial that, under our present knowledge, "the facts of organic nature furnish *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design, if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly*."

Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually?* How has the former presumption—the contrary of which was quite inconceivable—been done away with? For this presumption arose, and had its full force under the consideration of animals and plants produced by natural propagation; and the then irresistible inference of intelligent design was drawn directly from their adaptations in themselves and to their environment; whence it was concluded that the series of phenomena must have been instituted somehow and at some time or times (sudden creation is no doctrine of natural theology) under intelligence. How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations to the environment? We do not see how or why the inference, which was so cogent, should under the new showing become at once irrelevant and out of all logical connection with the facts of the case, which *quoad design* are just what they were. *Suddenness*—if that must needs be entertained—is of course incompatible with the Darwinian view, and also with the facts as we understand them; but *gradualness* is in nowise incompatible with design. Under the conception of Nature as the outcome of Divine intelligence, questions of time and mode, of generality and particularity, are well nigh devoid of real significance.

But what may be contended for, and what is probably meant, is that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world upon known physical principles, and so renders the idea of design superfluous, as some would say; or, as it is better stated by Dr. Romanes, renders the evidence of design from these adaptations of no other or better value than that from anything else in Nature. So that the argument from teleology "must now take its stand upon the broader basis of the order of nature as a whole." This last, sensible natural theologians are prepared for. But the whole is made up of parts; and it is a whole in which the designed (if such there be) and the contingent can never be accurately discriminated, in which, indeed, from the very nature of the case, limitation is inconceivable. This need not be wondered at, since we are equally unable to discriminate the two in human action. The evidence of design may be irresistible in cases where we cannot indicate its limits. We can only infer with greater or less probability, according to circumstances, and especially according to relation to ends. Better evidence than that of exquisite adaptation of means to ends is seldom, if ever, obtainable of human intention, and in the nature of the case it is the only kind of evidence which is scientifically available in regard to superhuman intention. Now if means and ends are predicable of inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*.

To make the contrary supposition tenable, it must be shown that natural selection scientifically accounts for the adaptation; that the survival of only the very best adapted, out of the brood of more or less adapted to the environment at the time, gives sufficient scientific explanation of the adaptability or actual adaptation of the organism. Certainly this has not yet been done, and it seems incredible that it ever will be. That organisms have undergone changes as the Darwinian theory predicates, and that these changes have been picked out and led on by natural selection, seems to me most probable. That the action of the environment in some wholly unexplained way induces organisms to movement and change which would not otherwise occur, is also probable; but such change appears to be a response of the organisms to the physical surroundings and stimuli. And this most important factor in the result receives no explanation from the natural selection which operates upon it or co-operates with it. In other words, real causes have been assigned under which, *given the requisite changes*, the actual diversity and adaptations of plants and animals must or may have come to pass. But none have been assigned under which the organisms *must* have responded in the ways they do, or have responded at all, to the influences of the environment. Yet this is the very gist of the matter. The whole tenor of Darwin's writings and many explicit statements assure us that he completely recognised this distinction, which less exact minds overlook. If this distinction is valid, then the conclusion is at least premature which affirms "that the argument from teleology has been dislodged by the theory of natural selection," and its special value, as derived from adaptations in organic nature, utterly and for ever destroyed."

ASA GRAY

Cambridge, U.S.

Intelligence in Animals

MR. ROMANES remarks in his book that there are few recorded instances of intelligence in bears; the following facts may therefore be worth recording:—In the Clifton Zoological Gardens there are two female Polar bears between two and a half and three years old, which came here quite young. One of these shows remarkable intelligence in cracking cocoa-nuts. A nut was thrown to-day into the tank; it sank a long way, and the bear waited quietly till after some time it rose a little out of her reach. She then made a current in the water with her paw, and thus brought it within reach. This habit has already been several times noticed in Polar bears. She then took it on shore, and tried to break it by leaning her weight on it with one paw. Failing in this, she took the nut between her fore-paws, raised herself on her hind-legs to her full height, and threw the nut forwards against the bars of the den, three or four feet off. She then again leant her weight on it, hoping she had cracked it; but failed again. She then repeated the process, this time successfully. The keeper told me she employed the same method to break the leg-bone of a horse. That this is the result of individual experience, and not of instinct, is clear from the fact that her companion has not learnt the trick of opening them thus, nor could this one do it when she first came. The method of throwing is precisely similar to that adopted by the Cebus monkey described by Mr. Romanes.

J. G. GRENFELL

Clifton College, Clifton, Bristol, January 15

On a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies

AS I do not find that the following relation between the latent heat of evaporation, the expansion undergone in changing into the gaseous state, and the specific heat of a volatile body has been previously pointed out; and as, if verified, it might be of some value in the determination of one or other of the above quantities I submit it, not however without considerable diffidence, to the readers of NATURE.

Briefly stated the relation stands thus—

The latent heat of gasification at constant pressure of any body, divided by the product of the relative volume of the gas and the specific heat of the body is approximately constant; or, if

λ = latent-heat of gasification of any body,

v = relative volume of the gas; *i.e.* the vol. of the body on assuming the gaseous state compared with its vol. as a liquid,

s = specific heat of the body. Then

$$\frac{\lambda}{v \times s} = \text{const.}$$

The calculated value of this constant approximates to 0·8, as will be seen in the following table.

The letters λ, v, and s, heading columns 2, 3, and 4, have the same signification as above.

	λ.	v.	s.	$\frac{\lambda}{v \times s}$
Ether	91·11	228	·515	·775
Carbon disulphide . .	86·67	414	·235	·890
Wood spirit	263·7	651	·645	·628
Bromine	45·9	510	·107	·841
Oil of turpentine ...	68·73	204	·410	·837
Formic acid	169·0	548	·536	·575
Ethyl acetate	92·6	209	·527	·840
Methyl acetate	110·2	321	·507	·677
Butyric acid	114·67	271	·503	·841
Ethyl formate	105·3	241	·513	·851
Amyl alcohol	121·0	268	·587	·967
Acetone	129·7	339	·530	·736
Alcohol	208·0	456	·547	·833
Benzene	91·47	282	·395	·821
Chloroform	61·0	318	·232	·828
Perchloride of carbon.	47·0	263	·198	·902
Phosphorus trichloride	51.42	311	·209	·790
Methyl butyrate ...	87·33	273	·487	·657
Ethyl chloride	93·0	320	·427	·679
Ethyl iodide	46·87	317	·162	·914
Acetic acid	102·0	515	·503[1]	·393
Chloride of arsenic ...	46·5	324	·176	·813
Tetrachloride of tin ...	30·53	237	·148	·869
Water...	537·0	1612	1·000	·333

It would appear then that the latent heat of a body may be considered as approximately proportional to the expansion of the body in vaporising and to its specific heat; and that the amount of heat required to convert a unit mass of the body at the boiling point from the liquid to the gaseous state, is equal to an amount of heat which would raise through one degree a quantity of the body in the liquid state which is approximately proportional to the expansion undergone by the liquid on evaporating.

It will be noticed that among the bodies instanced in the table there are some which appear to be very far indeed from according with the relationship in question. Notably acetic acid and water; of these, however, water presents so many peculiarities that perhaps it may be allowable to consider this as only adding one more to their number. In the case of acetic acid it is noteworthy that in plotting the curve of the latent heats of the group of acids of which acetic acid is a member, Favre and Silbermann found an irregularity arising from this body. It is, at any rate, possible that this irregularity may mean an error in the determination of the latent heat of this body.

Considering the difficulties which attend the accurate determination of latent heats, relative volumes, and specific heats of the several bodies, and that, of course, an error in any one of these will introduce inaccuracies into the constant, it may well be supposed that some, at least, of the variations noticeable in the results tabulated arise from inaccurate data. Further, there are in many cases two or more distinct determinations of these physical properties extant, of which one might be so selected in each case as to reduce the variations occurring in the constant to a minimum.

F. TROUTON

Trinity College, Dublin

The Gresham Funds

IN an account of a meeting of the Common Council of the City of London, held last week, I read in the *Times* that the

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[1] Varies with temperature of determination irregularly.

Gresham Committee reported that they had agreed with the Mercers' Company upon a scheme by which the open area of the Royal Exchange should be roofed over at a cost of 10,000*l.* Does this mean that the funds of the Gresham Estate are particularly flourishing just now, or that they are to be burdened with a new liability which will indefinitely postpone the time when there may be a surplus to be devoted to that advancement of science which Sir Thomas Gresham had in view in forming Gresham College? It is not long since some of the bonds, which represent money borrowed on the Gresham Estate for the building of the Royal Exchange, were advertised for repayment out of its surplus annual income, which afforded a hope that a good time for the scientific part of Sir Thomas Gresham's bequest might be drawing nigh. The public would be glad to know whether this hope is to be falsified or not. W. B.

January 20

Siwalik Carnivora

May I ask space to thank your correspondents for their answer to my previous inquiries concerning collections of Siwalik fossils in England, (many of which I have not yet had an opportunity of visiting,) and to add that I am now about undertaking the description of Siwalik Carnivora for the Indian Government? All remains of this order are very scarce, and in general fragmentary, and every specimen is, therefore, important. If any specimens exist in any provincial collections, I should be very glad of any information regarding them, and if possible of the opportunity of describing them in my forthcoming memoir. Any specimens sent to me, to the care of Dr. H. Woodward, F.R.S., British Museum (Natural History), Cromwell Road, S.W., will be thankfully received, and duly returned after comparison and description, if necessary. RICHARD LYDEKKER

The Lodge, Harpenden, Herts, January 17

Earthquakes

EARTHQUAKE phenomena are extremely rare in this highly favoured part of the world; but we had a very decided shake near the close of the year 1882. It occurred last night (Sunday, December 31) at about five minutes past ten o'clock, Halifax time, as nearly as can be determined at present. My observation was made at Lucyfield, ten miles north from the city of Halifax; the house stands on a rounded hill formed of unaltered drift, overlying slate rock, and at an elevation of about 350 feet above sea-level.

The air was perfectly still. There was a sudden rumble as of heavy waggons on a hard road at some little distance, then the sound became louder, I may say deafening, as of heavy loaded waggons running close to the wall; or of a heavy railway train running through a reverberating cutting; then the noise seemed overhead as if caused by rolling heavy furniture on the upper floor; there was a slight vibration of the building, as if something large and heavy had struck the roof, icicles fell from the eaves, fragments of plaster fell down behind the lathing of the walls, and there was a sound like a sudden gust of wind upon the windows and walls outside (there was no wind however). Suddenly noise and vibration ceased, and all was perfectly still. Passing outside, to look for some cause for these remarkable phenomena, nothing particular was noticeable. The country was covered with a thick white mantle of snow, the air was perfectly calm, there had been no rain drops, nor hail, but a faint flash of lightning (unaccompanied by thunder) occurred about a minute and a half or two minutes after cessation of the shock.

For two or three days prior to the shock, the temperature did not fluctuate much. The thermometer stood at zero centigrade (32° F.) at sunset on the 31st and has not varied much since; it was within a degree of the same at sunset of the previous day, but went down at night, rising again in the morning. During the day (31st) the sky was clear with some light fleecy clouds, wind northerly, but veered round to south-west about sunset, and the sky became overcast with clouds; later the clouds seemed to clear away, but the air became foggy, and was so at the time of the shock. (About Truro I am informed the sky was "clear and starry.") The air had been in a highly electric state during the afternoon and evening.

The earthquake shock lasted, as nearly as I can compute from recalling circumstances, something less than a minute, certainly more than half a minute, but probably not more than a whole one. I cannot indicate with any degree of certainty the direction of oscillation; so far as a retrospect of circumstances and sensations indicate, the apparent movement was from south-west to north-east.

Most persons in the city of Halifax to whom I have spoken to day observed the shock more or less distinctly, but it does not appear to have been nearly as violent in the city as in some other places. I ascertained from the conductor of the morning railway train that the shock was felt more or less severely all along the journey traversed by his train this morning, viz. from Truro to Halifax, a distance of sixty-one miles. At Shubenacadie, nearly thirty miles from my point of observation, flower-pots in the railway station house were toppled over on the window-sill and rolled upon the floor.

I have jotted down these particulars, thinking they may possibly prove of some interest if compared with the observations of others at different points. GEORGE LAWSON

Dalhousie College, Halifax, Nova Scotia, January 1

A SHOCK of earthquake was felt in this district on Tuesday, January 16, about 5 p.m. Comparatively few persons perceived it, but to those who did it was a striking phenomenon. The following report has been handed to me by a trustworthy observer:—

"About 5 p.m. on Tuesday, January 16, I was standing in a room, leaning against the foot of an iron bedstead, and facing a window, in front of which, on a table, was a cage containing one of the small African parrakeets known as love birds. The room was perfectly quiet, when this bird, which had settled itself for the night, surprised me by craning out its neck and flattening its plumage with every appearance of alarm, without any sound or movement on my part, or anything in the room which could possibly have frightened it. Immediately afterwards the iron bedstead I was leaning against, as well as the floor, trembled sufficiently to make me wonder what on earth was going on, especially as I heard no sound sufficient to account for it. The trembling ceased in a few seconds, and, while I was still wondering, returned in a greater degree than before, lasting this time about five seconds. The feeling I experienced was similar to that of standing on a bridge while a load was passing over. The second time I speedily came to the conclusion that it was caused by an earthquake." GEORGE F. BURDER

Clifton, January 20

I SHALL feel obliged if you will put on record in your columns that an earthquake was felt at Hastings by my sister and myself in separate rooms, on Tuesday morning last, the 16th inst., at 9½ minutes past 9 a.m. The undulations were between E.S.E. and W.N.W., and lasted about 4 seconds.

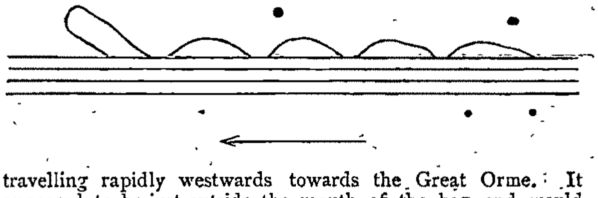
R. H. TIDDEMAN

H.M. Geological Survey, 28, Jermyn Street, S.W., Jan. 20

The Sea Serpent

BELIEVING it to be desirable that every well-authenticated observation indicating the existence of large sea serpents should be permanently registered, I send you the following particulars.

About three p.m. on Sunday, September 3, 1882, a party of gentlemen and ladies were standing at the northern extremity of Llandudno pier, looking towards the open sea, when an unusual object was observed in the water near to the Little Orme's Head, travelling rapidly westwards towards the Great Orme. It appeared to be just outside the mouth of the bay, and would therefore be about a mile distant from the observers. It was watched for about two minutes, and in that interval it traversed about half the width of the bay, and then suddenly disappeared. The bay is two miles wide, and therefore the object, whatever it was, must have travelled at the rate of thirty miles an hour. It is estimated to have been fully as long as a large steamer, say 200 feet; the rapidity of its motion was particularly remarked as being greater than that of any ordinary vessel. The colour



appeared to be black, and the motion either corkscrew like or snake-like, with vertical undulations. Three of the observers have since made sketches from memory, quite independently of the impression left on their minds, and on comparing these sketches, which slightly varied, they have agreed to sanction the accompanying outline as representing as nearly as possible the object which they saw. The party consisted of W. Barfoot, J.P. of Leicester, F. J. Marlow, solicitor, of Manchester, Mrs. Marlow, and several others. They discard the theories of birds or porpoises as not accounting for this particular phenomenon.

F. T. Mott

Birstal Hill, Leicester, January 16

A Novel Experiment in Complementary Colours

The old maxim of an *adjacent gray* in order to give visibility to a complementary colour seems to hold its ground. Mr. Charles T. Whitmell puts it very clearly when he alludes to "the advantage of a reduction of brightness to a level comparable with that of the existing colour."

Mr. Whitmell will find, I think, that this brightness may be still further reduced below the level of the existing colour. This may be shown by one or two remarkable experiments with light admitted through a small needle hole the one-fiftieth of an inch in diameter, made through the bottom of a half ounce pill-box painted inside with lampblack. On placing a sheet of white paper on the table at night in a room lighted with ordinary gas, and looking through the small hole with one eye; *both eyes being open*, he will see on the paper a disc of a beautiful cobalt *blue* colour, evidently the complementary of the yellow light of the gas. On examining the sky in the same way in the morning, there will be seen, especially if the weather is dull and hazy, as it has been of late, a disc of a *primrose yellow* colour, the complementary of the blue sky, which, although invisible, is still making its impression on the sensitive retina. Later on in the day, between five and six o'clock, when the weather is murky, the disc has a well-marked *pink* colour, the atmosphere being evidently tinged with dark green. These several results I have witnessed from day to day for the last fortnight, and they have been verified by others to whom I have shown them. But when the sky is very blue and clear, there is seen, for obvious reasons, a blue disc only.

In the above experiments there is the curious anomaly of having one eye impressed with the exciting colour, the other with its complementary.

John Gorham

Bordyke Lodge, Tunbridge, January 20

The Projection of the Nasal Bones in Man and the Ape

In my letter in the last number of Nature (p. 266) the walls of the human nose were carelessly ascribed to the elevation of the *pre*-maxillary bones. This is not the case. It is only in the ape and lower animals that the ascending processes of the premaxillary assist in forming the external nose-case, or muzzle, *above* the nostrils. The frame-work of the nose in the lower types of the negro seems, therefore, in this respect, to differ more from the nose-case of the ape, than it does owing to any great development of the nasal bones.

I take this opportunity to mention that the woodcut of the embryo, which I referred to, appeared first in Quain's "Anatomy." Also, the quotation about the nasal bones of the orang, I have since found from my notes, to have been derived from Prof. Mivart's "Man and the Ape."

January 22 J. Park Harrison

HOVERING OF BIRDS

This problem, to account for the phenomenon of the motionless hovering of hawks and other birds in mid-air, was the subject of correspondence in Nature, vol. viii. pp. 86, 324, 362; vol. ix. p. 5; vol. x. pp. 147, 262: vol. xi. p. 364. The only plausible explanation advanced (by Joseph Le Conte, vol. ix. p. 5, and previously by the present writer, vol. viii. p. 362) was that the birds take advantage of slant upward currents of wind sufficiently strong to neutralise the force of gravity. But the arguments brought forward in support of this explanation were perhaps not quite conclusive, for lack of a sufficient series of observations.

During the past six years I have noted such instances as I have chanced to witness in the course of a wandering occupation, and now offer the results as a further contribution towards the solution of the question.

I may state at once that in every case where I have seen a bird hovering, the following three conditions have obtained:—

(1) There was a fresh wind blowing.

(2) The bird was facing the wind.

(3) Beneath the bird there was a steep slope of ground facing the wind.

The particular localities in which I have observed the phenomenon are the following:—

(1) 1877, *September* 17.—Driving from Aberayron to Llanrhystyd (Cardiganshire). Wind W.N.W., moderate. Cliffs facing N.W. Gulls under cliff top, below road, in poise. Hawk under hill top, above road, in perfect poise.

(2) 1877, *October* 13.—Approaching Llantrisant town (Glamorganshire) from Llantrisant Junction. Wind S.W., moderate. Hawk over S.W. slope, barely poising, partly fluttering, tail plainly brushed up.

(3) 1877, *October* 14.—Llantrisant (Glamorganshire). Wind S.S.W. Rooks upborne, above S.S.W. slopes of hill with entrenched-fort (Caerau).

(4) 1877, *October* 20.—Cliff facing S. between Longland and Caswell Bays, Gower (Glamorganshire). Gull and crows upborne. Wind moderate, S.S.W.

(5) 1877, *October* 21.—Cefn Bryn, Gower (Glamorganshire), facing S.S.W., climbing from Reynoldston. Rooks upborne. Wind strong, S. by W.

(6) 1879, *October* 17.—On road from Llantrisant to Pontypridd (Glamorganshire). Wind W. Rooks upborne over slopes facing W.

(7). *October* 28.—Killay, near Swansea (Glamorganshire). Hawk poised above hill-side facing N.E., to the west of Killay railway station. Wind N.E. I was almost under the bird, and could see the conduct of wings and tail suiting the ripples of wind.

(8) 1879, *November* 5.—Near Merthyr Tydfil (Glamorganshire). Hawk poised over N. slope of hill above (to S. of) tunnel on Merthyr-Abernant Railway. Wind N.

(9) 1880, *March* 13.—Near Penally (Pembrokeshire). Sea-gulls, rooks, and jackdaws upborne and floating with wings outstretched all along cliff line facing S., between Penally and Lydstep. Wind S., full on cliff from the sea. Gulls up to 200 feet above cliff edge. At greater height and inland, they were flapping. Different behaviour of rooks over inland northern slope.

Further on, over caves at north end of Lydstep Sands, hawk poised for 1 min. and 1½ min. at a time, just over cliff line, in teeth of wind off sea.

(10) 1880, *March* 17.—Near Cardiff (Glamorganshire). Hawk poised about 10 or 12 feet above railway embankment facing E.N.E. (20 or 25 feet high) of Llandaff and Penarth line, near Ely Station. Wind E.

(11) 1880, *March* 27.—Gulls uplifted over E. scarp of Beachy Head Down (Sussex). Wind E.N.E.

(Same day)—Over N.E. slope of Lighthouse Down. Bevy of eight gulls, all in perfect poise, immediately over edge of cliff.

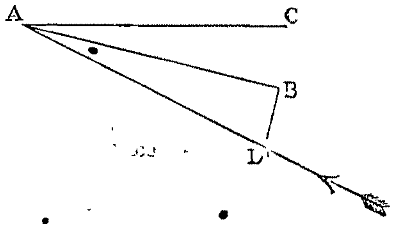
(12) 1880, *August* 8.—Wells next-the-sea (Norfolk). Wind N.W. Hawks poising over W. slope of sea-wall, and over N.W. slope of sand-hills (projecting from the main line of dunes that runs east and west), and trying unsuccessfully over railway embankment which runs N.W. and S.E.

In several of the instances here recorded I was near enough to see that the bird was delicately adapting the slope and spread of its wings to the momentarily varying inclination and force of the wind. Among the sand-hillocks near Wells, on the Norfolk coast, I succeeded in approaching, under cover of ridges and long grass, within about ten yards of a hovering hawk, and saw the posture of the bird very well for a few seconds, till he became aware of my presence and dashed away. I was much struck by those instances in which the obstacle that caused the upward slant of the wind was only a sea-wall or a railway embankment, and especially by the critical case (No 12) where the bird was evidently baffled because the wind lay along the embankment, not against it, and therefore gave no upward current.

My list includes four cases (3, 4, 5, 6) of rooks and gulls "up-borne" on outspread wings, under conditions similar to those present in hovering—cases that could not be explained by any theory of *vis viva*, but clearly involved an external mechanical force, which could only be that of the wind, sustaining and uplifting the birds. The close relation between the "up-borne" and the "hovering" action was evident in case (9) where the gulls, &c., were up-borne and sailing, while the hawk was poised and motionless.

These observations, as far as they go, appear to indicate plainly the law which governs the phenomenon in question. I think they strongly confirm the theory already advanced, that the bird in hovering is upheld by a slant upward current of air. A strong wind pressing against a slope of ground is necessarily thrown into a slant upward current, "as slopes a wild brook o'er a hidden stone." There may be a downward eddy if the slope is precipitous, as one may often feel at the foot of a high wall, but the main stream of the air for some considerable height above the slope is forced to take an upward slant, with increased velocity, in order to surmount the obstacle in its path.

Given such a slant upward current, it is easy to see



that a bird, with the exquisite muscular sense that every act of flight demands and denotes, might so adapt the balance of its body and the slope of its wing-surface to the wind as to remain motionless in relation to the earth. The slope of the wing-surface should divide the angle between the horizontal and the direction of the slant wind-current in such proportion that, if the air were at rest, the bird, under the action of gravity, would float forwards, downwards, on outspread wings, with exactly the same velocity as that of the wind (in which it remains motionless) and in exactly the opposite direction. The mechanical conditions are identical in the two cases, whether we consider the air at rest and the bird floating through it, or the bird at rest and the wind rushing under it. In either case the bird has reached, and maintains, its maximum velocity, due to gravity, compatible with the resistance of the air, which resistance is the same in both cases.

I have heard it objected that the mechanical conditions are not the same in these two cases, because in the one case the bird has momentum, in the other not. Need it be said that momentum is a purely relative possession, just as velocity is? In each case the bird has the same velocity, and therefore the same momentum, relative to the air. The mechanics of the situation, as between bird and air, are not affected by the possession or loss of velocity (and with it momentum) relative to the earth.

Perhaps the feasibility of the thing may be best shown by a simple diagram. Let A B represent the slope of the bird's wing (viewed from the right side), dividing the angle between the horizontal, A C, and the direction of the wind, D A. Draw B D at right angles to A B, and take A D to represent the force of the wind. Then D B and B A will represent the force of the wind resolved perpendicular and parallel to the slope of the wing A B. The resolved part, B A, meeting only the resistance of the bird's head and shoulders and front edge of the wings, tends not strongly to push the bird in the direction, B A, that is, backwards and a little upwards. But the resolved part, D B, which meets the full area of the outspread wings and tail, tends powerfully to push the bird in the direction D B, that is, upwards and a little forwards. Then all that is required to keep the bird at rest is that the effect of the forward force exerted by D B should balance the effect of the backward force exerted by B A (both being resolved vertically and horizontally), and that the great upward force exerted by D B, together with the small upward force exerted by B A should exactly neutralise the downward force of gravity.

The only difficulty in the way of the slant-upward-current theory lies in the statement of the Duke of Argyll (NATURE, vol. x. p. 262) that "a hundred times" he has seen birds hovering "when by no possibility could any upward deflection of the wind have arisen from the configuration of the ground." My own observations testify so consistently in favour of slant upward currents that I feel justified in asking for more precise information concerning the instances alluded to by the Duke of Argyll, before relinquishing the theory which I hold. Wherever I have seen a hawk trying to remain in one position over a plain or slightly undulating ground, the feat has only been accomplished by continued vibration of the wings.

The problem of the "soaring" of birds introduces other conditions, which require separate consideration, though I believe it will be found that the two phenomena of "soaring" and "hovering" depend upon essentially similar causes.

(By the bye, does not the provincial name of one of the hawks, the "Windhover," record the constantly observed dependence of the act of hovering on the wind?)

HUBERT AIRY

THE LATE EDWARD B. TAWNEY

By the death of this young naturalist English geology has lost one of its most enthusiastic and cultivated students. Hardly beyond the threshold of his career, he had already gained for himself a notable place among the geologists of this country, and his friends augured for him a future of distinction and usefulness. But in the fulness of his promise and in the midst of his work he has been struck down so suddenly that few of his friends knew he had been ailing until they were shocked and saddened by the news of his death.

Born in 1841, he was the third child of the Rev. Richard Tawney, Vicar of Willoughby, Warwickshire, who had gained a distinguished place at Rugby, and had been a Fellow of Magdalen College. On the death of his father, young Tawney was placed under the care of his guardian, Dr. Bernard of Clifton, and received his early education there. During these years he seems to have acquired a bent towards natural science mainly through the influence of Dr. Bernard and Dr. Fox of Brislington. He was eventually enabled to gratify this inclination by attending the courses of instruction at the Royal School of Mines, Jermyn Street, from 1860-63, where he greatly distinguished himself. He gained a Royal Scholarship, Duke of Cornwall's Scholarship, the De la Beche Medal for Mining, and the Edward Forbes Medal for Natural Science, and took Associate's diplomas in the Mining and Geological divisions.

With the training in scientific methods thus obtained, he soon betook himself to original research, gaining experience by excursions at home and by travel abroad. In 1872 he accepted the offer of Assistant Curator of the

Bristol Museum. With characteristic energy he at once set to work, re-tableting. re-arranging, and naming the geological collection, taking care to have gaps in the series filled up, and making the museum really serviceable for purposes of instruction. Six years later, in the early part of 1878, he received the appointment of Assistant Curator of the Woodwardian Museum, Cambridge. He soon made his mark there, as was acknowledged in the following year by the bestowal upon him of the honorary M.A. degree. His indefatigable industry and wide range of acquirements so peculiarly fitted him for this position, that his death must for some time to come be an almost irreparable loss to the University.

Looking over his published papers one cannot but be struck with his versatility. At one time we find him discussing the Rhoetic beds of South Wales, at another dealing with that vexed question of Alpine geology—the position of *Terebratula diphya*. From Devonian fossils he passes to the description of new species of Oolitic gasteropods, or to the Cretaceous Aporrhaidæ, or to Palæozoic star-fishes. He could enter minutely into the stratigraphy of the Isle of Wight Tertiary strata, and with not less energy and clearness of insight described the microscopic structure of the crystalline rocks of Wales. Well versed in the Continental languages, he kept himself abreast of the foreign progress of his favourite science. Nor were his tastes wholly scientific. He delighted in Piers Ploughman and the Niebelungenlied. What he might have done who may guess? That with his feebleness of constitution he should have been able to accomplish so much, shows how ardent was his love of nature and how indomitable his spirit of inquiry. His devotion to truth and abhorrence of everything savouring of insincerity or sham led him to speak out freely and uncompromisingly. But no one could mistake the honesty of his purpose. A. G.

REMARKS ON AND OBSERVATIONS OF THE METEORIC AURORAL PHENOMENON OF NOVEMBER 17, 1882

THE interesting meteoric phenomenon seen in England during the aurora of November 17 last, has induced me to endeavour to find the true path of that object. Though I have spent much time in applying the method given by Prof. E. Heis in his "Periodischen Sternschnuppen," I have got no farther than the point to which Mr. H. D. Taylor has brought us, the observations being in no way capable of combinating. In fact, when seeking the lines of intersection, formed by the different planes of the great circles, wherein the apparent path was seen, with the mean horizon (say the plane of a common map), these lines have but little tendency to converge to the same point. Therefore the method of Mr. Taylor seems to me the most convenient. When the object has followed a straight line, all the places where it was seen passing just before the moon, must lie in a plane containing the true path and the moon. This plane must cut the plane of the map in a straight line. Now the four places where observers saw the meteor before the moon's disc are:—Woodbridge, near Ipswich, Lincoln's Inn Fields (London), Windsor, and Ramsbury, near Hungerford, fulfilling, by no means, the above-mentioned condition. Nevertheless the most probable direction of this line seems to be that accepted by Mr. Taylor, N.E. by E -S.W. by W. (astronomical), because this is the general direction of the lines of section, given by the great circles, mentioned above. Here it is to be remarked that when the meteor was seen from S.E. to S.W. (as in the case at York), but at some height (here 10°) above the horizon, the intersections of the apparent path with the horizon may lie near E. and W. (here, according to the observation of the meteor passing 6° below the moon, at 12° south of E.). We give here bearings as seen from the different places, taken directly from the communications, or deduced indirectly from them:—

Street (3° south of Leeds)	S.E.–S.W.
Clifton (Bristol)	E. 18° N.–W. 18° S.
Greenwich	E.N.E.– (?)
Guildown (p. 149)	E.–W. (nearly)
Bedford	S.E.–S.W.
Clevedon (p. 100)	N. 70° E. (?)–S. 70° W.
Cambridge	E.–S.S.W.
York (H. D. Taylor)	E. 12° S.–W. 12° N.
Woodbridge	E. 10° N.–W. 10° S.
Windsor	E.–W.
Coopers Hill	?–S.W.
Ramsbury	?
Lincoln's Inn Fields	E.–W.

Now we can add to these English observations[1] two others made in the Netherlands.

1. Prof T. A. C. Oudemans gives in the *Utrecht Newspaper* (No. 318) the following (translated) description:—"At 6h. 23m. (6h. 2½m. Greenw. T.) a feather-like appearance, resembling in the beginning a brilliant comet, formed suddenly in the eastern part of the heavens, the end being just before Aldebaran. Within two minutes this feather had prolonged itself above Saturn, through the Pegasus quadrate, and south of the three Eagle-stars, the east or following end shortening, while the other or preceding end advanced. . . . When this arch had obtained the length of 90° (which lasted but a few seconds) a separation was made in the middle of its length, where the arch had a breadth of about 3°. This separation had a length of about 10° and a breadth of ½°, and was pointed at the ends. At 6h. 25m. this arch disappeared wholly in the west." Prof. Oudemans says further that the great circle of the apparent path intersected the equator at 110° and 290° of right ascension. This gives me, combined with the position of Aldebaran, a direction in the horizon of E. 20° N.–W. 20° S.

2. Mr. P. Zeeman observed the same phenomenon at at Zonnemaire, near Zierikzee (51° 42′ lat. and 57′ W. Amsterdam). He wrote me the following on November 19 and 24:—"About 6h. 20m. (I saw) a magnificent, splendid white arch, beginning a little north of east, and prolonging itself to south-west, but in the meantime shortening at the east end and disappearing in a very short time." Mr. Zeeman declares in his second letter that this arch went through Aldebaran, and through α Pegasi. This gives me a horizontal bearing of E. 20° N.–W. 20 S, as the observations of Prof. Oudemans gives also.

Thus we find these two Dutch observations (unhappily the sky in Groningen had just, at 6h. 1m. Greenw. T., got cloudy, the aurora being very splendid before) supplement and confirm the greater part of the English observations. Only the phenomenon seems to have been of greater apparent size, and therefore nearer to the observer. The separation by an obscure streak seems not to have been visible in England, perhaps from the change of its relative position.

The conclusion to which we come after all, regretting earnestly the want of French observations, is that we have here probably a meteoric object, moving, according to the calculations of Mr. H. D. Taylor (vol. xxvii. p. 140), with great velocity through the upper strata of the atmosphere and at the same time of auroral character, as the spectrum observation of Mr. Rand Capron (vol. xxvii. p. 84), makes out beyond any doubt. The separation and the feather-like forms, observed at Utrecht, make it probable that it was a mass of meteoric dust, passing through our atmosphere like an accumulation of little shooting stars. In this way the phenomenon of November 17 brings a confirmation of my own theory of auroræ, proposed by me in the "Appendice alle Mémorie della Società degli Spettroscopisti Italiani," 1878, vol. ii, and received with sympathy by many of the German and Dutch astronomers; but as it seems little known in England, though referred to by Mr. Rand Capron on p. 64 of his beautiful work, "Auroræ." In this theory most of the properties of auroræ are deduced from cosmic dust entering into the atmosphere of the earth. I take the liberty to direct attention to the unexpected argument, that the brilliant object of November 17, 1882, has brought forth in favour of my "Théorie Cosmique," to which I had already the opportunity to refer in this Journal in my article "On Dust, Fogs, and Clouds" (vol. xxiii. p. 195).

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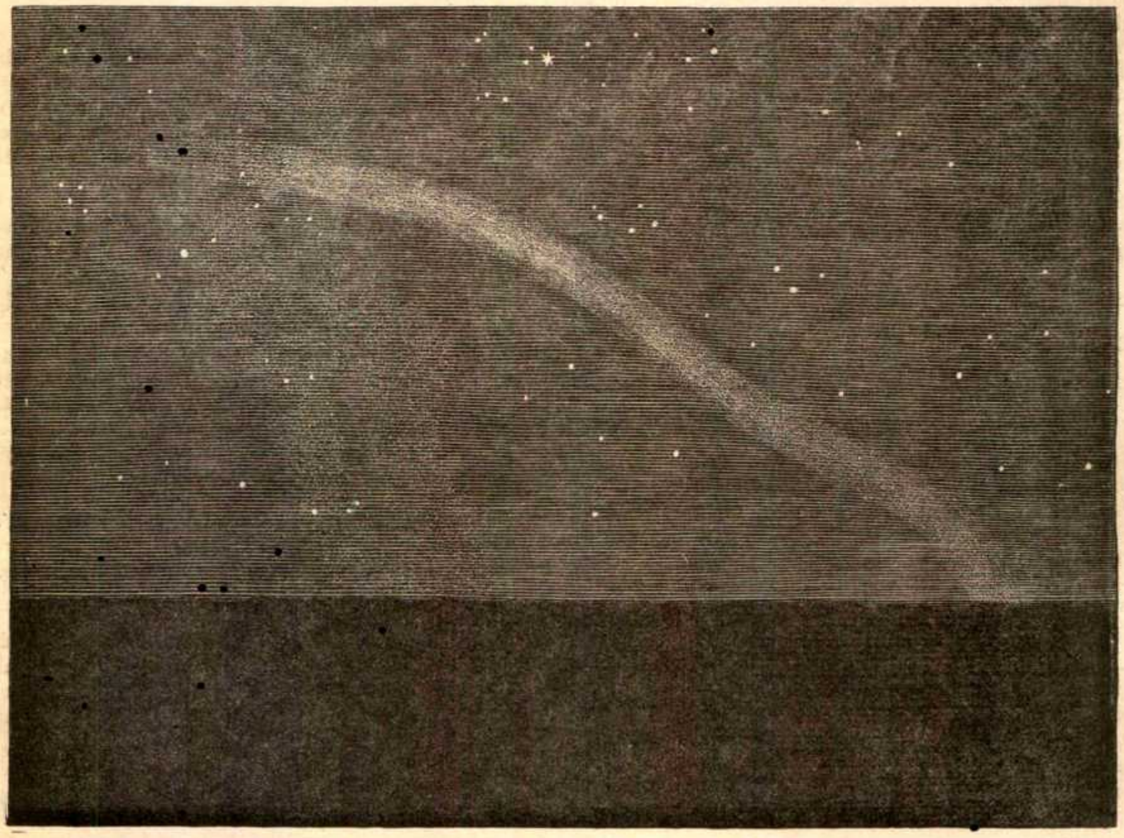
[1] Will Mr. W. M. Flinders Petrie be so kind as to tell us where we can find the Swedish observation mentioned by him, vol. xxvii. p. 140?

Furthermore, I think that this object is not the only example of such a phenomenon. On November 2, 1871, there was seen in Groningen and several places of Germany a strange, brilliant arch, striped parallel to its well defined sides and changing its curve during its two hours of existence. The beginning of the phenomenon (of which I gave a description in the Dutch journal *Isis*) was seen by a student, Mr. Gratama, like an elliptic patch of light round the Pleiades. Dr. Vogel, who observed the same arch at Bothkamp, determined its auroral character by the spectrum. Otherwise it resembled very much the bright spur of a gigantic meteor or fireball. Also it disappeared slowly, beginning at the east end, as the illustration shows. A faint aurora, with dark segment, was visible in the north. The height of this arch was calculated by me approximately at 127 kilometres or 79 miles. I think that the only difference between these two feather-like phenomena of November 2, 1871, and of November 17, 1882, consisted in the different apparent velocity and in the greater mass of meteoric dust, forming in the case of November 17, 1882, but a short, and in that of November 2, 1871, a very long train of incandescent matter. It must be remembered here that the tails of great fireballs remain visible for half an hour or more (see *e.g.* the article of Mr. Branfill, vol. xxvii. p. 149). In NATURE, vol. xii. p. 330, is to be found a description of similar arches, seen at Fremantle in Australia by Mr. Lefroy, in presence of the moon, which was obscured by one of them.



Auroral Arch, observed November 2, 1871, at Groningen (Netherlands).

This leads us to a question, touched by Mr. Backhouse, NATURE, vol. xxvii. p. 198, that of the halos seen in Siberia (by Von Wrangel, I believe), when an auroral beam was in front of the moon. I watched in vain if such an event should perhaps occur November 17 last, but Mr. Zeeman, whom I have cited above, seems to have been so happy as to have seen a white and bright auroral cloud floating over the moon's disc at 5h. 47 (local time), giving the common interference phenomena. It is unnecessary to remark, that these phenomena can be formed by all kinds of dust, formed of nearly equal particles, and that they in no way require ice-particles. On my inquiry why the observer could decide that it was not a common cloud, he brought forward the following arguments:—(1) Its great brightness; (2) its transparency to the starlight; (3) its very great velocity, unusual in common clouds.

Returning to the meteoric phenomena, visible simultaneously with auroræ, it seems that such phenomena were seen during the marvellous aurora of January 7, 1831, described in *Poggendorff's Annalen* of the same year. We read (p. 440) that Bergrath Senff, in Colberg, at 6.30 o'clock, saw above the west horizon a bright yellow streak, rising upward with a common cloud-velocity, passing at 30° N. Zen. D., and forming an arch from W. to E., beginning to disappear from the west end, almost at the same moment that it reached the east horizon. At p. 458 we see that Prof. Rudberg, at Upsala, December 7, 1830, saw a very bright patch of double the dimensions of the moon's disc, moving with great velocity behind the common auroral beams. Further, Prof. Bischoff, in Burgbrohl (p. 461), observed, on the occasion of the aurora of January 7, 1831, a moving cloud as bright as the milky way, from E. to W., in five minutes. Prof. Moll saw, in Utrecht a similar object, rising from N.E., through the Pleiades, to S.E. (S.W.?). Similar observations are to be found during the same aurora, p. 471 (one advancing arch), p. 472 (four similar arches, and a dark streak).

In several articles on the aurora of November 17, 1882, the height of auroræ is spoken of. Mr. W. M. F. P. (p. 173) says that the strange object observed is *physically impossible* to auroral nature, because of its height of about 170 miles. It was already observed by Mr. Backhouse that auroræ are often observed at very great heights. The same is also the case with shooting stars. I take the liberty to refer once again to an article of mine in this journal entitled "The Height of the Aurora," where I refer to the beautiful determinations by Prof. Heis and Dr. Flögel, published in the *Zeitschrift der Oesterr. Gesellsch. f. Meteor.*, vii. p. 73. The heights were found from 10 to 100 geogr. miles (46 to 461 Engl. miles). Dr. Sophus Tromholt found, besides apparent low heights of some auroræ in Norway, the height of that of March 17, 1880, to be 17 geogr. miles ("Wochenschrift redigirt," von Dr. H. J. Klein, 1880, p. 172). Prof. Galle of Breslau calculated by his method, described in the *Zeitschr. f. Met.*, vii. p. 73, and in the *Astr. Nachrichten*, Bd. 79, No. 1882, 40 to 60 geogr. miles, and I found for the great aurora of May 13, 1862, 59 geogr. miles.

H. J. H. GRONEMAN

Groningen (Netherlands), January 14

NOTES

THE Council of the Institution of Civil Engineers have arranged for the delivery at the Institution of a series of six lectures, on the Applications of Electricity, on the following Thursday evenings, at 8 o'clock:—February 15—The Progress of Telegraphy, by Mr. W. H. Preece, F.R.S., M.Inst. C.E. March 1—Telephones, by Sir Frederick Bramwell, F.R.S., V.P.Inst., C.E. March 15—The Electrical Transmission and Storage of Power, by Dr. C. William Siemens, F.R.S., M.Inst. C.E. April 5—Some Points in Electric Lighting, by Dr. J. Hopkinson, F.R.S., M.Inst. C.E. April 19—Electricity applied to Explosive Purposes, by Prof. F. A. Abel, C.B., F.R.S., Hon. M.Inst. C.E. May 3—Electrical Units of Measurement, by Sir W. Thomson, F.R.S., M.Inst. C.E. This is an excellent step which the enterprising Institution has taken, and we are sure will be productive of good both to science and to engineering.

MR. ERNEST H. GLAISHER, B.A., Trinity College, Cambridge, has been appointed Curator of the British Guiana Museum, George Town, Demerara.

MR. W. H. WHITE, one of the Chief Constructors to the Navy, has resigned his position to take up a managerial appointment in the firm of Sir Joseph Whitworth.

AN interesting boring through the chalk is now about to be resumed at Southampton. At the last meeting of the British Association a paper by Mr. T. W. Shore and Mr. E. Westlake on the Artesian well on Southampton Common was read in the Geological Section. The Town Council has now accepted a tender for continuing the boring which was abandoned in 1851, after a depth of 1317 feet had been reached. The boring was then passing through the lower chalk or chalk marl, and we believe it is now intended to continue it to the Lower Greensand. The well at the bottom of which the boring commences is 563 feet deep, and this was reopened last week, after having been closed for thirty-two years. Some observations on the temperature of the water were at once made by Mr. T. W. Shore and Mr. J. Blount Thomas, of Southampton, for the Underground Temperature Committee of the British Association. By means of a heavy elongated sinking weight and a registering windlass, a thermometer was passed down the bore shaft to a depth of 1210 feet, when it was stopped by chalk mud. An outer case which was attached to the sinking weight was much scratched in passing through the Upper Chalk. A temperature of $71^{\circ}\cdot9$ F. was registered at the bottom, that of the outer air being 49° F.

THE City of Neuchâtel celebrated in the beginning of the present month the fiftieth anniversary of the foundation of its Natural History Society. The leader among its founders, who first met for the purpose on December 6, 1832, was Louis Agassiz.

THE biennial Hunterian oration will be delivered on Wednesday, February 14, at three o'clock, by the President of the College of Surgeons, Mr. Spencer Wells, in the theatre of that institution. The biennial festival will be given in the library the same evening, to which the president and vice-presidents have, as usual, invited several distinguished visitors.

THE Pontifical Academy of the Nuovi Lincei have appointed a Committee to take steps for the erection of a monument in Rome to the late eminent astronomer, Father Secchi. The monument will be of a meteorological character. The sculptor Prinzi has already made a model which combines convenience for arranging the meteorological apparatus with features recalling the work of Father Secchi. The statue of the astronomer crowns the monument, and among other emblematical figures will be one of Meteorology holding in one hand a gigantic barometer, which can be seen from a great distance, and another of Physics holding up to view an equally large thermometer.

THE rumour that the fragments of the unfortunate Mr. Powell's balloon have been found in the Sierra del Pedroso, in the far south of Spain, is too vague and incredible to deserve much attention.

AT the meeting of the Essex Field Club, to be held on Saturday evening next, January 27, the attention of the members and the public generally will be directed to the Bill about to be introduced into Parliament for the construction of a line of railway from Chingford to High Beach. In January, 1881, the Club, in conjunction with other Natural History Societies in and around London, strongly protested against any portion of Epping Forest being occupied by a Railway or other Company, to the prejudice of the provisions of the Epping Forest Act, and certainly no sufficient arguments or expressions of public opinion have since been brought forward in favour of the scheme. It is believed that the proposed line is quite unnecessary, as no part of the forest is more than two miles from a railway station, and moreover a railway and its concomitants could not fail to destroy the chief interest and charm of the district—its seclusion and naturalness; qualities of inestimable value so near a large city.

THE following papers are set down for reading at the meetings of the Society of Arts during the part of the Session after Christmas:—At the Ordinary Meetings—W. K. Burton, The Sanitary Inspection of Houses; General Rundall, The Suez Canal; Prof. Thorold Rogers, M.P., Ensilage in the United States; Sir Frederick Bramwell, F.R.S., Some Points in the Practice of the American Patent Office; J. H. Evans, The Modern Lathe; A. J. Hipkins, The History of the Pianoforte; Prof. George Forbes, The Electrical Transmission of Power;

D. Pidgeon, Recent Improvements in Agricultural Machinery; Wilfred Cripps, F.S.A., English and Foreign Silver Work, with some Remarks on Hall-marking. In the Foreign and Colonial Section—Edmond O'Donovan, Life among the Turkoman Nomads; Rev. J. Peill, "Social Conditions and Prospects in Madagascar; Robert W. Felkin, Egypt: Present and to Come; W. Delisle Hay, Social and Commercial Aspects of New Zealand. In the Applied Chemistry and Physics Section—C. F. Cross, F.C.S., Technical Aspects of Lignification; Walter G. McMillan, F.C.S., Chemical Means for Preventing or Extinguishing Fires; W. N. Hartley, F.R.S.E., Self-purification of River Waters; R. W. Atkinson, B.Sc., The Formation of Diastase from Grain by Moulds; James J. Dobbie, D.Sc., and John Hutchinson, On the Application of Electrolysis to Bleaching and Printing. In the Indian Section—Charles H. Lepper, Overland Commercial Communication between India and China, *viâ* Assam; W. S. Seton-Karr, Agriculture in Lower Bengal, with some Notice of Tenant Right, &c.; J. M. Maclean, Private Enterprise in India; C. Purdon Clarke, Some Notes on the Domestic Architecture of India.

WE have received from Egypt a publication of some interest in the shape of the *Bulletin* of the Chemical Laboratory at Cairo, directed by M. Albert Ismalun. The laboratory is under the Department of Public Works, and judging from the report in the *Bulletin* is doing a fair amount of useful work. The laboratory has been recently much improved, and attached is a museum of specimens in geology, palæontology, and zoology.

A CONSIDERABLE number of names has been added to the list of those who are unfavourable to the meeting of the British Association in Canada in 1884. The request of the protesters to the Council seems to us quite reasonable,—"that it is highly desirable that you should take some further steps in order to ascertain the general feeling of the members of the Association upon the subject, before allowing our kind and liberal friends in Canada to incur any further trouble or expense" There are 141 names appended to the circular, and while some of them are well known, still we note the absence of some of the leading representatives of English science.

THE proceedings at the meeting of the Association for the Improvement of Geometrical Teaching were not carried out quite on the lines laid down in a recent number of NATURE (vol. xxvii. p. 247). In consequence of a delay in the delivery of the copies of "the Elements of Plane Geometry," the President was obliged to defer the moving of his resolution till the next meeting, which, it is hoped, will be held about Easter next. The members were also informed that Mr. Levett had, in answer to an appeal made to him, consented to retain office as Hon. Secretary for the present year. Mr. E. B. Sargant, Trinity College, Cambridge, was elected to act as joint secretary with Mr. Levett. The following members were elected: Miss Burstall, Professors G. C. Foster, F.R.S., W. H. H. Hudson, H. Lamb, and G. M. Minchin, Rev. A. Jamson Smith, and Messrs. G. Griffith, E. B. Sargant, Charles Smith, F. Turner, and H. H. Turner.

DR. PEVERATI, director of the Meteorological Observatory of Cassine, states that an earthquake shock was felt there on January 16, at 7.42 a.m. (Roman time). The shock was undulating, preceded by a rumbling noise in the direction W.E., and lasted three-quarters of a second. The accompanying noise is compared to that of a very heavy body in motion in contact with another body at rest. The shock is classified as No. 3 in the scale of intensity proposed by Dr. Forel. A similar shock was felt the same day at Demonte (Cuneo) at 5.25 a.m., moving in a west-south direction. On the night of the 14-15th, several shocks were felt at Terranova and Pollino in Basilicata. Twenty-two shocks of earthquake were felt on January 16, at Centi, in the province of Murcia, Spain. Several houses were destroyed, but the inmates escaped unhurt. There was no loss of life.

IT is announced from Mexico, January 23, that a new comet near Jupiter has been discovered at the Puebla Observatory.

A NEW electrical paper *Electricity*, has issued its first number at Buda-Pesth. It is written in the Magyar language. The first paper of this description ever published was called *Les Archives de l'Electricité*, and was published by M. de la Rive at Geneva in 1840, and the first issued in England was edited by the late Mr. Walker in 1843, under the title, *Electrical Magazine*. None of these papers lasted for more than three or four years.

THE Algerian Government is preparing an expedition for next spring, in order to protect effectually the southern part of the province of Oran against incursions of the surrounding independent tribes. The results of this expedition are not without interest for English journals, many of which are printed on paper made from alfa, a plant cultivated in those remote regions, and manufactured in England. A curious fact is, that no French paper-maker ever attempts to manufacture alfa for inland consumption.

THE oases of the Beni-mzab Confederacy to the south of Algeria, have been annexed to the French Algerian possessions, and a military expedition has established a regular administration in the country. The Algerian section of the French Alpine Club is organising a scientific expedition which will leave shortly in order to take advantage of a favourable season for travelling. Any one wishing to take part in this excursion should communicate with M. Durando, president of the Algerian section. The newly-annexed oases are seven in number, with a population estimated at 40,000, with about 200,000 palm-trees under cultivation. The ruins of several large towns have been covered by sand.

TWO of the most important scientific expeditions which attempted to get into the Siberian seas last year were those in the *Dijmphna*, with Lieut. Hovgaard bent on reaching the North Pole, and the Dutch Meteorological Expedition in the *Varna*, bound for Port Dickson. These two vessels succeeded in forcing the ice in the Waigatz Straits in September last, and perhaps the *Dijmphna* would then have got through the Kara Sea, had she not, by mistaking certain signals, been led to leave the open "lead" in which she was, and gone to the assistance of the steamer *Louise*, beset by the ice. She was caught in the pack, as the *Varna* had previously been, and was frozen in on September 17. The last report which we possess from these vessels, is dated September 22, and was brought to Europe by Capt. Dallmann of the *Louise*. Since that date no news whatever has come to hand from the vessels, and the statements which have appeared in the Russian press relating to the discovery by Samoyedes of a wreck, supposed to have been that of the *Dijmphna*, south of Waigatz Island, have been proved to refer to an old Russian whaler, stranded there some years ago. Although the expedition, if it had met with any mishap, would undoubtedly have found its way to the mouth of the Petchora, of which we should have had information before now, it has been decided by the Danish Government to send out a search expedition, under Capt. Norman, from Siberia, in case the *Dijmphna* should be in want of anything. On the other hand the Swedish-Norwegian Consul at Arkangelsk reports under date of December 13, that fishermen who had visited Waigatz Island in November last, had not seen any vessel near that island. In the last message received from Lieut. Hovgaard he expressed the opinion that the ice in the Kara Sea would break up during the periodical storms in September and October, and enable him to reach Port Dickson, where he intended to winter. If to this is added the statements made by Mr. Leigh Smith and Sir Henry Gore Booth, as to open water north and east of Novaya Zemlya during the summer, it is not improbable that the *Dijmphna* has got free in October, and safely reached Port Dickson, or, perhaps, even Port Aktinia on the Taimur Island. Should this be the case, Lieut. Hovgaard has, no doubt, despatched a messenger to the nearest habitation, viz. Goltochicha, and thence by express to Jeniseisk, and we may therefore look forward to reassuring news from the gallant Danish explorer at the end of January or early in February.

THE results of a fourth years' observations of periodic movements of the ground as indicated by spirit levels at Secheron, are given by M. Ph. Plantamour in the *Archives des Sciences* of December 15. The curves obtained from the east-west spirit level, for the four years, are strikingly similar in the manner (pretty regular generally) in which they follow the thermal oscillations of the air. Different years show a notable difference in the epoch of maximum descent of the east side relatively to the minimum of mean temperature, and maximum rise of the same side relatively to the maximum of temperature. One is led to consider the maximum and minimum of temperature rather as accidents as regards the epoch at which they occur, and to attribute a preponderant influence to the distribution of mean temperatures during the four months November-February, and the four June-September. Probably, too, the degree of moisture influences largely the rapidity with which the deeper ground layers are affected by exterior temperature. The curve for the north-south level is also very similar to the previous ones; but has this peculiarity, that while the south side follows, in general, from October 1 to the end of September, the oscillations of external temperature (descending in winter and rising in summer) the intermediate variations of temperature have an inverse effect. The cause is at present unknown. Col. van Orff's observations at Bogenhausen reveal oscillations of the ground similar to those at Secheron, only with greater amplitude south-north, and less east-west. M. Plantamour regrets that, excepting Col. van Orff and M. d'Abbadie, no one, so far as he knows, has undertaken observations of the kind at any other station. They are easily made, and should yield important results.

NEARLY thirty years ago, Poggendorff described a "fall-machine" of his invention. Its merits, according to Herr Bauer, who spoke warmly in commendation of it at a recent meeting of philologists and schoolmasters in Karlsruhe (*Wied. Ann.*, No. 13), appear to have been somewhat overlooked. Few physical cabinets have it, and the only text-book in which Herr Bauer has found it described is that of Reis. We may state that the two pulleys over which the cord runs are at the middle and one end of a balance beam, a weight being hung from the other end (with which, and a running weight, the beam can be rendered horizontal). The machine is supplied by Herr Karl Sickler in Karlsruhe.

THE *Nation* states that from Mr. Agassiz's annual report on the condition of the Museum of Comparative Zoology it learns that it is his intention, in connection with Prof. Faxon and Dr. Mark, to issue in the Museum Memoirs a "Selection from Embryological Monographs," containing quarto illustrations derived from innumerable scientific transactions and periodicals, and serving as an atlas for any text-book on embryology. By the purchase of the large Schary collection of Bohemian Silurian fossils, and by its own rich amassing in the West and South-west during the year, the museum now contains one of the finest collections of palæozoic fossil invertebrates in existence. Mr. Samuel Garman, whose explorations for mammalian remains in the Western Territories were very successful, was led to believe from the mode of their accumulation that the cause of extinction of the more recent was "a very severe winter, much more extensive and severe" than the occasional blizzards of our time. "As if from freezing, the shafts of the larger bones are generally splintered."

WE have received the *Annuaires* of the Bureau des Longitudes and of Montsouris Observatory, both of them abounding with useful information on many subjects. Gauthier-Villars is the publisher.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus* ♂ ♂) from India, presented by Mr. J. Steel; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, presented by Mr. John Wormald; a West Indian Rail (*Aramides cayennensis*) from West Indies, presented by Mr. E. H. Blomefield; an Orange-winged Dove (*Leptoptila ochroptera*) from Brazil, presented by Mr. C. A. Craven, C.M.Z.S.; a Long-eared Owl (*Asio otus*, British), presented by Mr. — Dyer; a Great Barbet (*Megalæma virens*), a Silky Starling (*Sturnus sericeus*), two Grey Thrushes (*Turdus cardis*), twelve Red-sided Tits (*Parus varius*) from Japan, a Crested Grebe (*Podiceps cristatus*), four Razorbills (*Alca torda*), a Red-throated Diver (*Colymbus septentrionalis*), British, purchased.

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—The first determination of elliptical elements of this comet by Mr. S. C. Chandler, of Harvard Observatory, U.S., assigned a period of revolution of about 4000 years. Later investigations have diminished this period very considerably, though the length of revolution is not determined within narrow limits. Prof. Frisby, of Washington, employing observations on September 19, October 8, and November 24, gives a period of 794 years, and finds a close agreement between the position indicated by his orbit and the Cape ante-perihelion observation of September 8. Dr. Kreutz, of Berlin, using chiefly meridian observations or normal places from September 8 to November 14, gives 843 years, and finds a pretty close accordance with observation throughout this interval, thus showing no very material perturbation at perihelion passage. Further, Dr. Morrison, of Washington, founding his calculation upon positions for September 19, October 8, and December 11, finds a period of 652½ years. But while these later computations favour a shorter revolution than was at first attributed to the comet, there remains to be ascertained to what extent the abnormal form of the nucleus since the end of September has affected the observations, and hence the deduced elements of the orbit, and a much more complete discussion of the observations than has yet been attempted, when details respecting the point of the comet observed are before us, may be required before confidence can be placed in the result of any calculation. *Primâ facie* the elements assigned by Dr. Kreutz look satisfactory enough.

In view of a possible period of seven or eight hundred years, attention may be again directed to the comet of 1106, which had several characteristics favourable to identity, though the statement in several of the chronicles (chiefly English) that at the latter part of its appearance it was seen between the north and east, is not reconcilable therewith. Pingré, advocating the identity of the comet of 1106 with the great comet of 1680 (in which he followed Halley) rather questioned the authority of the *Chronicon Alberici, Trium-fontium Monachi*, that the tail extended "below the constellation Orion," which might have been the case if the comet were identical with either of the comets of 1843, 1880, or 1882; he remarked, the monk was "ni contemporain, ni exact, ni judicieux." This subject will deserve further attention, and we may return to it shortly.

THE WASHINGTON OBSERVATORY, U.S.—The first volume of "Publications of the Washington Observatory of the University of Wisconsin" has just been issued by the director, Prof. E. S. Holden, and augurs most favourably for the reputation of this institution, which was founded within the last five years through the liberality and scientific spirit of a private individual, the Hon. C. C. Washburn. The volume is especially valuable, from its containing a large number of measures of double stars made by Mr. S. W. Burnham during his temporary connection with the Observatory, from April 23 to September 30, 1881. The measures are contained in three catalogues: (1) a list of sixty new double stars discovered in the zone observations, chiefly by Prof. Holden; (2) a list of eighty-eight new double stars discovered and measured by Mr. Burnham; and (3) measures by the same eminent observer of 150 double stars from his manuscript general catalogue. A number of difficult objects are included in the third series. 8 Sextantis was not elongated with the highest powers at the epoch 1881·34, nor did γ Coronæ show any sign of duplicity; as Mr. Burnham remarks, "it has been apparently single with all apertures since about 1871." Among the more difficult binary stars there are measures of O.Σ. 235, Σ 3123, 42 Comæ, Σ 2173, β Delphini, δ Equulei, and 85 Pegasi. There are also positions and descriptions of eighty-four red stars, of which twenty-seven are stated to be new, and a list of new nebulæ and clusters discovered in the zone-observations at the Washburn observatory.

The position of the observatory is in latitude 43° 4′ 36″·6 N., and longitude 89° 24′ 28″·3 west of Greenwich. Prof. Watson, of Ann Arbor, Michigan, was first appointed to the superintendence, but at his premature death in November, 1880, he had not been able to commence astronomical observations. Prof. Holden gives some account of the preparations he was making for scientific activity, as the only way of associating his name with the observatory.

CHEMICAL NOTES

HERR LELLMANN (*Berichte*, xv. 2835) describes an interesting case of *physical isomerism*. Dibenzyl-diamido dibromdiphenyl melts at 195°; if the liquid so produced is quickly cooled, the solid now melts at 99°, but on heating again solidifies at 125° to 130°, and melts a second time at 195°; if the substance melting at 195° be slowly cooled and then again heated, the melting point now observed is 195°.

BERTHELOT AND VIELLE (*Compt. rend.* xcv. 129) sum up the results of their researches regarding explosive waves. The propagation of an explosive wave occurs when the ignited stratum of gas exerts the maximum pressure on the adjacent stratum; increase of pressure is accompanied by increased velocity of propagation. To produce an explosive wave it is necessary that a considerable mass of gas should be employed and that the cooling by radiation and conduction should not be great; if the temperature fall below 1700°-2000°, or if the volume of the products of combustion is less than one-fourth, or in some cases one-third of the total volume of the final mixture, the propagation of the wave ceases.

ACCORDING to the experiments of M. Corne (*J. Pharm. Chim.*, [5] vi. 17) the glowing of phosphorus is due to volatilisation of the phosphorus and subsequent production of ozone by electrical energy generated by the volatilisation of the phosphorus. Phosphorus does not glow in pure oxygen at high pressures because, says M. Corne, volatilisation is impeded and at a certain limit becomes too slow to ozonise the oxygen. Gases which hinder the formation of ozone also prevent phosphorescence.

BAEYER (*Berichte*, xv. 2856) has obtained nearly pure indigo-blue by acting on a solution in acetone of ortho-nitrobenzaldehyde. Acetone and nitrobenzaldehyde react to form a condensation product, $C_{10}H_{11}NO_4$, from which alkali withdraws the elements of acetic acid with production of indigo-blue, thus—

$$2C_{10}H_{11}NO_4 + 2H_2O = C_{16}H_{10}N_2O_2 + 2C_2H_4O_2 + 4H_2O.$$

J. HORBACZEWSKI (*Berichte*, xv. 2678) has obtained uric acid by heating together glycocoll and urea to 200°-230°; details of the reaction are promised.

THE working of the Food Adulteration Act for the year 1881 is considered in a Report of the Local Government Board lately issued in a Blue Book, and published in *The Analyst* (vii. 218). The total number of districts in which analysts were acting at the close of December 1881, was 260; during the year 17,823 samples were analysed, of which 2613, equal to 14·7 per cent. were reported as adulterated; in 1877, 14,706 samples were analysed, and 19·2 per cent. reported as adulterated. More than a third of the samples analysed, and more than a half of those reported against, were of milk. Birmingham still "maintains the distinction which it has for some years enjoyed, of having a larger proportion of its milk reported as adulterated than any other great town in the kingdom." The adulteration of bread and of butter seems to be steadily on the decrease; in coffee the proportion of adulteration is rather less than last year; chicory is still the commonly used adulterant. The adulteration of sugar is practically a thing of the past. More than one-fourth of all the samples of spirits examined were reported as adulterated, chiefly with water: a good deal of gin is sold containing not much more than 20 per cent. of alcohol.

PROF. HOFMANN describes in the *Berichte* (xv. 2656) a number of interesting lecture experiments. To determine that no loss of matter occurs during combustion, he employs a two-litre flask fitted with a cork carrying a small manometer, a glass tube with stopcock, and a straight piece of rather wide tubing to the under-end of which a small porcelain crucible is attached. The wide tube is closed by a cork, about half a gram of dry phosphorus is placed in the little crucible, a portion of the air in the flask is pumped out, and the flask—and also a little bit of stout copper wire—is counterpoised; the little bit of copper wire is heated and dropped down the wide tube, from which the cork is withdrawn for a moment; the phosphorus is thus ignited; after the combustion, which proceeds slowly, is completed, the flask is found to weigh the same as before; the stopcock is now opened, air rushes in, and the flask now weighs more than it did at the beginning of the experiment.

To illustrate the great difference between the volumes of equal weights of liquid and gaseous water, Dr. Hofmann employs a glass bulb of about 300 c.c. capacity, with a narrow glass tube at each end, the upper tube being fitted with a stopcock. This apparatus is supported so that the lower tube reaches to about 1 centim. from the surface of the mercury in a basin; a rapid current of steam is passed into the apparatus; after five minutes or so, when every trace of air is expelled, the stopcock is closed, and at the same moment the lower tube is pushed beneath the mercury, which at once begins to rise into the bulb; after a little time the bulb is almost filled with mercury, on the surface of which the condensed water appears as a thin layer.

VERY simple apparatuses are also described for containing considerable quantities of liquefied gases: *e.g.* SO_2: for exhibiting *quantitatively* the reactions on which the manufacture of sulphuric acid is based; and for demonstrating the law of Dulong and Petit. For descriptions of these, and for other experiments, reference must be made to the original paper.

THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION[1]

II.

THE problem before which we are here placed may be formulated as follows:—How is it, that while the tendency to vary which obtains in all organised beings, and which forms one of the foundation stones of the theory of evolution, how is it that this tendency has exerted upon a number of living beings a so much less considerable influence than upon others, so that even in the present day numerous representatives are found of the most primitive animal groups which belong to the oldest known in the geological succession?

Still more, why are there certain genera which, since the Silurian period, appear to have undergone a stagnation in their development, in their advance towards higher differentiation, whereas within a much shorter period the whole of the living mammalian fauna has developed out of more primitive vertebrates and the important modifications have taken place among these mammalia which have finally led to the appearance of the elephant on the one hand, and of the shrews on the other?

In other words, can it be assumed that this tendency to vary could be totally and persistently neutralised by other causes amongst whole series of living beings during thousands of years, whereas during the same number of years this tendency, aided by natural selection, could lead other series of animals along roads where they have advanced with gigantic strides?

I need not remind you that this objection against the theory of evolution, which has also been felt and combated by Darwin, was very often advanced against it, especially in the beginning. Cuvier had already reminded Lamarck that the absolute identity between the Egyptian animals, as they were embalmed three thousand years ago, with those inhabiting the same provinces in the present day, rendered untenable his ideas about gradual change and perfection of organic beings.

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[1] By Prof. A. A. W. Hubrecht. Inaugural Address delivered in the University of Utrecht. September, 1882. Continued from page 281.

Huxley, to whose close reasoning powers and untiring readiness for battle the rapid progress of evolution is in a great measure due, has devoted several pages to the refutation of this objection. His argument runs as follows[1]:—

The two chief factors in the process of evolution are: the one the tendency to vary, the existence of which in all living forms may be proved by observation; the other, the influence of surrounding conditions, both upon the parent form and upon the variations which are evolved from it. Now, as often as the first factor makes itself felt, and modified forms take their origin out of a common parent form, it will depend entirely on the conditions which give rise to the struggle for existence, whether the variations which are produced shall survive and supplant the parent, or whether the parent form shall survive and supplant the variations. If the surrounding conditions are such that the parent-form is more competent to deal with them and flourish in them, than the derived forms, then, in the struggle for existence, the parent-form will maintain itself, and the derived forms will be exterminated. But if, on the contrary, the conditions are such as to be more favourable to a derived than to a parent-form, the parent-form will be extirpated, and the derived form will take its place. In the first place there will be no progression, no change of structure through any imaginable series of ages; in the second place there will be modification and change of form.

So far Huxley. No doubt but he has made us acquainted with a very reliable explanation of how the variation of any form of animals or plants may be retarded. The hypothesis of degeneration first formulated by Anton Dohrn, and afterwards warmly advocated by Ray Lankester, is no doubt of considerable importance for our comprehension of numerous lower stages of organisation in the animal and vegetable world, which may no longer be looked upon as parent-forms of more highly differentiated groups, but which, on the contrary, have in their lineage much more complicated ancestors than their own stage of organisation would appear to show. At first sight these degenerated animals show different points of similarity with animal forms, lower than those to which they are genetically allied.

So, for example, the Tunicata have for a long time been arranged amongst or close to the Mollusca, but lately-continued researches have evermore tended towards the conclusion that we have here before us the degenerate descendant of animals which had already attained the level of the lowest Vertebrates, but whose descendants, thanks to degeneration, have at present all the appearance of Invertebrates. In this way the number of lower animal types which may be looked upon as primitive, and whose persistence through geological periods gives rise to the questions as formulated above, is deceptively increased by forms, which we must remove from amongst them, and place in the vicinity of their more direct allies.

The process of degeneration is, however, confined within certain limits; it cannot do the same service towards the refutation of the objection here dealt with as can Huxley's argumentation above referred to, which is fully directed against the cardinal point, and the value of which I cannot estimate highly enough.

Still it appears to me that his explanation of the lengthened persistence of so many of the lower organised animals and plants can yet be supplemented by a new hypothesis.

To this I give the name of the hypothesis of accelerated development by primogeniture. If I have the advantage to lay it before you to-day, you will bear in mind that it has as yet only a preliminary shape, and that for its ultimate confirmation extensive researches will yet be required.

The fact is daily confirmed by continuous observation, that not only numerous vertebrates, but also very many invertebrates, can attain a very old age without the capacity for reproduction being essentially diminished. This is confirmed by the recently published researches of Weissmann[2] on the connection between the length of the reproductive period and the duration of life. We may fairly assume that all those animals attaining an old age leave issue which has been born at different periods—issue from a youthful age, which itself has again brought forth children and grandchildren, and issue from old age, which is on a level with the fourth or fifth generation of the first-born descendants. An example of old age combined with successful attempts towards reproduction is furnished by the well known sea-anemone, "Granny," which was captured in 1828 by Dalyell on the Scotch coast, and being still alive, last year gave birth to a certain number of young Actiniæ.

The large Tridacnas and the gigantic Cephalopods which have now and then been observed, must also have attained a considerable age; nothing authorises us to maintain that these have been infertile in all the later years of their lives. We need not stop to consider the higher groups: fishes, birds, and mammalia. They all contribute during a shorter or longer time towards the procreation of the species, and the considerable age which both fishes and birds are known to attain is the cause of a very considerable difference in age of the oldest and the youngest individuals of their own breeding. And so all of them will leave both first-born and last-born posterity. With the first-born this will in their turn be the case, so with *their* posterity, and so forth. Similarly the last-born, when they have attained maturity, will bring forth a series of descendants of very different ages; the last-born of the last-born being the final term of this series.

After centuries the effect will be this: From one pair of parents a large number of descendants will have sprung, a small number of these being the descendants in a direct line of the first-born of every successive generation; another small number being the descendants in a direct line of the last-born of every successive generation, whereas the remainder belong to intermediate stages. The first-born are separated from the primitive parent form by a number of generations, x, which is necessarily a considerable multiple of the number of generations y, which lies between the same parent form and their last-born descendants. Evidently the difference in age between the first-born descendant and his parents is a minimum, for the sole reason of his being the first-born, that between the last-born descendant and these same parents being on similar grounds a maximum. Thus, if we follow up in the direct line of descent the series of first-born of the first-born, &c., we find that the distance between two terms of that series corresponds to a much smaller number of years than the distance between two terms of the series of the continually last-born, which have always descended from last-born.

Comparing these two series simultaneously after the lapse of centuries, the series of the first-born will count numerous terms, many generations, at short distances from each other, whereas the series of the last-born will, on the contrary, consist of a much smaller number of terms, each of which is separated from its predecessor by a much more considerable distance. It is the number of these terms which in the one case I wished to express by x, in the other by y.[1]

From this fact we are led to propose the following question: Is there any reason to expect, that in the struggle for existence, the representatives of each of the two divergent series are collectively provided with different weapons? Or are both these groups quite equal to each other in the struggle?

Both observation and theoretical deduction force the conclusion upon us that a difference is indeed present. A difference, (1) in the external circumstances under which the first-born and the last-born come into existence; (2) in the internal properties and acquirements with which both series are provided; a difference which does not appear sporadically between certain representatives of both groups, but which may indeed be collectively observed between all of them.

As to the first point, the external circumstances, I call your attention to the following example, which shows how nature indeed makes a difference on a large scale in the conditions—under which she awaits the first-born and the last-born progeniture.

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[1] American Addresses, p. 39.

[2] A. Weissmann, "Ueber die Dauer des Lebens," 1881.

[1] I am doubtful whether there are indeed *first-born descendants* in the pure signification of the word, *i.e.* such which, both from the paternal and from the maternal side, count only first-born in the whole of their ancestry. However, this does not materially influence our argument. We bring together in the series of first-born all those descendants in which mixture and intercrossing with second and third births was always reduced to a minimum, whereas on the other hand, in the group of the last-born, not only those cases which are theoretically pure are brought together, but those in which the number of ancestors on both sides most closely approaches to the number of generations y, which lies between the last born *in abstracto* and the common parent form. In the majority of cases, however, intercrossing and blending will have occurred on a large scale, and the average number of generations which leads from them to this parent form may be expressed by $\frac{x+y}{2}$. The calculus of probabilities would be able to furnish us in any given case, supposing enough data are available, with the exact grouping of these numbers.

From the observations which Livingston Stone has made in 1878 in the North American institutions for fish-culture on the McCloud River, it follows that 14,000,000 eggs obtained from ripe but relatively young and smaller salmon were without exception at least one third smaller than the millions of eggs which were before obtained from older, larger salmon of the same species, and that nevertheless they developed quite normally. By these observations the fact is established that the salmon, when older, lays larger eggs than at a more youthful age, and this, more especially, is of great value for our hypothesis. Firstly, the size of the egg must influence the chances which they have for escaping or falling a prey to different voracious animals. In this respect the smaller eggs are exposed to other dangers than the larger ones. Furthermore, the relative size of the egg will, without doubt, exert a certain influence—however insignificant—upon the individual which is developed out of it.

In comparison with the larger egg of the older salmon, either the food-yolk or the formative yolk in the smaller one will be of smaller dimensions, or both together will have been reduced in size. In each of these three cases, even in the last-named, the conditions under which the smaller egg (that is to say, the whole generation of the first-born) attains its development, differ from those of the generation issued from the larger eggs, the generation of the last-born. The first-born will either be of smaller size, or because they possess a smaller food-yolk they will have to provide their own nourishment at an earlier date; or both circumstances are combined.

Nobody will deny that in each of these cases natural selection can freely come into play. In addition to this it must be remarked that however insignificant this difference in external circumstances may be its presence is nevertheless undeniable, since it reappears again with unerring certainty in every successive generation. In this way the effect can gradually accumulate, and finally the path may have been entered upon which leads to a specific differentiation of the descendants of the first and of the last born.

This having taught us that indeed the external circumstances which preside at the birth and at the growth of the first and the last born are different (at least for this species of salmon, reliable observations on a similar scale concerning other animals being for the present wanting), I must now call your attention to the second cardinal point, viz., that the internal properties and acquirements with which each of the two series of births is provided, are also different. Heredity has indeed invested them with peculiarities, part of which show themselves in their organisation, another part remaining latent, and only attaining development in following generations. Such a latent potential tendency towards eventual modification of the individual or his progeny, must needs find more numerous occasions to unfold itself in the first born, simply because these are possessed of a larger number of ancestors. On the contrary those that have a smaller number of ancestors, *i.e.* the last born, have had this occasion for development offered to them at rarer intervals. From this it follows that further modifications under the influence of natural selection will be started by preference in the different series of first born, because *ceteris paribus*, there are here more chances for the appearance of small deviations, which to a certain extent are always due to reversion to the parent forms.

And so there is reason to suppose that also the internal properties of the series of first born differ from those of the last born, in the same way as we have just defined it for external agencies. In my opinion the difference in internal structure is of greater consequence than that in external agencies, although we must at the same time acknowledge that our present methods do not allow us to test this experimentally. Only by extensive and long-continued experiments more light will be thrown on this subject. The example which was mentioned of the seventy-years-old sea anemone, which reproduced itself successfully proves that the material for similar experiments is not deficient. In the vegetable kingdom forms will certainly be hit upon which will fully reward the difficulties of the experiment.

Once a new species, modified and generally higher-differentiated, having arisen out of the first-born by gradual accumulation of the small deviations, intercrossing and bastardising with the last-born descendants of the parent form, becomes rarer, copulation taking place by preference with specimens of the same species, and only exceptionally with representatives of the species which has lagged behind in its development. For this new species the same process sets in; here, too, the first born progeniture will surpass in the course of years the last-born, and will in its turn give rise to new modifications. And so *ad infinitum*.

We now come to another important point, which is in direct connection with the question, which are the last-, which the first-born. With most lower animals—Protozoa, Cœlenterata, Echinoderms, Worms—reproduction by fission is very common by the side of reproduction. Cut arms of starfishes grow to be complete starfishes after having passed the so-called "comet" stage; certain annelids divide themselves after one of the posterior body-segments have become converted into a head; certain Nemertines break themselves into pieces under spasmodic contractions, each fragment being able to reproduce both head and tail; Amœbæ divide themselves into halves.

Now it cannot be admitted that in fissiparous reproduction, heredity can come into play in the same measure as it can in the case of sexual reproduction. It is not even possible to determine which of the two halves represents the older generation. Weissmann has lately humorously said: if we fancy an Amœba gifted with consciousness, she will think upon dividing into two, "I now bring forth a child," and there is no doubt that each half would look upon the other as the child, and upon itself as the mother. Weissmann has thus introduced the idea of the (approximate) immortality of the Protozoa, an idea which can also be adduced in favour of the hypothesis here maintained, and which at all events deserves to be mentioned by the side of the hypothesis proclaimed by Hæckel and others, viz. that the Monera living in the present day are in no genetical connection with older ancestors from earlier periods, but have come into existence by the aid of repeated spontaneous generation.[1]

The same views hold good for the self-division of worms and Cœlenterata. Here too both parts are the direct continuation of a single individual which, although dividing, does not cease to exist. Coral reefs which principally multiply by division may be looked upon in the same way.

Never, in case of fissiparous reproduction, does that mysterious potentiation take place which brings together in the egg-cell and in the spermatazoön, not only the characteristic properties of father and mother, but of whole series of ancestors; never in this case can the special process of fixation of a part of these latent forces, the process which we term *heredity*, take place to its full extent. Never can selection during embryonic and larval life, which, according to recent researches, plays a much more conspicuous part than was originally expected, favour the stability of a variation, and thus lead to modification of the species, where multiplication by division takes place.

In his chapter on pangenesis ("Origin of Species," second edition, pp. 353 and 390) Darwin too touches upon this subject, and insists upon the fact that organisms produced asexually, consequently not passing through the earlier phases of development, "will therefore not be exposed *at that period of life when structure is most readily modified* to the various causes inducing variability in the same manner as are embryos and young larval forms."

The series of generations which owe their origin to a-sexual and not to sexual reproduction, are thus in a much lesser degree liable to vary.[2] And yet a variation of some sort must always first occur, in order that natural selection, acting upon it, may finally produce a definite modification of the species. Nevertheless, fissiparous multiplication continues to play—and has always played—a very important part in the invertebrate kingdom, *by the side* of sexual reproduction. Thus the presumption is allowed, that where in the course of centuries a-sexual reproduction has been more predominant than sexual reproduction, a stagnation in development has resulted, the differentiation of those series of individuals and genera which have originated through sexual reproduction, in the meantime always continuing its regular course onwards.

Both factors—the retardation of development by a-sexual reproduction, and the acceleration of the development of the always first-born, make it very probable, in my opinion, that we have to look upon the more highly-developed groups of animals, and amongst these upon their higher-differentiated representatives, as forms which are separated from the original parent stock by a maximal number of ancestors, the number of times that a-sexual reproduction has taken place in their ancestry being at the same time reduced to a minimum.

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[1] Lamarck had already, by this same assumption, attempted to overcome the difficulty.

[2] Observation tends to confirm this in a general way (*vide* Darwin, *l.c.*, p. 353.

On the contrary, we must expect that a much smaller number of ancestors lies between the lower-developed groups and the common parent form, that a-sexual reproduction has here more repeatedly occurred, and that finally, Darwin's and Huxley's explanation, which we have above alluded to, of the non-occurrence of further modifications, may here have been realised to a greater extent.

Keeping in view the combined action of both these principles, we no longer wonder that even in the present day living representatives are found of genera which were already present in the Silurian epoch, nor that the simplest organised beings have continued to exist in that primitive form.

They are for the greater part the younger sons, and being condemned to a slower rate of development, they could not keep apace of their elder brothers. The latter, which have so much oftener passed through the improving crucible of sexual reproduction, are indebted to that cause for having become the parent stock out of which the higher and highest-developed animal and vegetable forms, now surrounding us, have gradually sprung.

THE ETHER AND ITS FUNCTIONS[1]

I HOPE that no one has been misled by an error in the printing of the title of this lecture, viz. the omission of the definite article before the word ether, into supposing that I am going to discourse on chemistry and the latest anæsthetic; you will have understood, I hope, that "ether" meant *the* ether, and that the ether is the hypothetical medium which is supposed to fill otherwise empty space.

The idea of an ether is by no means a new one. As soon as a notion of the enormous extent of space had been grasped, by means of astronomical discoveries, the question presented itself to men's minds, what was in this space? was it full, or was it empty? and the question was differently answered by different metaphysicians. Some felt that a vacuum was so abhorrent a thing that it could not by any possibility exist anywhere, but that nature would not be satisfied unless space were perfectly full. Others, again, felt that *empty* space could hardly exist, that it would shrink up to nothing like a pricked bladder unless it were kept distended by something material. In other words, they made matter the condition of extension. On the other hand, it was contended that however objectionable the idea of empty space might be, yet emptiness was a necessity in order that bodies might have room to move; that, in fact, if all space were perfectly full of matter everything would be jammed together, and nothing like free attraction or free motion of bodies round one another could go on.

And indeed there are not wanting philosophers at the present day who still believe something of this same kind, who are satisfied to think of matter as consisting of detached small particles acting on one another with forces varying as some inverse power of the distance, and who, if they can account for a phenomenon by an action exerted across empty space, are content to go no further, nor seek the cause and nature of the action more closely.[2]

Now metaphysical arguments, in so far as they have any weight or validity whatever, are unconscious appeals to experience; a person endeavours to find out whether a certain condition of things is by him conceivable, and if it is not conceivable he has some *primâ facie* ground for asserting that it probably does not exist. I say he has *some* ground, but whether it be much or little depends partly on the nature of the thing thought of, whether it be fairly simple or highly complex, and partly on the range of the man's own mental development, whether his experience be wide or narrow.

If a highly-developed mind, or set of minds, find a doctrine about some comparatively simple and fundamental matter absolutely unthinkable, it is an evidence, and it is accepted as good evidence, that the unthinkable state of things is one that has no existence; the argument being that if it did exist, either it or something not wholly unlike it would have come within the range of experience. We have no further evidence than this for the statement that two straight lines cannot inclose a space, or that the three angles of a triangle are equal to two right angles.

Nevertheless there is nothing final about such an argument; all that the inconceivability of a thing really proves, or can prove, is that nothing like it has ever come within the thinker's experience; and this proves nothing as to the reality or non-reality of the thing, unless his experience of the same kind of things has been so extensive as to make it reasonably probable that if such a thing had existed it would not have been so completely overlooked.

The experience of a child or a dog, on ordinary scientific phenomena, therefore, is worth next to nothing; and as the experience of a dog is to ordinary science, so is the experience of the human race to some higher phenomena, of which they at present know nothing, and against the existence of which it is perfectly futile and presumptuous to bring forward arguments about their being inconceivable, as if they were likely to be anything else.

Now if there is one thing with which the human race has been more conversant from time immemorial than another, and concerning which more experience has been unconsciously accumulated than about almost anything else that can be mentioned, it is *the action of one body on another;* the exertion of force by one body upon another, the transfer of motion and energy from one body to another; any kind of effect, no matter what, which can be produced in one body by means of another, whether the bodies be animate or inanimate. The action of a man in felling a tree, in thrusting a spear, in drawing a bow; the action of the bow again on the arrow, of powder on a bullet, of a horse on a cart; and again, the action of the earth on the moon, or of a magnet on iron. Every activity of every kind that we are conscious of may be taken as an illustration of the action of one body on another.

Now I wish to appeal to this mass of experience, and to ask, is not the direct action of one body on another across empty space, and with no means of communication whatever, is not this absolutely unthinkable? We must not answer the question offhand, but must give it due consideration, and we shall find, I think, that wherever one body acts on another by obvious contact, we are satisfied and have a feeling that the phenomenon is simple and intelligible; but that whenever one body apparently acts on another at a distance, we are irresistibly impelled to look for the connecting medium.

If a marionette dances in obedience to a prompting hand above it, any intelligent child would feel for the wire, and if no wire or anything corresponding to it were discovered, would feel that there was something uncanny and magical about the whole thing. Ancient attempts at magic were indeed attempts to obtain results without the trouble of properly causing them, to build palaces by rubbing rings or lanterns, to remove mountains by a wish instead of with the spade and pickaxe, and generally to act on bodies without any real means of communication; and modern disbelief in magic is simply a statement of the conviction of mankind that all attempts in this direction have turned out failures, and that action at a distance is impossible.

If a man explained the action of a horse or a cart by saying that there was an attraction between them varying as some high direct power of the distance, he would not be saying other than the truth—the facts may be so expressed—but he would be felt to be giving a wretchedly lame explanation, and any one who simply pointed out the traces would be going much more to the root of the matter. Similarly with the attraction of a magnet for another magnetic pole. To say that there is an attraction as the inverse cube of the distance between them is true, but it is not the whole truth; and we should be obliged to any one who will point out the traces, for traces we feel sure there are. If any one tries to picture clearly to himself the action of one body on another without any medium of communication whatever, he must fail. A medium is instinctively looked for in most cases, and if not in all, as in falling weights or in magnetic attraction, it is only because custom has made us stupidly callous to the real nature of these forces.

When we see a vehicle bowling down-hill without any visible propelling force we ought to regard it with the same mixture of curiosity and wonder as the Chinaman felt when he saw for the first time in the streets of Philadelphia a tram-car driven by a rope buried in a pipe underground. The attachment to these cars comes through a narrow slit in the pipe, and is quite unobtrusive. After regarding the car with open-mouthed astonishment for some time, the Chinaman made use of the following memorable exclamation, "No pushee—No pullee—Go like mad!" He was a philosophic Chinaman.

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[1] A lecture by Prof. Oliver Lodge at the London Institution, on December 28, 1882.

[2] In illustration of this statement an article has since appeared in the January number of the *Philosophical Magazine*, by Mr. Walter Browne.

Remember then that whenever we see a thing being moved we must look for the rope; it may be visible or it may be invisible, but unless there is either "pushee" or "pullee" there can be no action. And if you further consider a pull it revolves itself into a push; to pull a thing towards you, you have to put your finger behind it and push; a horse is said to pull a cart, but he is really pushing at the collar; an engine pushes a truck by means of a hook and eye; and so on. There is still the further very important and difficult question as to why the parts hang together, and why when you push one part the rest follows.

Cohesion is a very striking fact, and an explanation of it is much to be desired; I shall have a little more to say about it later, but at present we have nothing more than an indication of the direction in which an explanation seems possible. We cannot speak distinctly about those actions which are as yet mysterious to us, but concerning those which are comparatively simple and intelligible we may make this general statement:—The only way of acting on a body directly is to push it behind.

There must be contact between bodies before they can directly act on each other; and if they are not in contact with each other and yet act, they must both be in contact with some third body which is the medium of communication, the rope.

Consider now for an instant the most complex case, the action of one animate body on another not touching it. To call the attention of a dog, for instance, there are several methods: one plan is to prod him with a stick, another is to heave a stone at him, a third is to whistle or call, while a fourth is to beckon him by gesture, or, what is essentially the same process, to flash sunlight into his eye with a mirror. In the first two of these methods the media of communication are perfectly obvious—the stick and the stone—in the third, the whistle, the medium is not so obvious, and this case might easily seem to a savage like action at a distance, but we know of course that it is the air, and that if the air between be taken away, all communication by sound is interrupted. But the fourth or optical method is not so interrupted; the dog can see through a vacuum perfectly well, though he cannot hear through it; but what the medium now is which conveys the impression is not so well known. The sun's light is conveyed to the earth by such a medium as this across the emptiness of planetary space. The only remaining typical plans of acting on the dog would be either by electric or magnetic attractions, or by mesmerism, and I would have you seek for the medium which conveys these impressions with just as great a certainty that there is one as in any of the other cases.

Leaving these more mysterious and subtle modes of communication, let us return to the two most simple ones, viz., the stick and the stone. These two are representative of the only possible fundamental modes of communication between distant bodies, for one is compelled to believe that every more occult mode of action will ultimately resolve itself into one or other of these two.

The stick represents the method of communication by continuous substance; the stone represents the communication by actual transfer of matter, or, as I shall call it, the projectile method. There are no other known methods for one body to act on another than by these two—by continuous medium, and by projectile.

We know one clear and well-established example of the projectile method, viz., the transmission of pressure by gases. A gas consists of particles perfectly independent of each other, and the only way in which they can act on each other is by blows. The pressure of the air is a bombardment of particles, and actions are transmitted through gases as through a row of ivory balls. Sound is propagated by each particle receiving a knock and passing it on to the next, the final effect being much the same as if the first struck particles had been shot off through the whole distance.

The explanation of the whole behaviour of gases in this manner is so simple and satisfactory, and moreover is so certainly the true account of the matter, that we are naturally tempted to ask whether this projectile theory is not the key to the universe, and whether every kind of action whatever cannot be worked out on this hypothesis of atoms blindly driving about in all directions at perfect random and with complete independence of each other except when they collide.[1] And accordingly we have the corpuscular theories of light and of gravitation; both account for the respective phenomena by a battering of particles. The corpuscular theory of gravitation is, however, full of difficulties, for it is not obvious according to it why the weight of a plate is the same when held edgeways as when held broadside on, in the stream of corpuscles; while it is surprising (as indeed it perhaps is on any hypothesis) that the weight of a body is the same in the solid, liquid, and gaseous states. It has been attempted to explain cohesion also on the same hypothesis, but the difficulties, which were great enough before, are now enormous, and to me at any rate it seems that it is only by violent straining and by improbable hypotheses that we can explain all the actions of the universe by a mere battery of particles.

Moreover, it is difficult to understand what the atoms themselves can be like, or how they can strike and bound off one another without yielding to compression and then springing out again like two elastic balls; it is difficult to understand the elasticity of really ultimate hard particles. And if the atoms are not such hard particles, but are elastic and yielding, and bound from one another according to the same sort of law that ivory balls do, of what are they composed? We shall have to begin all over again, and explain the cohesion and elasticity of the parts of the atom.

The more we think over the matter, the more are we compelled to abandon mere impact as a complete explanation of action in general. But if this be so we are driven back upon the other hypothesis, the only other, viz. communication by continuous medium.

We must begin to imagine a continuous connecting medium between the particles—a substance in which they are imbedded, and which extends into all their interstices, and extends without break to the remotest limits of space. Once grant this and difficulties begin rapidly to disappear. There is now continuous contact between the particles of bodies, and if one is pushed the others naturally receive the motion. The atoms of gas are impinging as before, but we have now a different idea of what impact means.

Gravitation is explainable by differences of pressure in the medium, caused by some action between it and matter not yet understood. Cohesion is explainable also probably in the same way.

Light consists of undulation or waves in the medium; while electricity is turning out quite possibly to be an aspect of a part of the very medium itself.

The medium is now accepted as a necessity by all modern physicists, for without it we are groping in the dark, with it we feel we have a clue which, if followed up, may lead us into the innermost secrets of nature. It has as yet been followed up very partially, but I will try and indicate the directions in which modern science is tending.

The name you choose to give to the medium is a matter of very small importance, but "the Ether" is as good a name for it as another.

As far as we know it appears to be a perfectly homogeneous incompressible continuous body incapable of being resolved into simple elements or atoms; it is, in fact, continuous, not molecular. There is no other body of which we can say this, and hence the properties of ether must be somewhat different from those of ordinary matter. But there is little difficulty in picturing a continuous substance to ourselves, inasmuch as the molecular and porous nature of ordinary matter is by no means evident to the senses, but is an inference of some difficulty.

Ether is often called a fluid, or a liquid, and again it has been called a solid and has been likened to a jelly because of its rigidity; but none of these names are very much good; all these are molecular groupings, and therefore not like ether; let us think simply and solely of a continuous frictionless medium possessing inertia, and the vagueness of the notion will be nothing more than is proper in the present state of our knowledge.

We have now to try and realise the idea of a perfectly continuous, subtle, incompressible substance pervading all space and penetrating between the molecules of all ordinary matter, which are imbedded in it, and connected with one another by its means. And we must regard it as the one universal medium by which all actions between bodies are carried on. This, then, is its function—to act as the transmitter of motion and of energy. First consider the propagation of light.

Sound is propagated by direct excursion and impact of the atoms of ordinary matter. Light is not so propagated. How do we know this?

1. Because of speed, 3×10^{10}, which is greater than anything transmissible by ordinary matter.

2. Because of the kind of vibration, as revealed by the phenomena of polarisation.

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[1] To this hypothesis Mr. Tolver Preston has addressed himself with much ingenuity.

The vibrations of light are not such as can be transmitted by a set of disconnected molecules; if by molecules at all, it must be by molecules connected into a solid, *i.e.* by a body with rigidity. Rigidity means active resistance to shearing stress, *i.e.* to alteration in shape; it is also called *elasticity of figure*; it is by the possession of rigidity that a solid differs from a fluid. For a body to transmit vibrations at all it must possess inertia; transverse vibrations can only be transmitted by a body with rigidity. All matter possesses inertia, but fluids only possess volume elasticity, and accordingly can only transmit longitudinal vibrations. Light consists of transverse vibrations; air and water have no rigidity, yet they are transparent, *i.e.* transmit transverse vibrations; hence it must be the ether inside them which really conveys the motion, and the ether must have properties which, if it were ordinary matter, we should style *inertia* and *rigidity*. No highly rarefied air will serve the purpose; the ether must be a distinct body. Air *exists* indeed in planetary space even to infinity, but it is of almost infinitesimal density compared with the ether there. It is easy to calculate the density of the atmosphere at any height above the earth's surface, supposing other bodies absent.

The density of the air at a distance of n earth radii from the centre of the earth is equal to a quarter the density here divided by $10^{350\frac{n-1}{n}}$. So at a height of only 4000 miles above the surface, the atmospheric density is a number with 127 ciphers after the decimal point before the significant figures begin. The density of ether, on the other hand, has been calculated by Sir William Thomson from data furnished by Pouillet's experiments on the energy of sunlight, and from a justifiable guess as to the amplitude of a vibration, and it comes out about 10^{-18}, a number with only 17 ciphers before the significant figures. In inter-planetary space, therefore, all the air that exists is utterly negligible; the density of the ether there, though small, is enormous by comparison.

Once given the density of the ether, its rigidity follows at once, because the ratio of the rigidity to the density is the square of the velocity of transverse wave propagation, viz. in the case of ether, 9×10^{20}. The rigidity of ether comes out, therefore, to be about 900. The most rigid solid we know is steel, and compared with its rigidity, viz. 8×10^{11}, that of ether is insignificant. Neither steel nor glass, however, could transmit vibrations with anything like the speed of light, because of their great density. The rate at which transverse vibrations are propagated by crown glass is half a million centimetres per second—a considerable speed, no doubt, but the ether inside the glass transmits them 40,000 times as quick, viz. at twenty thousand million centimetres per second.

The ether outside the glass can do still better than this, it comes up to thirty thousand million, and the question arises what is the matter with the ether inside the glass that it can only transmit undulations at two-thirds the normal speed. Is it denser than free ether, or is it less rigid? Well, it is not easy to say; but the fact is certain that ether is somehow affected by the immediate neighbourhood of gross matter, and it appears to be concentrated inside it to an extent depending on the density of the matter. Fresnel's hypothesis is that the ether is really denser inside gross matter, that there is a sort of attraction between ether and the molecules of matter which results in an agglomeration or binding of some ether round each atom, and that this additional or bound ether belongs to the matter, and travels about with it. The *rigidity* of the bound ether Fresnel supposes to be the same as that of the free.

If anything like this can be imagined, a measure of the density of the bound ether is easily given. For the inverse velocity-ratio is called μ (the index of refraction), and the density is inversely as the square of the velocity, hence the density-measure is μ^2. The density of ether in free space being called 1, that inside matter has a density μ^2, and the density of the bound portion of this is $\mu^2 - 1$.

This may all sound very fanciful, but something like it is sober truth; not as it is here stated very likely, but the fact that $\left(1 - \frac{1}{\mu^2}\right)$th of the whole ether inside matter is bound to it and travels with it, while the remaining $\frac{1}{\mu^2}$th is free and blows freely through the pores, is fairly well established and confirmed by direct experiment.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following further announcements of lectures have been made:—

Prof. Humphry, Circulatory and Respiratory Systems, Jan. 25; senior class, Jan. 29; Demonstrations by the Demonstrator for Natural Science Tripos, Jan. 26; Osteology, for beginners, Jan. 17; Demonstrations for second year students, Jan. 18; Mr. McAlister will give six lectures later in the term, on the Mechanism of the Human Skeleton. Dr. Michael Foster's course of Elementary Physiology, Jan. 23; Mr. Lea, Chemical Physiology, Jan. 24; Dr. Vines, Anatomy of Plants, advanced, with practical work, Jan. 24 (Christ's College); General Elementary course, New Museums, Jan. 23, to extend over two terms, and be illustrated by demonstrations. A class for the practical study of systematic botany, Mr. T. H. Corry, assistant curator of the Herbarium, will be formed. Dr. Hicks will lecture on the Morphology of Flowering Plants, with especial reference to classification, including floral diagrams, in the Hall of Sidney College, beginning Jan. 26; Mr. Glazebrook, advanced Demonstrations in Electricity and Magnetism, Cavendish Laboratory, Jan. 24; Mr. Shaw, Demonstrations in Mechanics and Heat, Jan. 23; if more students attend than can be accommodated in the laboratory at one time, the course will be repeated on the same days. Mr. Trotter, Trinity College, Physical Optics, Jan. 25. Mr. Pattison Muir, Non-metallic Elements, Elementary, Jan. 22, Caius College Laboratory; General Principles of Chemistry, Advanced, Jan. 23. Mr. Solly will give Demonstrations on Minerals in the Lecture Room of the Mineralogical Museum, first lecture, Jan. 22. Prof. Stuart, Jacksonian Lecture Room, Theory of Structures, Jan. 30; the Demonstrator of Mechanism, Mathematics required for Engineering, Jan. 29.

Christ's College Open Scholarships, Natural Science; E. L. Sortain, Bath College, 30*l.*; 3rd year, J. C. Bose, 30*l.*; Caius College, Natural Science, Edgworth, Clifton College, 40*l.*

MR. MARSHALL WARD is giving a course of free public lectures at Owens College, on the Nutrition of Plants.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, January.—Electric lighting in mills, by C. J. H. Woodbury.—Bricks and brick-making machinery, by C. Chambers, Jun.—Experiential principles of controlled combustion, by E. J. Mallett, Jun.—Olsen's testing machines.

Archives des Sciences Physiques et Naturelles, December 15, 1882.—Meteorological *résumé* of the year 1881 for Geneva and the great St. Bernard, by A. Kammermann.—Observations on cometary refraction, by W. Meyer.—Development of the vegetable kingdom in different regions since the tertiary epoch, according to Dr. Engler's work, by A. de Candolle.—Periodical movements of the air indicated by spirit levels, by Ph. Plantamour.—On the same, by C. von Orff.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, January 18.—Dr. Gilbert, president, in the chair.—It was announced that a ballot for the election of fellows would be held at the next meeting, February 1.—The following papers were read:—The fluorine compounds of uranium, by A. Smithells. The author has investigated the action of aqueous hydrofluoric acid upon the green uranoso-uranic oxide. He finds that a voluminous green powder, uranium tetrafluoride, is left, and that a yellow solution is formed which contains uranium oxyfluoride. The author confirms the results previously obtained by Bolton, and proves those obtained by Ditte to be erroneous.—On a new method of estimating the halogens in volatile organic compounds, by R. T. Plimpton and E. E. Graves. The authors burn the vapour of the compound in a glass Bunsen burner, the products of the combustion are aspirated through caustic soda solution, which is heated with sulphurous acid and the halogen precipitated by silver nitrate, &c., in the usual way. Good results were obtained with various liquids from ethyl bromide boiling at 39° to acetylene bromiodide boiling at 150°.—On a modified Liebig's condenser, by W. A. Shenstone. The author has slightly modified a vertical con denser so that it can be used for prolonged digestion and subsequent distillation without shifting.—On two new aluminous mineral species Evigtokite and Liskeardite, by W. Flight.—On the volume alteration attending the mixture of salt solutions, by W. W. J. Nicol. The salts employed were NaCl, KCl, KNO_3, $NaNO_3$, $CuSO_4$ and K_2SO_4.

Zoological Society, January 16.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. H. E. Dresser, F.Z.S., exhibited and made remarks on a specimen of *Merops philippensis*, which was said to have been obtained near the Snook, Seaton Carew, in August, 1862.—Lieut.-Col. Godwin-Austen, F.R.S., read the third and concluding of a series of papers on the shells which had been collected in Socotra by Prof. J. Bayly Balfour. The present portion treated of the freshwater shells of Socotra, which were stated all to belong to the genera *Planorbis*, *Hydrobia*, and *Melania*. Not a single bivalve was obtained. Four species were described as new, namely, *Planorbis socotrensis*, *P. cockburni*, *Hydrobia balfouri*, and *Melania sclateri*.—Prof. E. Ray Lankester, F.R.S., read a paper on the right cardiac valves of *Echidna* and of *Ornithorhynchus*. Seven additional specimens of the latter animal had been examined since the author's former paper on this subject had been read, all of which, whilst showing interesting variations, agreed in the absence of the septal flap of the right cardiac valve. This character was shown to exist also in *Echidna*, and was therefore presumed to be a distinctive feature in the structure of the Monotremes.—A communication was read from Mr. F. Moore, F.Z.S., containing the descriptions of some new genera and species of Asiatic Lepidoptera Heterocera.—A communication was read from Mr. G. B. Sowerby, jun., in which he gave the descriptions of five new species of shells from various localities.

Anthropological Institute, January 9.—Mr. A. J. Lewis in the chair.—The election of Admiral F. S. Tremlett, F.R.G.S., was announced.—Mr. Worthington G. Smith exhibited four palæolithic implements from Madras. One of them weighed 4 lbs. 7¾ oz., and the author believed that it was the second largest specimen of the kind extant.—Mr. W. S. Duncan read a paper on the probable region of man's evolution. Starting with the assumption that man was evolved from a form lower in organisation than that of the lowest type yet discovered, and that his origination formed no exception to the general law of evolution recognised as accounting for the appearance of the lower forms of life, the author said that man's most immediate ancestors must have been similar in structure to that of the existing Anthropoid apes, although it is not necessary to suppose that any of the Anthropoid apes at present existing belong to the same family as that of man. The science of the distribution of animals showed that the higher types of monkeys and apes appear to have had their origin in the Old World, the American continent being entirely destitute of them, either alive or fossil. The distribution of the greater portion of the animals of the Old World was shown to have taken a generally southward direction, owing to the gradual increase of the cold, which culminated in the last Ice Age. This migration was, however, interrupted by the interposition of the Mediterranean and other seas, and thus, although a few of these animals were enabled to journey on until they reached tropical regions, the majority were compelled to remain behind, where they had to exist under altered circumstances. The temperature was much lower, and as a result of the consequent diminution of the number of fruit forests, a change in the food and in the manner in which it was obtained by the apes occurred. A considerable alteration took place also in the manner in which they were forced to use their limbs, and it was due to the operation of these and other causes that the ape form became stamped with human characteristics such as the curvature of the spine and an increase in the breadth of the pelvis. For these reasons the author regarded the south of Europe as the part in which it was most likely that the evolution of man took place. Mr. Duncan concluded by urging the importance of forming a committee to watch discoveries bearing on this branch of anthropology.

Meteorological Society, January 17.—Annual General Meeting.—Mr. J. K. Laughton, F.R.A.S., President, in the chair.—The Secretary read the Report of the Council which showed that the total number of Fellows was 571, 47 new Fellows having been elected during the year.—The President then delivered his Address. He referred briefly to the great importance of the uniform series of observations now taken under the auspices of the Society, and proceeded to speak, at greater length, of certain other points in which the Society might, by its concerted action, further the interests of meteorological science. The first of these was anemometry, which is at present in a condition far from satisfactory. We know nothing positively either as to the pressure or the velocity of the wind; there is no exact standard instrument, and observations, whatever may be their absolute value, are not comparable one with the other. He thought that the Society might properly interfere, so far as to regulate the wide diversity amongst the instruments now used, in order that when the proper time came, and it was known what anemometer could be trusted, the older observations might be reduced. The movement of air in the upper regions of the atmosphere is not measurable by any existing method; but experiments have been made, at the suggestion of the Meteorological Council, in which the drift of the smoke-cloud of a bursting shell may be observed and measured. The observations of the barometer taken at elevated stations in the United States seem to throw considerable doubt on the received formulæ for the reduction of barometric readings to sea-level, and for the calculation of heights. When the observations extend over a long period, and are regularly taken under all conditions of weather, then no doubt the height of a mountain can be calculated with a fair approach to accuracy; but isolated observations, subject to the fluctuations of the different readings are extremely wild in their results. In the same way, the reduction of the barometer to sea-level is complicated by many discrepancies which arise between observations at the upper and lower stations, which have hitherto been ignored. It is impossible to say how far they affect the isobars on which our daily weather charts are based; but it is probable that they are at least one additional source of error and of difficulty. It is much to be wished that systematic and continuous observations at high-level stations could be taken, not only on the top of Ben Nevis, but on the top of some others of the highest peaks in different parts of the country. In this way alone, can these difficulties of reduction be cleared away.—The following gentlemen were elected the Officers and Council for the ensuing year:—President, John Knox Laughton, F.R.A.S., Vice-Presidents: Edmund Douglas Archibald, M.A., Rogers Field, B.A., Baldwin Latham, F.G.S., William Marcet, F.R.S., Treasurer, Henry Perigal, F.R.A.S., Trustees: Hon. Francis Albert Rollo Russell, Stephen William Silver, F.R.G.S., Secretaries: George James Symons, F.R.S., John William Tripe, M.D., Foreign Secretary, Robert Henry Scott, F.R.S., Council: Hon. Ralph Abercromby, William Morris Beaufort, F.R.A.S., John Sanford Dyason, F.R.G.S., Henry Storks Eaton, William Ellis, F.R.A.S., Joseph Henry Gilbert, F.R.S., Charles Harding, Robert John Lecky, F.R.A.S., Capt. John Pearse Maclear, R.N., Edward Mawley, F.R.H.S., George Matthews Whipple, F.R.A.S., Charles Theodore Williams, M.D.

Edinburgh

Royal Society, January 15.—Prof. Maclagan, vice-president, in the chair.—In a paper on the diurnal variation of the force of the wind on the open sea and near land, Mr. Buchan gave the first instalment of the meteorological results of the *Challenger* expedition. From fully 1200 observations which had been taken, mean diurnal curves were drawn for the different oceans, from which it appeared that in the open sea no clear marked diurnal variation existed, but that near land a very evident maximum showed itself about two in the afternoon, and a much smaller maximum at midnight. Also near land the force of the wind was distinctly less than in the open sea, a fact readily accounted for by the greater friction experienced at the surface in the former case. The wind was strongest in the southern ocean, feeblest in the Pacific. Though the temperature observations had not been completely reduced, enough had been done to show that the surface temperature of the North Atlantic was subject to a very small variation of not more than ·75 of a degree Fahrenheit.—The Rev. Dr. Teape read a long paper on the Semitic and Greek article, in which he pointed out the influence of the Hebrew idiom upon the use of the Greek article, both in the Septuagint and the New Testament, and maintained, in opposition to Prof. Blackie's views, that the use of the Greek article was regulated by definite grammatical rules.—Mr. W. W. J. Nicol, M.A., B.Sc., read a paper on the nature of solution, which he regarded from the point of view of molecular attraction. Solution took place because the particles of water had a greater attraction for the particles of the salt than these had for themselves. The theory was applied to explain various facts established by himself and other experimenters, such for example as the relation between the density of a crystal and the temperature at which it is made to crystallise out.—An elaborate experimental paper on the relative electro-chemical positions of wrought iron, steels, cast metal, etc., in sea-water and other solutions by Mr. Thomas Andrews, Assoc.M.Inst.C.E., F.C.S., was communicated by Prof. Crum Brown. The time changes in the galvanic relations were very curious, showing in some instances a complete reversal of the poles. This was regarded as probably due to the penetration of the liquid into the plates, which would thus seem to be very far from homogeneous. The experiments have evidently an important bearing on the question of erosion in sea-water.

SYDNEY

Linnean Society of New South Wales, October 25, 1882.—Dr. James C. Cox, F.L.S., &c., president, in the chair.—The following papers were read:—Description of a new species of *Solea* from Port Stephens, by E. P. Ramsay, F.L.S. This new species of sole, of which a drawing was exhibited, was proposed to be named *S. lineata*.—Contributions to Australian oology (continuation), by E. P. Ramsay, F.L.S. In this paper the author gave descriptions of the nests and eggs of nineteen additional species of Australian birds, whose nidification and oology had previously been imperfectly known.—Descriptions of Australian Micro-lepidoptera, by E. Meyrick, B.A. This, the eighth paper by Mr. Meyrick on the Micro-lepidoptera of this country, treats exclusively of the *Oecophoridæ*, a family represented in Australia by about 2000 species. Fifteen genera and 107 species are described at great length in the present paper.—Notes on the geology of the Western coal-fields, by Prof. Stephens, M.A., No. 1. This was a brief account of the Wallerawang and Capertee conglomerates and overlying coal-measures, together with some description of the Devonian beds of the Capertee Valley and Coco Creek. Specimens of *Brachiopoda* and *Favosites*, together with a large *Pleurotomaria* as well as of Porphyry and other rocks obtained from the same locality were shown in illustration of the paper.—Notes on the oyster beds at Cape Hawke, by James C. Cox, M.D., &c. This was a paper in support of the author's views, as expressed in a previous paper, of the undoubted specific difference between the drift oyster and rock oyster of our coasts.

PARIS

Academy of Sciences, January 15.—M. Blanchard in the chair.—The following papers were read:—Choice of a first meridian, by M. Faye (Report in name of Commission). This is favourable to the American proposal.—On the mechanical and physical constitution of the sun (first part), by M. Faye. He presents a *résumé* of his researches on the subject.—Researches on alkaline sulphites, by M. Berthelot.—On alkaline hyposulphites, by the same.—On complex units, by M. Kronecker.—Separation of gallium (continued), by M. Lecoq de Boisbaudran.—Table concerning the ramification of *Isatis tinctoria*, by M. Trécul.—On hydraulic silica, and on the *rôle* it plays in the hardening of hydraulic compounds, by M. Landrin. The pure silica obtained by decomposing a solution of silicate of potash with an acid, and repeatedly washing and drying at a dark-red heat, he names *hydraulic silica*, and he considers it the cause of the final hardening of hydraulic mortars. The aluminate of lime cannot concur in this effect, because of solubility, but at the moment of immersion it facilitates the intimate union of the hydraulic elements, hinders water from penetrating the mass of mortar, and so aids the slow reciprocal action of the lime and hydraulic silica.—Chemical studies on maize, &c. (continued), by M. Leplay.—Treatment of typhoid fever at Lyons, in 1883, by M. Glénard. Instead of the *expectant* method, which awaits complications, combating them as they arise, the method of treatment with cold baths has been adopted in Lyons (as in Germany), with a view to preventing those complications. The mortality is thus greatly reduced (*e.g.* in the civil hospitals of Lyons from 26 to 9 per cent., in private practice to 1 or 2 per cent.).—On the proposals of M. Balbiani for opposing phylloxera, and on the winter egg of the phylloxera of American and European vines, by M. Targioni-Tozzetti. He throws doubt on the data on which the Phylloxera Commission have proceeded, in directing effort towards the destruction of the winter-egg. M. Balbiani replies at length to his arguments, none of which, he states, are new.—Treatment of phylloxerised vines, with sulpho-carbonate of potassium in 1882, by M. Mouillefert. The surface treated was 2225 hectares, on 385 properties, and a steady increase is shown since 1877. The amount of sulpho-carbonate used was 821,317 kg.; the cost varied between 200 and 450 francs per hectare; 0·05 fr. and 0·04 fr. per stock.—Observations on the subject of the Circular of the United States Government, concerning the adoption of a common initial meridian and a universal hour, by M. de Chancourtois. He advocates the adoption of a decimal division of the day and of the circle (the latter into 400 degrees, the right angle containing 100). The ancient meridian of Ptolemy, about 31·7 degrees from that of Paris, he considers the best for the initial meridian.—On the hypergeometric functions of superior order, by M. Goursat.—On Fourier's series, by M. Halphen.—On a general property of an agent whose action is proportional to the product of the quantities in presence and to any power of the distance, by M. Mercadier.—Methods for determination of the ohm, by M. Brillouin.—Reply to a note of M. Maurice Lévy.—Researches on the relative oxidisability of cast iron, steel, and soft iron, by M. Gruner. Various plates, suspended in a frame, by their four corners, were immersed simultaneously in water acidulated with 0·5 per cent. of sulphuric acid, or sea-water, or were simply exposed in moist air of a terrace. *Inter alia*, in moist air, chromate steels were oxidised most, and tungsten steels less than mere carbon steel. Cast iron, even with manganese, is oxidised less than steel and soft iron, and white specular iron less than grey cast iron. Sea-water, on the other hand, attacks cast iron more than steel, and with special energy white specular iron. Tempered steel is less attacked than the same steel annealed, soft steel less than manganese steel or chromate steel, &c. Acidulated water, like sea-water, dissolves grey cast iron more rapidly than steel, but not white specular iron; the grey impure cast iron is most strongly attacked.—On the losses and gains of nitrogen in arable land, by M. Dehérain. The losses are due not only to the exigencies of crops, but also, and for the most part, to the oxidation of azotised organic matter. When the land is not stirred, but kept as natural or artificial meadows, the combustions are less active, and the gains of nitrogen exceed the losses. Thus a farmer will more easily enrich a soil with nitrogen by keeping it in a meadow than by prodigal manuring.—Physiological action of picoline and lutidine, by MM. de Coninck and Pinet.—New experiments on irian grafts, with a view to establishing the etiology of cysts of the iris, by M. Masse.—On the solutions of continuity produced at the moment of moulting, in the apodemian system of decapod crustaceans, by M. Nevequard.

CONTENTS

PAGE
THE THIRST FOR SCIENTIFIC RENOWN 285
CINCHONA PLANTING 287
MARINE SURVEYING 289
LETTERS TO THE EDITOR:—
  Natural Selection and Natural Theology.—Prof. ASA GRAY . . 291
  Intelligence in Animals.—J. G. GRENFELL 292
  On a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies.—F. TROUTON . . . 292
  The Gresham Funds.—W. B. 292
  Siwalik Carnivora.—RICHARD LYDEKKER 293
  Earthquakes.—Prof. GEORGE LAWSON; GEORGE F. BURDER; R. H. TIDDEMAN 293
  The Sea Serpent.—F. T. MOTT (*With Illustration*) 293
  A Novel Experiment in Complementary Colours.—JOHN GORHAM 294
  The Projection of the Nasal Bones in Man and the Ape.—J. PARK HARRISON 294
HOVERING OF BIRDS. By HUBERT AIRY (*With Diagram*) 294
THE LATE EDWARD B. TAWNEY 295
REMARKS ON AND OBSERVATIONS OF THE METEORIC AURORAL PHENOMENON OF NOVEMBER 17, 1882. By Dr. H. J. H. GRONEMAN (*With Illustration*) 296
NOTES 298
OUR ASTRONOMICAL COLUMN:—
  The Great Comet of 1882 300
  The Washington Observatory, U.S. 300
CHEMICAL NOTES 301
THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION, II. By Prof. A. A. W. HUBRECHT 301
THE ETHER AND ITS FUNCTIONS. By Prof. OLIVER LODGE 304
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 306
SCIENTIFIC SERIALS 306
SOCIETIES AND ACADEMIES 306

THURSDAY, FEBRUARY 1, 1883

POPULAR ASTRONOMY

The Sun, its Planets, and their Satellites. A Course of Lectures upon the Solar System, read in Gresham College, London, in the Years 1881 and 1882. By Edmund Ledger, M.A., Rector of Barham, Suffolk. Pp. 432. (London: Stanford, 1882.)

ANOTHER work on Astronomy! It must have demanded some courage to venture on such an attempt in these days, so unprecedentedly fertile in similar undertakings. We are not speaking of the inundation of lighter productions—the magazines, the lectures, the newspaper articles—by which the lower grounds of modern society are overrun, to the benefit no doubt, in many cases, of those who may thus be led to find out in what a glorious world they live. Provided only that such efforts have something clear and something pleasant about them, we shall not be disposed to say "the fewer the better cheer." It is a worthy and honourable attempt, to introduce one new interest, one fresh and innocent pleasure, into the dull round of a careworn plodding life. The shepherd will love his work none the less for learning something of the movements of his "unfolding star:" the evening of the weary mechanic will bring unalloyed refreshment, if he is enabled to turn an inquiring gaze upon "the fields of light that lie around the throne of GOD." But not only is provision being thus made for the development of thought and intelligence among those whose lives too often are divided between uninteresting labour and debasing gratification, but a corresponding advance has been made in the production of treatises addressed to the possessors of more cultivated minds and leisurely opportunities. A full collection of such treatises during the last half-century would be at once voluminous and interesting. What would come out in strong relief from a comparison of them would be the comprehensiveness and many-sidedness of the subject! It is indeed a glorious subject—the "consideration of the heavens"; the subject of a life-time, of many life-times—in all its complexity of magnificence. No one mind, no one book, can do it justice. It is as boundless as the spaces of which it treats, and the mechanism which it professes to explain. It embraces no small part of the history of human intelligence; it demands the utmost power and subtlety of the most consummate analysis; the picturings of the most poetical imagination will be tame and feeble in the presence of its realities; and yet so simple are many of its elementary truths as to invite and recompense familiar inquiry. There may be room then for another, and another, and yet another work on astronomy; and provided they are thoughtfully designed and accurately wrought out, there will be little question as to their success; for it arises from the very comprehensiveness of the matter, that every writer will address himself to the task from his own point of view, and all readers may find something to interest them in every varied presentation of the subject.

We are pleased to give a welcome reception to the treatise which is now before us. In many respects it will be found worthy to take rank among the best. Where, as we have said, the study is so many-sided, it is obviously better to work on certain lines; not to attempt too wide a grasp, with the inevitable annoyance of bulk and costliness; not to be led into the opposite course of saying a little about everything, and enough about nothing. The author of these Lectures has chosen his own line, preferring to give us a good deal that is explanatory of the mechanism of the solar system, and a good deal that is descriptive of its wonders. And he has executed his task on the whole remarkably well. He has evidently a clear apprehension of what he is going to write about, and therefore succeeds in making it clear to other minds; and there is a pleasant facility in his style which imparts readableness to matters intrinsically somewhat dry. And if we meet with little of vivid and imaginative description, its place is supplied by a truly valuable amount of caution and discretion in dealing with the theories of the day. If he does not lead us far he will certainly not lead us wrong: and "when," as he characteristically tells us, "we know so little, we must not let our ignorance suggest unnecessary difficulties. Rather let it teach us to wait, and watch, and learn." Availing himself of no common extent of reading, he has used his materials with conscientious accuracy; and if we may venture to point out a few matters to which in our view some exception might be taken, we hope it may be looked upon as only the fulfilment of his own express desire to receive friendly communications of this nature.

A comparatively undeveloped point, we venture to think, in the programme, is the very brief notice that has been taken of the theory of the tides. Granted that its minuter details are affected by some complicated considerations, its general outline admits of easy explanation, and is at the same time the cause of occasional misconceptions which ought to be removed; and it would be probably considered by many persons an improvement if the larger space allotted to it were obtained at the expense of the refutation of the fallacy of the exploded Ptolemaic system.

We do not meet with any reference to outbursts of light on the surface of the sun; so interesting a proving that the brilliancy of the photosphere may be far outshone, and so suggestive as to their possible origin.

The author's usual lucidity is scarcely exemplified in the explanation of phases in p. 63, where we venture to think a more familiar treatment might have been adopted.

The larger map by Beer and Mädler, notwithstanding its able reduction by Neison, might have found place in the enumeration of aids to selenographical study.

There seems a little confusion on p. 77 between Sir W. Herschel's idea that Aristarchus and some other spots visible in the earthshine were volcanoes in actual eruption, and the observations by Schröter and others of minute illuminations on the dark side, which seemed to point to an unreflected origin, and are still, unlike the former, not accounted for.

With regard to Mercury, we feel it right to say that Sir W. Herschel's failure to confirm the statements of Schroter may not be entitled to much weight; as is sufficiently indicated by their controversy in the *Phil. Trans.* respecting the phenomena of Venus. As far as this latter planet is concerned, it may be concluded, without accepting the measures of Schroter, that the irregularities witnessed by many observers prove the existence of elevations much more considerable than any upon the earth: as to Mercury, notwithstanding Schröter's deficiency as an artist, and his occasional mistakes of preconception, his observations are always too honest and faithful to be set wholly aside; and we are not sure that the uselessness of devoting time to this planet may not be found a mistake at some future day.

As to the physical condition of Mars, we venture to think that our author has dealt very fairly and judiciously with a subject of controversy, which might have become less pleasant but for the unassuming modesty of Schiaparelli and the liberal candour of Green, so honourable to each of them. We are not sure that it is always borne in mind, how much of the difference may have been due to the early return of the English observer from Madeira to a far inferior climate, previous to the development of the additional features which were subsequently perceived at Milan, and which may possibly, like their strange gemination, become more visible from prolonged solar influence. The less favourable position of the planet at the next opposition is much to be regretted; but Schiaparelli's experience has warned us that increase of distance may possibly be compensated by improvement in definition: to which we would add on the one hand the constantly verified adage of Sir W. Herschel, that "when an object is once discovered by a superior power, an inferior one will suffice to see it afterwards"; and on the other, the advantage which may be expected from the 18 inches of aperture with which the Italian Government are about to mark their appreciation of their astronomer's ability, and their willingness to enable him to meet the emergency. It will be matter of regret, if in this honourable contest no corresponding preparation should be made among ourselves; though it is difficult indeed to counteract the disadvantage of the English sky. It is not easy to forecast the result; but we think there are indications that possibly the supposed terrestrial analogy has been pushed quite far enough. As to the interesting question of the habitability of Mars by beings like ourselves, it deserves more attention than it perhaps has often received, that none of the supposed correspondence with our own constitution could be maintained excepting on the supposition of a higher internal temperature on the globe of Mars, or possibly a very different composition of atmosphere. We are not so much struck as the author with the progressive diminution of the measured diameter of Mars effected by the employment of modern instruments; at least Schröter's determination by the mode of projection in 1798 scarcely exceeds by 0″·5 that adopted by Newcomb for 1850. Irradiation no doubt is a fact; and a very troublesome one; but we suspect that its effects have been sometimes over-estimated, or mixed up with those of diffraction; and possibly the subject might bear further investigation.

As to the internal heat of Jupiter, so interesting an inquiry ought not to have been left so long in abeyance. If it exists, it would hardly be less capable of detection than that of Arcturus; and the bolometer of Langley seems to offer a fair chance for the discovery. The satellite whose strange reappearance is so difficult of explanation was, it will be found, about to enter on transit instead of suffering occultation. It may be noted, *en passant*, that a telescope must have had a marvellous power of indistinctness, that could show M. Flammarion the third satellite with a disc as large as that of Uranus (p. 409).

It seems a pity that the traditional misrepresentation of the ball of Saturn, at p. 358, as carrying a faint shadow on one side, should still be adhered to; and we may venture to suggest that there is a good deal of inequality in the execution of the diagrams in various parts of the book.

We are confident that the author will not misunderstand our remarks, or hesitate to accept our assurances that they are made in the most friendly spirit. If we are in error, he is fully able to hold his own; and he has our cordial wishes not only for his success on the present occasion, but for the extension of his labours, at no distant time, to a wider review of the glorious works of Nature.

THE ZOOLOGICAL RECORD

The Zoological Record for 1881. Being Vol. xviii. of the "Record of Zoological Literature." Edited by E. C. Rye. (London: John Van Voorst, 1882.)

IT is gratifying to be able to announce that the persevering efforts of the editor of the *Zoological Record* to publish the record of one year's work before the termination of the next year have been at last crowned with success; nor do we doubt but that this very desirable effort will be continued, and indeed become even less difficult with the advance of time, so that through the good will of the Recorders the date of publication will recede backwards from December to September or August in each year, enabling the worker to begin his autumn session with the volume in his hand. The facilities of intercourse are now so great with all parts of the world, that the Transactions and Proceedings really published in the month of December in any one year can be, nay are on our bookshelves in these British Isles, long ere the spring is on its wane, and no doubt long ere 1882 was out, some of the Recorders of this very volume had the record for that year well in hand. However grea may be our expectations for the future we cordially welcome this present volume, and acknowledge that our thanks are due to both Editor and Recorders for what they have already done.

To those who have time for reflection and dare to look back over those eighteen years since Dr. Günther and his friends launched this work upon the world of science, the thought naturally arises of the vastness of the amount of work that is year after year being accomplished without apparently in any way leading to exhaustion. The Editor's own comments are naturally in the volume very few, but how full of meaning is the following: "This volume is 36 pages longer than its predecessor," that is, even the very enumeration of the zoological literature of 1881 requires about 800 closely printed pages; and again we read, "the number of new genera and sub-genera contained in the present volume is 1438"—a simply appalling number. The Insecta are credited with 543, and the Protozoa with 517 of these genera. An enthusiastic zoologist once contemplated the posting up to date of Agassiz's "Nomenclator Zoologicus," that was when the generic increase was some 400 to 500 a year; what would he say or think now of these new births at the average of over 1000 a year. The "examination of this large number of new names, as regards prior occupation," the Editor states was necessarily superficial, we quite sympathise with him; before we read his footnote we rushed into the subject with the A's, but on turning over to page 2 we saw how matters stood and we gave the critical business up at once, and it was obvious at a glance that the greatest genus maker of the year was Ernest Haeckel.

The year 1881 showed a lull so far as the works on recent Mammalia were concerned—at least in comparison with 1880—but the flood of new extinct mammalian forms from North America shows no sign of abatement. In 1881 the lamented Balfour completed his excellent and masterlike treatise on Embryology. The account of the Mammalia in Messrs. Salvin and Godman's work on the Biology of Central America has been finished, and Peters and Doria have published an important work on the Mammals of New Guinea.

The contribution to Bird Literature has been considerable, and the year was marked by the appearance of two more volumes of the Catalogue of the Birds in the British Museum (vols. v. and vi.). Among the Reptiles, Batrachians, and Fishes, no work of any very special importance seems to have appeared. Dr. von Martens still records the Mollusca and Crustacea. The record of the former group extends to 108 pages, and of the latter to 38 pages; both are most painstakingly executed.

The literature of the Arachnida is more extensive than usual, and the year's work is marked by the appearance of several important contributions by the Recorder, Holmberg, Karsch, Keyserling, Koch, Pavesi, Simon, and Thorell, so that it is evident that the Arachnid treasures of the world are at last being worked. Among the Myriopods, Cantoni's Monograph of the Lombardy forms seems to call for notice.

The enormous group of Insecta is recorded by Mr. Kirby, with the exception of the Neuroptera and Orthoptera, which fall to the skilled hands of Mr. McLachlan.

The Vermes and Echinoderms are recorded by Prof. Jeffrey Bell; the Cœlenterata by A. G. Bourne and Sydney J. Hickson. It is remarkable that not a single new genus or species of any recent Octactiniæ seems to have appeared in 1881, nor indeed any separate paper on the group. The Sponges and Protozoa have engaged the attention of Stuart O. Ridley; while nothing very striking seems to occur among the Sponge literature. Kent's Manual of Infusoria, and Haeckel's Prodomus of the Radiolaria mark the year; among the Protozoa, the latter work records 483 new genera and 2000 new species—an almost embarassing number of pretty things.

We are truly glad that the importance of this Record is still practically witnessed to by the generous help rendered to the Zoological Record Association by the British Association for the Advancement of Science and by the Grant Committee of the Royal Society.

OUR BOOK SHELF

The Brewer, Distiller, and Wine Manufacturer. (London: J. and A. Churchill, 1883.)

THE little work before us is the first of a series of technological handbooks to be issued by the publishers, "each of which will be complete in itself, will appear in a handy form and at a low price." Practically they will be a re-issue of articles in Cooley's "Cyclopædia of Practical Receipts and Collateral Information in the Arts, Manufactures, Professions, and Trades," with a somewhat fuller treatment and with reference to the more recent developments which have taken place in industrial processes. As this, the first of these handbooks, treats of Alcohol and Alcoholmetry, Brewing and Beer, Cider, Liqueurs and Cordials, Distillation of Alcoholic Liquors, and lastly, Wine and Wine Making, necessarily much of the Encyclopædic form of treatment must remain, when such important industries are discussed in so small a compass.

Though we cannot endorse the statement of the publishers, that each handbook will be *complete* in itself, we are compelled to admit that the Editor has given a remarkably well condensed *précis* of what has been written on industrial fermentation processes.

The first chapter describes the sources of alcohol, its detection in liquids, and its estimation by volume and by weight: numerous tables are given for this purpose. In addition to the usual distillation process, the methods of Balling, Groning, Brossard-Vidal, Silbermann, Geissler, and others are described; this part of the book must prove of much use to the technologist.

Brewing is described in fifty pages; this is sufficient to show how condensed the treatment of one of the largest industries in the kingdom must necessarily be. Brief though it be the Editor deserves the highest praise for the manner in which he has condensed the vast mass of facts now accumulated in this department of fermentation chemistry. In addition to the description of the English processes of malting, mashing, and fermentation, a brief account is given of the German Lager beer system now almost universal on the continents of Europe and North America. This is supplemented by a large number of elaborate analyses of English and "Lager" beers, showing the characteristic differences in the products of the two methods. Brief though this part of the handbook is, it will be found of interest to the general reader and of value to the practical brewer, who may not have hitherto given much attention to the scientific part of his manufacturing process.

In Chapter V. we have a full account of the mashing and fermentation processes adopted by the distiller and rectifier, including the methods followed by the latter to remove some of the fusel oils and to flavour "still" spirit so as to produce gins, whiskies, &c., of various taste and aroma. A useful feature in this part of the work will be found in the descriptions and drawings of the stills of Coffey, Siemens, Derosne, Laugier, Dorn, Pistorius, Pontifex and Wood, and others; this will be found of much interest to distillers, more especially in our colonies, where technical information is more difficult to obtain than in the old country.

The sixth and last chapter treats of Wine and its Manufacture.

After a brief description of the soils and manures best suited to the culture of the vine, we have an enumeration and description of some of the better known wines, such as Lafitte, Latour, Margaux, Haut Brion, Leoville, and other red wines of the Gironde, and of the White Graves, as Sauterne, Barsac, Château Yquem, Latour, &c.; of the Burgundies, Romanée Conti, Chambertin, Clos Vougeot, Clos St. George, and La Tache, and of some of the wines of the Champagne, Beaujolais and other vine districts of France.

A brief account is given of the so-called Hocks of the Rhine, and those of the valleys of the Moselle, Ahn, and other rivers of Germany. In the description given of wine-making some use is made of the invaluable treatise by Messrs. Dupré and Thudichum ("On the Origin, Nature, and Varieties of Wine," Macmillan), a work which will amply repay the technologist who consults it.

In conclusion we may add that this little book, though far from being "complete," or exhaustive of any one subject it treats of, is yet compiled with great care and discrimination by the Editor, and will be found of much value by those unable to consult larger treatises or original papers.

C. G.

Il Potentiale Elettrico nell' Insegnamento Elementare della Elettrostatica. Per A. Serpieri, Prof. di Fisica nella Università e nel Liceo Raffaelo di Urbino. (Milano, 1882.)

THIS treatise is an elementary exposition of the theory of the Potential in its application to Electrostatical Phenomena. It is founded, as we learn from the preface, on the author's lectures at the Raphael Lyceum of Urbino; and is intended for the use of the Lyceums and Technical institutes of Italy. It is well known to all who interest themselves in such matters that a promising young school of physicists has recently been springing up in Italy, and that those who wish to be abreast of their time can no longer neglect the Italian scientific literature. If the treatise of Prof. Serpieri may be taken as a fair specimen of the scientific instruction given in the secondary schools of Italy, it is clear that this harvest of physicists is due in no small degree to careful sowing.

The work deserves its title of Elementary, inasmuch as nothing is demanded of the student beyond a knowledge of elementary geometry and algebra, and a slight acquaintance with trigonometry The author is mistaken, however, in supposing that an elementary treatment of electrical theory has not hitherto been attempted; for the English work of Cumming, published some six years ago, is almost identical in its aims with his own Although Cumming's treatise is an excellent one in many ways, we cannot help thinking that the Italian one is better fitted for the purposes of elementary instruction. Prof. Serpieri appears to us to have happily kept the middle way alike between poverty and redundance of matter; and between excess of mathematical and excess of merely experimental detail.

In the first four chapters are developed the relation between potential and charge, and the theory of lines of force and equipotential surfaces. The fifth, sixth, and seventh chapters contain the theory of capacity, of electrostatic induction, and of the measure of potential. The eighth chapter contains a short sketch of the centimetre-gramme-second system of units, now universally adopted in accordance with the decision of the Electrical Congress at Paris; farther details on this all-important matter are given in one of the appendices, and a considerable number of numerical examples is provided to familiarise the student with the practical use of the system. The last seven chapters are devoted to the theory of condensers. Not only is the theory explained in a simple and interesting way, but abundance of experimental results and numerical illustrations are given to enable the learner to judge how far the mathematical theory represents the actual facts. The account of the experiments of Villari on the heat developed in the electric spark under various circumstances is interesting, and would probably be new to most English readers.

The main fault we have to find with Prof. Serpieri's work is that he has a tendency to cite second-hand authorities where it would have been quite as easy, more instructive for his youthful readers, and *more just* to give the original sources. Again, why of all the results concerning specific inductive capacity should he quote (p. 69) those of Gordon only, which have been precisely the most questioned, and why on the same page should the results of Boltzmann for the specific inductive capacity of gases not be coupled with the name of their author?

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Hovering of Birds

IN your last number I observe an interesting letter on the "Hovering of Birds," by Mr. Hubert Airy. In that letter he refers to an opinion which I have expressed, that this "hovering" cannot be accounted for by the mere supporting agency of an upward current of air. The writer quotes this opinion as it was expressed in a letter to you (NATURE, vol. x. p. 262). But he does not seem to have read the fuller explanation which I have given on this subject in Chapter III. of the "Reign of Law." To that chapter I must refer your correspondent for an explanation, which shows that hovering can be, and is perpetually accomplished under the ordinary conditions of horizontal currents of air. It is very commonly performed (especially) by the whole tribe of Terns, or sea-swallows, over the surface of the sea, where there are no hills or mountains to deflect aërial currents from the usual horizontal course.

Mr. Airy himself uses words which indicate that this agency of upward currents is quite superfluous. He says: "It is easy to see that a bird, with the exquisite muscular sense that every act of flight demands and denotes, might so adapt the balance of its body, and the slope of its wing-surface to the wind, as to remain motionless in relation to the earth." He prefaces these words by these others: "given such a slant upward current." But no such "gift" is needed. The bird has only to slope his wing-surfaces to the current, and precisely the same effect is produced as if the current had been otherwise "sloped" upwards against a horizontal wing-surface. Mr, Airy's own letter contains an excellent explanation of this correspondence.

Cannes, France, January 29 ARGYLL

WITH respect to Mr. Hubert Airy's interesting note (vol. xxvii. p. 294), I beg to say that I have very often seen the kestrel hovering over the perfectly level meadows of Middlesex with obvious ease, where no undulation of the ground could possibly affect the currents of air. Of the twelve instances Mr. Airy enumerates, I see only six refer to hawks (species undetermined), so this fact must be taken into consideration; the conduct of rooks and crows under such circumstances seems to me to come under quite a different category from that of hawks, and in some instances gulls, thus "prospecting" for their prey. Mr. Airy does not ignore this aspect of the question, but I think that by confusing objective with subjective "hovering" he complicates his theory.

HENRY T. WHARTON

39, St. George's Road, Kilburn, N.W., January 27

Action of Light on India-rubber

IT may be in the recollection of some of your readers, that in 1876 I pointed out that the deterioration of ebonite surfaces was due to the combined action of light and air. Some time afterwards it was remarked to me that our laboratory (an old greenhouse) was too light, and as a result all our india-rubber tubes would rapidly deteriorate. This led me to submit some pieces of ordinary black india-rubber to the same treatment as the ebonite in the former experiments. On October 11, 1879, four pieces of caoutchouc connector of 5 mm. internal diameter were taken, two were placed in test-tubes plugged with cotton-wool, and the remaining two inclosed in hermetically sealed tubes. One of the sealed tubes, and one of those plugged with cotton wool were placed in a dark drawer, and the other pair in the laboratory window, with a north aspect, and in such a position that they were not under the influence of direct sunlight in the summer. To-day the specimens were examined. Both the sealed tubes were found to be slightly moist inside, and on opening them an organic odour, like that of an india-rubber shop, was perceived. The caoutchouc which had been exposed to air and light, was covered with a thin brown coating, and on being bent this coating cracked; the end which had been most exposed to the light was rather brittle, and could not be stretched without splitting. The other three specimens were unaltered. All four specimens were slightly acid to test paper, but the quantity of acid was too small to be determined.

Mareck (*Chem. News*, xlvii. 25, from *Zeitschr. für Anal. Chem.* xxi.), has lately recommended the preservation of caoutchouc tubes, by keeping them in water when not in use. This is, no doubt, efficacious in consequence of the exclusion of air.

Cooper's Hill, January 22 HERBERT McLEOD

A Possible Cause of the Extinction of the Horses of the Post-Tertiary

A TRAVELLER in the Park region of northern Colorado, and the central portion of Wyoming, fifteen years ago, could not fail to notice the immense numbers of skulls and other bones of bisons in districts at that time no longer frequented by these animals. Scattered specimens were to be seen in all directions, some of them bearing marks of bullet and knife which left no doubt as to the agent of destruction. Others were to be found in numbers in localities which suggested that they had been surprised by death while seeking shelter from the weather rather than the human destroyer. In such cases, tumbled and mixed by the scavengers, they were thickly strewed over small areas, and the contour of the surface often was such as to bring them closer together with the movement of water or soil. When asked the cause of the wholesale slaughter, the reply of the natives was almost invariably "the hunters killed a great many, but the most died in the deep snow and cold weather some twenty-five years ago."

The great losses experienced by the cattle men of the Medicine Bow and Elk Mountain region, only a couple of winters ago, are too recent to have been forgotten. The next spring and summer the unfortunate owner found the carcases of his cattle in positions similar to those occupied by the bands of bisons. In small parties they had huddled in sheltered basins or nooks, and some, upheld by the snow through the winter. were still on their feet. Since then these "bone yards" have become similar in appearance to those of earlier date.

Last summer the kindness of Prof. Agassiz enabled me to make some discoveries in the Mauvaises Terres of the eastern slope of the Rocky Mountains which vividly brought to mind the pockets full of recent skeletons. Sections in the Post-Tertiary beds here and there disclosed groups or herds of fossil horses (*Equus*) in circumstances so similar as to leave no alternative to the conclusion that the same causes had filled the bone basins in the olden and in most recent time.

Stripped of the strata above them, the contour of the surface would have been similar, and the old-time Coyotes in their feasting had evidently brought about an equal amount of confusion in the remains. About the time of the deposition of these fossils the horses became extinct. *Why* is still an open question. Such evidence as was gathered there has led to the belief that, in that region at least, occasional "cold waves" of days—perhaps weeks in duration, which deep snows caused, or were the principal causes of the extermination of the horses. Other causes that may be suggested are these: lack of water, and an extended glacial period. A consideration of the character of the deposits, the drainage of the mountains at the time, the absence in these beds of proof of a glacial period affecting them since, and the continued existence in the same locality of other creatures, somewhat less sensitive to the cold, would seem to be sufficient objections to their acceptance. The tradition of the Indians, that there is a winter of terrible destruction to the animals once or twice in the lifetime of a man—say once in about forty years—appears to be confirmed by the testimony of the whites. A few degrees or a few days added to the measure of the "wave," or "blizzard," and a few inches added to the depth of the snow would suffice to sweep the herds from the pastures. Weather of this character is a possibility every winter in the Bad Lands, though we hardly expect it. Apparently the rocks contain evidence of such weather in post-Tertiary times. And it may not have differed so very much from that we are having to-day. S. GARMAN

Cambridge, Mass., U.S., January 12

Suicide of Scorpions

SPEAKING of scorpion suicide, Mr. G. J. Romanes in his "Animal Intelligence writes: "Still I think that so remarkable a fact unquestionably demands further corroboration before we shall be justified in accepting it unreservedly" (p. 225). Some years ago I made some experiments and observations on a smaller and a larger species of scorpion found on the Cape Peninsula. I am unable to ascertain the specific names; the smaller are found beneath the bark of decaying tree-stumps, the larger, which often weigh upwards of seventy grains, are found beneath stones and ant-balls. I have recently resumed these experiments and observations. The conclusion I come to is that neither of these species have any suicidal instinct. Only in one case have I found, after death, any sign of such a wound as the sting might inflict; in this case, though one of the tergal plates showed a largish irregular fracture, the wound did not seem a fresh one, and was dry and apparently skinned over; in this case, too, though I watched the death of the scorpion (caused by the gradual application of heat to the bottom of the glass vessel in which the creature was inclosed), I was not able to detect anything like the act of suicide. I will now briefly describe the nature of my experiments.

1. Condensing a sun-beam on various parts of the scorpion's body. The creatures always struck with the sting round, across, and over the heated spot, and seemed to try and remove the source of irritation.

2. Heating in a glass bottle. As this admits of most careful watching, I have killed some twenty or thirty individuals in this way. The creatures very commonly pass the sting over the body as if to remove some irritant. The poison exudes from the point of the sting and there coagulates

3. Surrounding with fire or red hot embers. I first took a newspaper, moistened a ring about a foot in diameter with alcohol, and placed a scorpion within the ring. The paper was, by this time, ignited. He walked without hesitation through the fire, and tried to make his escape. I made a ring of red-hot wood-embers, and placed a scorpion in the middle. He pushed his way out, displacing two of the embers. I made a better fire-wall, and put him in the middle again. He crept over the embers. I placed him in the midst of a ring of embers on the flat and much-heated stones of the fire-place. He crept over the embers again, but this time got baked before he could escape.

4. Placing in burning alcohol. I placed a layer of an eighth of an inch of alcohol in a shallow vessel, lit the alcohol, and placed the scorpion in the midst of the burning spirit.

5. Placing in concentrated sulphuric acid. I moistened the bottom of a large beaker with a very thin layer of concentrated sulphuric acid, and put in a scorpion. The creature died in about ten minutes. (I have also tried other strong acids, a concentrated solution of sodium hydrate, and a potassium cyanide solution.)

6. Burning phosphorus on the creature's body. I placed a small pellet of phosphorus near the root of the scorpion's tail, and lit the phosphorus with a touch of a heated wire. The creature tried to remove the phosphorus with its sting, carrying away some of the burning material.

7. Drowning in water, alcohol, and ether.

8. Placing in a bottle with a piece of cotton-wool moistened with benzene.

9. Exposing to sudden light. I have not tried special experiments as to this point, but have, on turning over an ant ball, suddenly exposed a scorpion, hitherto in complete or almost complete darkness, to the full glare of South African sunshine.

10. Treating with a series of electric shocks.

11. General and exasperating courses of worry.

I think it will be admitted that some of these experiments were sufficiently barbarous (the sixth is positively sickening) to induce any scorpion who had the slightest suicidal tendency to find relief in self destruction. I have in all cases repeated the experiments on several individuals. I have in nearly all cases examined the dead scorpion with a lens. My belief is that the efforts made by the scorpion to remove the source of irritation are put down by those who are not accustomed to accurate observation as efforts at self-destruction. On one occasion I called in one of my servants to watch the death of a scorpion by gradually heating it in a glass bottle. The creature at once began moving its sting across and over its back, upon which my servant exclaimed, "See it is stinging itself." I do not wish to imply that all the cases of alleged scorpion suicide are merely instances of careless observation. All I wish to do in this note is to record my individual experience, and to state clearly that after making a series of observations as carefully as I could on a large number of individuals, I cannot place on record a single instance of clear and unmistakable scorpion suicide.

Rondebosch, January 1 C. LLOYD MORGAN

Mimicry in Moths

I HAVE read with great interest the observations of the Duke of Argyll on Mimicry in Moths. I remember more than one similar occurrence during my travels. The most curious was as follows:—

Whilst in Japan, a messmate brought on board, in an ordinary pot, a beautiful trained shrub with a leaf much resembling that of an orange. It was placed on the ward room table where we all sat, the steward removed it from the table to the top of an harmonium at least three times a day, and I watered it when required, and often examined and admired it; in about eight or ten days it began to show signs of failing; and, thinking it might be infected with spider or green fly, I examined it carefully, and in doing so I disturbed a large green smooth-skinned caterpillar. Like all animals on board ship he soon became a great favourite, and we often asked strangers to point him out and in no case did they succeed.

He always lay along the edge of the leaf, with his head to the point and eat at each bite, exactly the breadth necessary to preserve the contour of the leaf as far as possible, when he reached the point, by a few sharp convulsions he returned to the stem and began another row. When he had finished one half of the leaf he began the other; and when nothing but the centre rib of the leaf was left he eat backwards along the stem. He was the most economical feeder I ever saw, only a very little bit of the centre rib of the leaf was bitten off and fell to the ground, and the hard stem of the leaf was left.

I soon observed that he could assume the exact shade of the leaf he was feeding on, and I frequently shifted him and watched the process.

In due time he assumed the chrysalis form; he partly suspended, partly glued, himself to the stem of the plant and it was very difficult to detect him; but not nearly so difficult as in the caterpillar state.

He remained a very short time in the pupa, and one day I was called by a messmate who informed me that "My beastly bug had hatched out," and at first I thought this was the case, as a beautiful black and gold butterfly was expanding his wings and legs on the table, and soon took wing, but was captured and handed over to our bug collector, who by the way took no interest whatever in the prior stages; he was neither butterfly, moth, nor beetle, so nothing to him.

I went to observe how he had broken out of the sheath and was astonished to find that my chrysalis was safe and sound, the butterfly we had certainly did not come from it. Then where did it come from? We were still in Yokohama harbour, and it was a common occurrence that insects flew off to the ships. But how did a butterfly in the state I saw this one get on the ward room table? I came to the conclusion that the pupa had been attached to the plant or pot; I did not anticipate what took place. In a few days another butterfly, to all appearance the brother of the first one, was seen (but not by me), to emerge from the chrysalis we had at first observed; and I have no doubt the first insect had eluded all our prying, and that there were two caterpillars all the time on the plant.

I do not get NATURE until it is a fortnight old, and I have waited with anxiety to see if any one better able than I am would endeavour to show that mere physical causation is sufficient to account for all the phenomena disclosed by the Duke's admirable observation of the moth.

I look upon the Duke as one of the best observers of Nature we have, and his opinions must carry great weight; and believing as I do that in the Theory of Natural Selection the future existence of our race and all hope of advancement in morality is bound up, I am anxious that his doubts on this subject should not carry weight with others.

I think the whole question lies in this—were either of these caterpillars, or the Duke's moth perfect, or even the most perfect of their kind?

I believe I have had more opportunity of observing cases of mimicry than his Grace has, and I have always found that the individuals vary as much in these forms of life as in any other. At Labuan one of the Engineers of the coal works sent a native out and in half an hour he returned with seven leaf-insects. I had picked one up in my walk from the settlement, and although at first each appeared a perfect leaf to my eye, I soon found great differences between the individuals; some being much better specimens than others—just as all sheep are not sheepish to the shepherd—and I think it is quite possible that not one of these eight insects would deceive the eye of an average natural enemy. Let us suppose that anyone of these were so perfect as a mimic, that it would deceive this enemy, it might be wanting in the advantage of perfect rest whilst under inspection, and thus be detected. It was by the movement of the insect that I was enabled to get the one I picked up. The Duke's moth was betrayed by his "beaded eyes and thorax;" and last of all, there was a small hole in the covering of the bright wings, which the Duke considers one of the mysteries of nature, and through all the mimicry of this moth the Duke with very little trouble detects the imposter; as far as he was concerned, all the effort of nature was wasted. If I may be allowed the paradox, it is only when one has come to see what a botch nature has made of its work that its beauties can be properly appreciated. I admire quite as much the quickness of eye that belongs to the lizard that may have been on the watch to capture the moth; these "mysteries" have gone on together; and where a moth or a lizard failed ever so little it went down whilst its better appointed brother was the fittest to survive. Until the mind has taken in how constant the battle is, how small the advantages must be when the enemy is travelling the same path, it is difficult to resist the feeling of wonder and the desire to account for all by a fiat of creation.

I remember some remarks by the Duke of Argyll in a similar strain, when he observed three water-ouzels take the water for the first time. He was struck with the way in which they all dived and swam, so perfectly; but I think he failed to consider this view of the matter—did any one of these surpass the others in the art, even were his advantage so little that the Duke was unable to detect it? if so, then provided he was equal of his brothers in all other respects, he was the fittest to survive; and as we evolutionists only claim little by little; its ordinary phrases are no lean and empty formulæ to me.

Nothing but the conviction that, in the new light thrown on nature by Charles Darwin and his numerous disciples, lies the happiness or misery of our race, would have emboldened me, so indifferently educated for the task, to take up the subject and your time. DUNCAN STEWART

Knockrioch, January 25

Clerk-Maxwell on Stress

CAN any of your readers give me a reference to the note in which Maxwell, commenting on or replying to a correspondent of NATURE, gave his ideas as to the nature of stress in a beam or cord? T.

The Comet

MAY I ask space to make some observations about the orbit of the Great Comet of 1882?

Looking on the many elements published in NATURE, in the *Dunecht Circulars*, and in the *Astronomische Nachrichten*, I find very great differences between one and another. Especially the elliptical elements calculated by Mr. S. C. Chandler, Mr. Frisby, Mr. Kreutz, and Mr. Morrison present periods peculiarly different.

Now this fact can be produced but by two causes; either it may be that the different observers considered different parts of the nucleus as the brightest part; or it may be that the movement of the comet has been much perturbed by some bodies of the solar system.

The first hypothesis is very probable, as you remark in the "Astronomical Column" in NATURE, vol. xxvii. p. 300.

The division of the head in two, and perhaps three portions, is a fact well observed by many astronomers, and well shown in the drawings published by Mr. A. A. Common, Dr. Doberck, and Mr. W. T. Sampson in NATURE, vol. xxvii. pp. 109, 129, and 150.

But I observed that with small magnifying power the appearances of the brightest part of the head maintained always a certain unity, which would not admit great mistakes in the observations. Therefore it seems to me that, unless we suppose considerable and unknown variations in the form of the nucleus, only the difference of appreciation of the point observed can hardly explain such a great, and I say regular, difference between one orbit and another.

I say *regular difference*, because I remark a certain peculiarity.

The first elliptical orbit calculated by Mr. J. C. Chandler, using observations from September 18 to October 20, gave a period of about 4000 years.

Afterwards Mr. Kreutz, using observations from September 8 to November 14, gave a period of 843 years, and lately Mr. Morrison, keeping observations from September 19 to December 11, has an elliptical orbit with only 642·5 years.

This fact induces me to believe that an accurate study of the perturbations of the motion of this comet may be as important as it was for Biela's comet.

It is my purpose to go, as far as I can, through a complete discussion of all the observations, and I shall be very glad if those of your readers, who are possessors of good unpublished remarks both about the appearance and about the positions of the comet, would kindly let me know of them.

E. RISTORI

13, Pembridge Crescent, Bayswater, W., January 30

The Aurora of November 17, 1882

I SHOULD like to ask H. J. H. Groneman whether he tried to find out if a curved path for the auroral beam would agree better with the observations than a straight one; because, if it was purely an auroral phenomenon, we should naturally expect its path to be a curve, maintaining a uniform height above the surface of the earth, and to be approximately a small circle having its centre at the magnetic pole, this being the ordinary position of the auroral arches. Of course the motion of the parts of the arch is often not exactly in this direction, because the arch has frequently a transverse motion in addition to the movements that take place longitudinally; and if there was any such transverse motion in the case of this beam, that would prevent its moving strictly along a parallel of magnetic latitude, though it is hardly likely it would deviate far from it. It would be well to ascertain whether such a motion would not agree better with the observations of the beam than Dr. Groneman's hypothesis that it was in a straight line; for the establishment of a curved motion would do away with the idea that the phenomenon was caused by a meteor.

In the other cases cited by Dr. Groneman of supposed meteoric masses passing through our atmosphere and producing auroral effects, the paths, so far as given, seem all to have been approximately along the parallels of magnetic latitude, which circumstance militates against their having had anything to do with meteors, because these traverse the atmosphere in all directions, and would be just as likely to go in a northerly or southerly direction as in an easterly or westerly one. Possibly, however, Dr. Groneman's theory may be that meteors only produce an auroral effect when they happen to go in such directions as may be calculated to produce it.

Sunderland, January 29 THOS. WM. BACKHOUSE

As Dr. Groneman in his most interesting paper on the phenomenon of November 17 asks for my authority for the Swedish observation, I may say that I merely saw it in the "Notes" in NATURE (vol. xxvii. p. 113). There seems a misprint in that statement, however, as "Eskilstuna, fifty-four miles south of Stockholm" would be in the sea, whereas Eskilstuna is fifty-four miles *west* of Stockholm.

As the spectroscope observation is said to put the auroral nature of the "spindle" beyond doubt, I would observe that until we know that gas excited by the passage of particles through it at fifteen miles a second does not give the same spectrum as when incandescent by an electric discharge, the observation of certain lines cannot prove anything of the exciting cause. Further, a good deal of the light might be reflected sunlight, as that would be scattered over the whole spectrum, and would thus be masked by the faint diffused spectrum of the moonlight at the time.

W. M. F. PETRIE

Bromley, Kent

REFERRING to Dr. Groneman's communication, possibly it may be of service to say that at 9 p.m., October 14, 1870, besides some ruddy auroræ, chiefly in the west and north, I saw a band having a very close resemblance to that figured in the illustration, p. 297. It, however, stretched all the way across the sky from west to east, and continued for some time without much apparent alteration in figure or locality. An appointment called me away before it had vanished.

HENRY MUIRHEAD

Cambuslang, January 26

The Sea Serpent

I HAVE seen four or five times something like what your correspondent describes and figures, at Llandudno, crossing from the Little Ormes head across the bay, and have no doubt whatever that the phenomenon was simply a shoal of porpoises. I never, however, saw the *head* your correspondent gives, but in other respects what I have seen was exactly the same; the motions of porpoises might easily be taken for those of a serpent; once I saw them from the top of the Little Orme, they came very near the base of the rock, and kept the line nearly half across the bay.

JOSEPH SIDEBOTHAM

Erlesdene, Bowdon, January 26

Influence of "Environment" upon Plants

REFERRING to Prof. Thiselton Dyer's letter on the above subject in NATURE (vol. xxvii. p. 82), it may interest your readers to know that I have had several trees of *Acacia dealbata* 30 feet high, in the open air, in flower for ten days past, but not so fully as they will be in a fortnight's time. I have had *Desfontainea spinosa* in flower during the past eight months; this shrub is 6½ feet high, and also in the open air.

Rosehill, Falmouth, January 29 HOWARD FOX

THE PEAK OF TENERIFFE ACTIVE AGAIN

A PRIVATE letter which I have just been privileged to see, from a native lady in Santa Cruz to her sister in this country, tells how the inhabitants of that present capital of Teneriffe had remarked for several months past, that there was no snow on the upper part of the Peak; though all the "Cumbree," or moderately high land over the rest of the island, was whitened with it in the usual manner for the season. But within the past few days, "fire, like three great bonfires" had been seen on the summit of the Peak, and a lava stream had begun to flow down from it.

Now this is interesting both chronologically, and chorographically. Chronologically, I had remarked at p. 150 of my little book "Teneriffe an Astronomer's Experiment," (published in 1858), that the lava eruptions there only break out about once in a century; the last eruption having occurred in 1798, and the previous one in 1703; and now we have one in 1883, but in what part of the mountainous island called Teneriffe has this last eruption appeared?

So far as I can gather from the said private letter, it has issued, if not from the very mouth of the craterlet which forms the tip-top of the Peak, yet from its sides or foot where it stands on a filled-up crater of much larger size, otherwise to be looked on as the Peak's proper and effective summit; and it is from that crater's lips that have proceeded all the later, and yet prehistoric, streams of black lava, which score and frill the Peak on every side; and contrast so strikingly with the far more ancient red, and the still more ancient, more abundant, and once hotter yellow streams from the older and larger craters lower down, before ever yet, the Peak, or final cinder heap, was formed.

But though in the Nature-primeval history of the Mountain, the black, unoxydised lava streams of the Peak, were its latest exudations, still nothing more of that kind was locally expected to occur there within the human period. This was partly because no addition to them had been made since the Spanish Conquest; and partly because the lava outflow of 1798 avoided the Peak, and broke out on the Western side of the general mountain mass, while the eruptions of 1703, which threatened the town of Guimar to the south, and destroyed Garachico to the north, filling up its once beautiful bay—broke forth nearer the sea-level than the peak's top. Whence the idea arose, that the central vent of the peak must have clogged up with time, and that nothing more than its merry little jets of steam and sulphurous acid were to be looked for in that quarter; yet now we are told of red hot lava pouring forth.

Nevertheless on the whole, and in the long course of time, the forces of the grand old volcano may be dying out. For in an earlier work than any other that I had ever met with before about Teneriffe, I have lately read a very different account of the average state of the summit crater, to what it has been in, ever since the days of modern travelling and visitation began.

The book I allude to, in the possession of the Earl of Crawford and Balcarres, is an exquisitely illuminated MS. volume in vellum, by the Chevalier Edmund Skory, of the date of about 1582, and dedicated to that name so dear to all the students of Natural Science, viz.:

> "Sir Frances Bacon,
> "the knower and lover of all good Arts."

The very first dipping into its old MS. pages brought out a quaint proof of its antiquity, by its involuntary allusions to Garachico, as a city that was necessarily the island's chief delight and glory; the seat of its Government, the abode of its commerce, the place of all its shipping, and of course, because it was so prosperous, destined to live a queen for ever, and to be the joy of all peoples. Yet it is now, and has been for nearly two centuries as deserted as another Tyre; hardly fit to be the habitation of foxes, a mere howling wilderness of black rocks, for a few fishermen to spread their nets upon.

This happily preserved author then in the Earl's valuable library, who had abundant experience of Teneriffe more than a century previous to Garachico's Herculaneum fate, speaks of—

"Great stones being, with noyse, fyre and smoke, many "times cast forthe" out of the craterlet on the top of the peak.

Also that, "On the sommer time the fyers doe ofte breake forth from out the hole in the topp of this hill; into which, if you throw a great stone, it soundeth as if a great weight had fallen upon infinite store of hollow Brasse."

C. PIAZZI SMYTH

JOHANN BENEDICT LISTING

ONE of the few remaining links that still continued to connect our time with that in which Gauss had made Gottingen one of the chief intellectual centres of the civilised world has just been broken by the death of Listing.

If a man's services to science were to be judged by the mere number of his published papers, Listing would not stand very high. He published little, and (it would seem) was even indebted to another for the publication of the discovery by which he is most widely known. This is what is called, in Physiological Optics, *Listing's Law*. Stripped of mere technicalities, the law asserts that if a person whose head remains fixed turns his eyes from an object situated directly in front of the face to another, the final position of each eye-ball is such as would have been produced by rotation round an axis perpendicular alike to the ray by which the first object was seen and to that by which the second is seen. "Let us call that line in the retina, upon which the visible horizon is portrayed when we look, with upright head, straight at the visible horizon, the horizon of the retina. Now any ordinary person would naturally suppose that if we, keeping our head in an upright position, turn our eyes so as to look, say, up and to the right, the horizon of the retina would remain parallel to the real horizon. This is, however, not so. If we turn our eyes straight up or straight down, straight to the right or straight to the left, it is so, but not if we look up or down, and also to the right or to the left. In *these* cases there is a certain amount of what Helmholtz calls 'wheel-turning' (*Rad-drehung*) of the eye, by which the horizon of the retina is tilted so as to make an angle with the real horizon. The relation of this 'wheel-turning' to the above-described motion of the optic axis is expressed by Listing's law, in a perfectly simple way, a way so simple that it is only by going back to what we might have thought nature should have done, and from that point of view, looking at what the eye really does, and considering the complexity of the problem, that we see the ingenuity of Listing's law, which is simple in the extreme, and seems to agree with fact quite exactly, except in the case of very short-sighted eyes." The physiologists of the time, unable to make out these things for themselves, welcomed the assistance of the mathematician. And so it has always been in Germany. Few are entirely ignorant of the immense accessions which physical science owes to Helmholtz. Yet few are aware that he *became* a mathematician in order that he might be able to carry out properly his physiological researches. What a pregnant comment on the conduct of those "British geologists" who, not many years ago, treated with outspoken contempt Thomson's thermodynamic investigations into the admissible lengths of geological periods!

Passing over about a dozen short notes on various subjects (published chiefly in the Gottingen "*Nachrichten*"), we come to the two masterpieces, on which (unless, as we hope may prove to be the case, he have left much unpublished matter) Listing's fame must chiefly rest. They seem scarcely to have been noticed in this country, until attention was called to their contents by Clerk-Maxwell.

The first of these appeared in 1847, with the title *Vorstudien zur Topologie*. It formed part of a series, which unfortunately extended to only two volumes, called *Göttinger Studien*. The term Topology was introduced by Listing to distinguish what may be called qualitative geometry from the ordinary geometry in which quantitative relations chiefly are treated. The subject of knots furnishes a typical example of these merely qualitative relations. For, once a knot is made on a cord, and the free ends tied together, its nature remains unchangeable, so long as the continuity of the string is maintained, and is therefore totally independent of the actual or relative dimensions and form of any of its parts. Similarly when two endless cords are linked together. It seems not unlikely, though we can find no proof of it, that Listing was led to such researches by the advice or example of Gauss himself; for Gauss, so long ago as 1833, pointed out their connection with his favourite electromagnetic inquiries.

After a short introductory historical notice, which shows that next to nothing had then been done in his subject, Listing takes up the very interesting questions of Inversion (*Umkehrung*) and Perversion (*Verkehrung*) of a geometrical figure, with specially valuable applications to images as formed by various optical instruments. We cannot enter into details, but we paraphrase one of his examples, which is particularly instructive:—

> "A man on the opposite bank of a quiet lake appears in the watery mirror perverted, while in an astronomical telescope he appears inverted. Although both images show the head down and the feet up, it is the dioptric one only which:—if we could examine it:—would, like the original, show the heart on the left side; for the catoptric image would show it on the right side. In type there is a difference between inverted letters and perverted ones. Thus the Roman V becomes, by inversion, the Greek Λ; the Roman R perverted becomes the Russian Я; the Roman L, perverted and inverted, becomes the Greek Γ. Compositors read perverted type without difficulty:—many newspaper readers in England can read inverted type. * * * The numerals on the scale of Gauss' Magnetometer must, in order to appear to the observer in their natural position, be both perverted and inverted, in consequence of the perversion by reflection and the inversion by the telescope."

Listing next takes up helices of various kinds, and discusses the question as to which kind of screws should be called right-handed. His examples are chiefly taken from vegetable spirals, such as those of the tendrils of the convolvulus, the hop, the vine, &c, some from fir-cones, some from snail-shells, others from the "snail" in clock-work. He points out in great detail the confusion which has been introduced in botanical works by the want of a common nomenclature, and finally proposes to found such a nomenclature on the forms of the Greek δ and λ.

The consideration of double-threaded screws, twisted bundles of fibres, &c., leads to the general theory of paradromic winding. From this follow the properties of a large class of knots which form "clear coils." A special example of these, given by Listing for threads, is the well-known juggler's trick of slitting a ring-formed band up the middle, through its whole length, so that instead of separating into two parts, it remains in a continuous ring. For this purpose it is only necessary to give a strip of paper one *half*-twist before pasting the ends together. If three half-twists be given, the paper still remains a continuous band after slitting, but it cannot be opened into a ring, it is in fact a trefoil knot. This remark of Listing's forms the sole basis of a work which recently had a large sale in Vienna:—showing how, in emulation of the celebrated Slade, to tie an irreducible knot on an endless string!

Listing next gives a few examples of the application of his method to knots. It is greatly to be regretted that this part of his paper is so very brief; and that the opportunity to which he deferred farther development seems never to have arrived. The methods he has given are, as is expressly stated by himself, only of limited application. There seems to be little doubt, however, that he was the first to make any really successful attempt to overcome even the preliminary difficulties of this unique and exceedingly perplexing subject.

The paper next gives examples of the curious problem:—Given a figure consisting of lines, what is the smallest number of *continuous* strokes of the pen by which it can be described, no part of a line being gone over more than once? Thus, for instance, the lines bounding the 64 squares of a chess-board can be drawn at 14 separate pen-strokes. The solution of all such questions depends at once on the enumeration of the points of the complex figure at which an odd number of lines meet.

Then we have the question of the "area" of the projection of a knotted curve on a plane; that of the number of interlinkings of the orbits of the asteroids; and finally some remarks on hemihedry in crystals This paper, which is throughout elementary, deserves careful translation into English very much more than do many German writings on which that distinction has been conferred.

We have left little space to notice Listing's greatest work, *Der Census räumlicher Complexe* (Göttingen *Abhandlungen*, 1861). This is the less to be regretted, because, as a whole, it is far too profound to be made popular; and, besides, a fair idea of the nature of its contents can be obtained from the introductory chapter of Maxwell's great work on Electricity. For there the importance of Listing's Cyclosis, Periphractic Regions, &c., is fully recognised.

One point, however, which Maxwell did not require, we may briefly mention.

In most works on Trigonometry there is given what is called *Euler's Theorem about polyhedra*:—viz. that if S be the number of solid angles of a polyhedron (not self-cutting), F the number of its faces, and E the number of its edges, then

$$S+F=E+2.$$

The puzzle with us, when we were beginning mathematics, used to be "What is this mysterious 2, and how came it into the formula?" Listing shows that this is a mere case of a much more general theorem in which corners, edges, faces, *and regions of space*, have a homogeneous numerical relation. Thus the mysterious 2, in Euler's formula, belongs to the two regions of space:—the one inclosed by the polyhedron, the other (the *Amplexum*, as Listing calls it) being the rest of infinite space. The reader, who wishes to have an elementary notion of the higher forms of problems treated by Listing, is advised to investigate the modification which Euler's formula would undergo if the polyhedron were (on the whole) ring-shaped:—as, for instance, an anchor-ring, or a plane slice of a thick cylindrical tube. P. G. T.

CLAUDE BERNARD

UNDER the title of "Notes et Souvenirs sur Claude Bernard," Prof. Jousset de Bellesme, of the School of Medicine of Nantes, has published an interesting sketch of the life and labours of the great French physiologist, his master, which those who are admirers of Claude Bernard will be glad to have their attention called to. The essay was meant for the opening address to be delivered at the commencement of the present session of the Nantes School. It seems to have been a little too outspoken to meet with the approbation of the director of the school. On the representation of a majority of the professors of the school, it was forbidden to be delivered *ex cathedrâ* by the Minister of Public Instruction, in an Order dated October 28, 1882. In the pages of the November number of the *Revue Internationale des Sciences biologiques*, the address appeals in type to a wider audience than the assembled professors and pupils of the School of Nantes. Commencing with an extremely graphic account of the author's first introduction to Claude Bernard, which concludes as follows:—"With a kind gesture of his head he bid me attend his laboratory; I thanked him, and was retiring. Just as I was about to close the door, he, taking his attention off his experiment, turned his eyes upon me and said, 'Have you read Descartes' "Discours de la Méthode?" Read it, and read it again." At the time of this interview Claude Bernard was in his forty-fifth year, and a great number of his striking works had been achieved. Having assisted for many years with astonishment at the apparently inexhaustible series of discoveries, Bellesme ventured to ask him one day, what was the secret which enabled him to penetrate so easily into things hidden from others. "Do not seek for a mystery," said Bernard, "nothing can be simpler, or less mysterious. My secret is open to all. When I was a young man, I lived greedily on the writings of Descartes. His 'Discourse' always completely satisfied my soul, and I was passionately fond of it. His rules appeared to me so just, that I came to the conclusion that by a strict observance of them all questions might be solved. That is all." The most important of these rules, Bellesme reminds his readers, is as follows:—"Ne recevoir jamais aucune chose pour vraie qu'on ne la connaisse évidemment être telle, éviter soigneusement la Précipitation et la Prévention dans ses jugements." The author, then, in a very striking manner, draws a series of comparisons between Descartes and Cl. Bernard. Passing from this, he criticises somewhat severely the tendency of a modern school, which without taking notice of the complexitiness of biological phenomena, seem to have culminated in the idea that no contagious disease can be conceived of which has not some special microbe as its cause; but the disciples of this school, he urges, have not meditated on the third rule of Descartes: "Conduire par ordre ses pensées, en commençant par les objets les plus simples et les plus aisés à connaître, pour monter peu à peu comme par degrés jusqu'à la connaissance des plus composés."

We are afforded a little glimpse of the private life of the great French physiologist, which explains a sadness about his domestic relations—possibly not understood by many of his foreign admirers and friends. Married late in life—and even in his very youth never having had much place in his mind for love—still his agreeable and quiet character, his inexhaustible kindness, his open frank cordiality, which so often secured the sympathy of others, seemed to promise an abiding union between him and his wife, but the liberal ideas of the husband, and his devotion to his very peculiar studies, did not please Madame Bernard. The state of things became irritable—intolerable; even the birth of two children did not improve the condition of affairs. In 1869 the separation came. The husband and the father was left alone; and from then to the end of his days he lived his solitary life in an apartment in the rue des Écoles, *vis à vis* to the College of France. His life was all too full of work to leave much time for a morbid appreciation of his solitude. Some slight rest was taken each year at the vintage period at Saint-Julien, near Villefranche, and he almost every year took part in the French Association for the Advancement of Science, an Association which he assisted in founding, and of which he was the first president. During these latter ten years Bellesme was his very constant visitor, his trusty friend. They were times not to be recalled, he tells us, without emotion, and he regards them as among the happiest of his life. Often he would spend the evening with him by his fire-side in the small bedroom, where by preference he would pass the afternoon, and which his old servant would keep with a quite canonical neatness. In the background was the bed with its curtains of blue damask, to the left the fireplace; at the side of the bed, a large armchair in which Claude Bernard would sit enveloped in a dressing-gown, which, on his ample shoulders, took the folds and plaits of an ancient toga; his head covered with a cap, which he would often remove while talking, with an action peculiar to him, as if his thoughts made him find it too tight. Close to him, opposite the fire, a small square table, on which the lamp is placed amidst a mountain of reviews, *brochures*, new books sent to him from all parts. At this epoch of his life he read, however, but little, nor did he write much. The volumes, which were published during these last ten years, were composed of extempore lectures of his, very carefully edited. "With our feet on the fender," writes Bellesme, "our conversation would begin with the striking events of the day, but speedily we turned to physiology. This was almost the sole object of the master's thoughts. About this he would wax eloquent, and speedily we would be entering on the higher regions of the science. These were charming excursions on the very mountain-tops, with the clear light of his mind illuminating all the dark valleys." No wonder that time was little thought of, or often altogether forgotten.

Up to 1865 Claude Bernard's health was excellent. About then he was attacked by an ill-defined chronic enteritis, from which, after eighteen months, he had only recovered. After this he had some rheumatic attacks, which did not frighten his friends, as he still preserved an alas deceitful appearance of vigorous health. Still nothing seemed to presage his approaching end. Towards the last days of 1877, after passing a long morning in the damp and unhealthy laboratory of the College of France, he returned home shivering, and with a feeling of intense uneasiness. The next day nephritis set in; he kept his room, and was not disquieted as to his state, but after a few days it was evident to all that his career was run. On February 7, 1878, after a six weeks of suffering, he lost all consciousness, and expired on February 10, at half-past nine o'clock in the evening. In Claude Bernard France lost a noble son, one who cultivated science purely and disinterestedly. His works will not ever perish, and in future years they will serve as a demonstration of the excellence of the "Discours de la Méthode," and as a very sure guide towards arriving at a knowledge of truth.

E. P. W.

THE FINSBURY TECHNICAL COLLEGE

THE Finsbury Technical College and the programme of instruction which we have recently received represent a *fait accompli* of the City and Guilds of London Institute.

Judging of the education to be given in the new College from the Programme forwarded to us, we may congratulate the Council of the Institute on having steered clear of the Scylla and Charybdis which overhang the narrow channel of technical education proper. In all such educational movements, there is the danger that the teaching shall either be too exclusively of the ordinary scientific type, or, by being too distinctly practical, shall attempt to take the place of workshop instruction. Theory and practice promise to be judiciously combined in the new school, and the experiment about to be tried in Tabernacle Row is interesting not only as a new departure in education, but also as showing the effect of beginning science teaching from the practical rather than from the theoretical side, as is still so frequently the case.

During the last three years the conception of the Finsbury College has undergone considerable development, and corresponds now much more nearly to what a technical school should be than appeared probable at its inception. According to the plans published in March, 1880, in the Report to the Governors, the College was to consist in the first place of chemical and physical laboratories only. These laboratories were to be adapted to instruction in various departments of applied chemistry and physics, but no provision was made for the teaching of mechanics, drawing, or of other subjects which find a place in the new programme. Such a school would scarcely have realised the idea of a technical college properly so called, least of all a college for the instruction of artisans. It is doubtful whether many of the pupils who frequent the excellent classes of Prof. Ayrton and Prof. Armstrong are really of the artisan class, for which instruction was originally intended to be given by the City Guilds. The progress that is being made in the completion of the Central Institution at South Kensington, which is expressly intended for the education of a higher class of students, renders it the more important, in order that the two schools may not clash with one another, that the instruction at Finsbury should be not only nominally, but really, of a different grade, and adapted to the improvement of artizans and workpeople.

The programme recently published shows that provision has been made for other branches of industry besides electrical lighting and technical chemistry.

The Technical College, Finsbury, consists really of two distinct schools: a day school and an evening school. It has for its objects the education of—

(1) Persons of either sex who wish to receive a scientific and practical preparatory training for intermediate posts in industrial works.

(2) Apprentices, journeymen, and foremen who are engaged during the day-time, and who desire to receive supplementary instruction in the art practice, and in the theory and principles of science connected with the industry in which they are engaged.

(3) Pupils from middle class and other schools who are preparing for the higher scientific and technical courses of instruction to be pursued at the Central Institution.

The College therefore fulfils the functions of a finishing technical school for those entering industrial life at a comparatively early age; of a supplemental school for those already engaged in the factory or workshop; and of a preparatory school for the Central Institution.

The College embraces the following four chief departments: (1) Mathematical and Mechanical; (2) Physical; (3) Chemical; (4) Applied Art.

It is under the general direction of a principal or superintendent of studies; and the Council of the Institute appear to have acted wisely in asking Mr. Philip Magnus, who has directed the work of the Institute up to the present time with so much ability, and whose exceptional experience of Continental technical schools renders him particularly fitting for such a position—to occupy this post, pending the completion of the Central Institution, and to carry into effect the general scheme of instruction indicated in the programme.

In the day school of the Finsbury College, pupils from middle class and higher elementary schools will have the opportunity of continuing their studies, and of preparing, at the same time, for the particular branch of industry in which they purpose to be engaged.

Such a school is a technical school in the true sense of the word, for it gives the pupil the best training he can receive for his future occupation.

The instruction is not limited to the application of one branch of science only; the future electrician is taught chemistry and mechanics, the chemist is taught mechanics and physics, the mechanician is taught physics and chemistry, and, what is almost equally important, all are taught drawing, French, German, and the manipulation of tools in the workshops.

The evening school is intended for those who are already engaged in practical work, and in this department of the College noteworthy changes have been introduced, with a view of adapting the teaching to the special requirements of artisans. To the courses of Applied Physics and Chemistry originally provided for, courses of Mechanical Engineering have been added; but besides these courses, which are adapted to the higher class of artisans, a complete syllabus of instruction has been added to the programme, suited to the requirements of the special industry of the district of Finsbury, viz. cabinet-making. To provide a systematic course of instruction for cabinet-makers it was necessary to add to the other departments of the College, a Department of Applied Art; and in order to secure a good number of students to start with, the Council affiliated to the College the City School of Art, one of the oldest art schools of the country, and appointed Mr. Brophy as head master.

Moreover, to satisfy the demand of workmen engaged in numerous small industries, the Council have arranged courses of instruction, on a more systematic basis than has been previously attempted in this country, for carpenters, joiners, metal-plate workers, bricklayers, &c., thereby supplying that popular element in the instruction provided by the City Guilds, which at first seemed likely to be wanting in their scheme of technical education.

By undertaking to admit apprentices to the evening classes at half the fees, which are small enough, charged to ordinary workmen, those who have had the direction of the work of the Institute have shown a just appreciation of the importance of encouraging apprentices of fifteen to twenty to follow the evening courses of instruction; for there will be far less difficulty in inducing youths, during their apprenticeship, to attend regular systematically-arranged lessons, covering a period of two or three years, than is generally found in the case of adult workmen.

Indeed, it is in the arrangement of systematised and progressive courses of instruction adapted to various industries and involving the application both of science and of art to the student's occupation, as well as in the practical methods of instruction adopted, that the Technical College, Finsbury, is differentiated from other science schools.

The programme of studies now before us is a publication that can hardly fail to prove useful to all persons who are interested in the establishment of technical schools, and shows unmistakably that the Council of the Institute and their advisers are fully conscious of the difficulties that beset the problem of technical education, and may be trusted to deal judiciously with them in the schools established under their direction.

The fittings of the new College, which are most complete and admirably adapted for practical teaching, have been designed and executed under the direction of Professors Armstrong, Ayrton, and Perry.

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE[1]

III.

THE determination of H and the measurement of a current in absolute units, can be effected simultaneously by the method devised by Kohlrausch, and described in the *Philosophical Magazine*, vol. xxxix. 1870. This method consists essentially in sending the current to be measured through two coils, of which all the constants are accurately known. One of these is the coil of a standard galvanometer, the other is a coil hung by a bifilar suspension, the wires of which convey the current into the coil. The latter coil rests in equilibrium when no current is passing through it, with its plane in the magnetic meridian. When a current is sent through it, it is acted on by a couple due to electro-magnetic action between the current and the horizontal component of the earth's force, which tends to set it with its plane at right angles to the magnetic meridian; and this couple is resisted by the action of the bifilar. The coil comes to rest, making a certain angle with the magnetic meridian, and as the couple exerted by the bifilar suspension for any angle is supposed to have been determined by experiment, a relation between the value of H and the value of the current is obtained. But, as the same current is sent through the coil of the standard galvanometer, the observed deflection of the needle of that instrument gives another relation between H and C. From the two equations expressing these relations the values of H and C can be found. Full details of the construction of Kohlrausch's apparatus and of the calculation of its constants will be found in the paper above referred to

In this method it is assumed that the value of H is the same at both instruments, an assumption which for rooms not specially constructed for magnetic experiments cannot safely be made. An instrument which is not liable to this objection has been suggested by Sir William Thomson. A short account of this instrument and its theory will be found in Maxwell's "Electricity and Magnetism," vol. ii. p. 328.

In the application of what has gone before to the graduation of galvanometers, we shall have to deal with the quantities resistance and potential, and in our calculations to measure potentials in volts, resistances in ohms, and currents in amperes. A full explanation of the terms resistance and potential would require a treatise on electricity, but perhaps a very short explanation of what is meant by a volt, by an ohm, and by an ampere may not be here out of place.

Two conductors are at different potentials when, on their being put in contact, electricity passes from one to the other. The difference of potential between them will be made manifest if one of them be connected with an electrically insulated plate which forms one of the scales of a delicate balance, and the other with a second insulated plate parallel to, and at a very small distance from the first plate. If the conductors be at different potentials the plates will attract one another, and the force of attraction may be weighed by means of the balance. With certain arrangements to ensure accuracy, a balance may be constructed by means of which the difference of potentials between two conductors can be measured. Such an instrument has been made by Sir William Thomson, and called by him an Absolute Electrometer.

[1] Continued from p. 108

It is found experimentally by measuring with a delicate electrometer that, between any two cross-sections A and B of a homogeneous wire, in which a uniform current of electricity is kept flowing by any means, there exists a difference of potentials, and that if the wire be of uniform section throughout, the difference of potentials is in direct proportion to the length of wire between the cross-sections. It is found further that if the difference of potentials between A and B is kept constant, and the length of wire between them is altered, the strength of the current varies inversely as the length of the wire. The strength of the current is thus diminished when the length of the wire is increased, and hence the wire is said to oppose *resistance* to the current; and the resistance between any two cross-sections is proportional to the length of wire connecting them. If the length of wire and the difference of potentials between A and B be kept the same, while the cross-sectional area of the wire is increased or diminished, the current is increased or diminished in the same ratio; and therefore the resistance of a wire is said to be inversely as its cross-sectional area. Again, if for any particular wire, measurements of the current strength in it be made for various measured differences of potentials between its two ends, the current strengths are found to be in simple proportion to the differences of potential so long as there is no sensible heating of the wire. Hence we have the law, due to Ohm, which connects the current C flowing in a wire of resistance R, between the two ends of which a difference of potential V is maintained,

$$C = \frac{V}{R} \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

In this equation the units in which any one of the three quantities is expressed depend on those chosen for the other two. We have defined unit current, and have seen how to measure currents in absolute units; and we have now to show how the absolute units of V and R are to be defined, and from them and the absolute unit of current to derive the practical units—volt, ampere, coulomb, and ohm.

We shall define the absolute units of potential and resistance by a reference to the action of a very simple but ideal magneto-electric machine, of which, however, the modern dynamo is merely a practical realisation. First of all let us imagine a uniform magnetic field of unit intensity. The lines of force in that field are everywhere parallel to one another: to fix the ideas let them be vertical. Now imagine two straight horizontal metallic rails running parallel to one another, and connected together by a sliding bar, which can be carried along with its two ends in contact with them. Also let the rails be connected by means of a wire so that a complete conducting circuit is formed. Suppose the rails, slider, and wire to be all made of the same material, and the length and cross-sectional area of the wire to be such that its resistance is very great in comparison with that of the rest of the circuit, so that, when the slider is moved with any given velocity, the resistance in the circuit remains practically constant. When the slider is moved along the rails it cuts across the lines of force, and so long as it moves with uniform velocity a constant difference of potentials is maintained between its two ends, and a uniform current flows in the wire from the rail which is at the higher potential to that which is at the lower. If the direction of the lines of force be the same as the direction of the vertical component of the earth's magnetic force in the northern hemisphere, so that a blue pole placed in the field would be moved upwards, and if the rails run south and north, the current when the slider is moved northwards will flow from the east rail to the west through the slider, and from the west rail to the east through the wire. If the velocity of the slider be increased the difference of potentials between the rails, or, as it is otherwise called, the electromotive force producing the current, is increased in the same ratio; and therefore by Ohm's law so also is the current. Generally for a slider arranged as we have imagined, and made to move across the lines of force of a magnetic field, the difference of potentials produced would be directly as the field intensity, as the length of the slider, and as the velocity with which the slider cuts across the lines of force. The difference of potentials produced therefore varies as the product of these three quantities; and when each of these is unity, the difference of potentials is taken as unity also. We may write therefore $V = ILv$, where I is the field intensity, L the length of the slider, and v its velocity. Hence if the intensity of the field we have imagined be 1 c.g.s. unit, the distance between the rails 1 cm., and the velocity of the slider 1 cm. per second, the difference of potentials produced will be 1 c.g.s. unit.

This difference of potentials is so small as to be inconvenient for use as a practical unit, and instead of it the difference of potentials which would be produced if, everything else remaining the same, the slider had a velocity of 100,000,000 cms. per second, is taken as the practical unit of electromotive force, and is called one *volt*. It is a little less than the difference of potentials which exists between the two insulated poles of a Daniell's cell.

We have imagined the rails to be connected by a wire of very great resistance in comparison with that of the rest of the circuit, and have supposed the length of this wire to have remained constant. But from what we have seen above, the effect of increasing the length of the wire, the speed of the slider remaining the same, would be to diminish the current in the ratio in which the resistance is increased, and a correspondingly greater speed of the slider would be necessary to maintain the current at the same strength. We may therefore take the speed of the slider as measuring the resistance of the wire. Now suppose that when the slider 1 cm. long was moving at the rate of 1 cm. per second, the current in the wire was 1 c.g.s. unit; the resistance of the wire was then 1 c.g.s. unit of resistance. Unit resistance therefore corresponds to a velocity of 1 cm. per second. This resistance, however, is too small to be practically useful, and a resistance 1,000,000,000 times as great, that is, the resistance of a wire, to maintain 1 c.g.s. unit of current in which it would be necessary that the slider should move with a velocity of 1,000,000,000 cms. (approximately the length of a quadrant of the earth from the equator to either pole) per second, is taken as the practical unit of resistance, and called one *ohm*.

In reducing the numerical expressions of physical quantities from a system involving one set of fundamental units to a system involving another set, as for instance from the British foot-grain-second system, formerly in use for the expression of magnetic quantities, to the c.g.s. system, it is necessary to determine, according to the theory first given by Fourier, and extended to electrical and magnetic quantities by Maxwell, for each a certain reducing factor, by substituting in the formula, which states the relation of the fundamental units to one another in the expression of the quantity, the value of the units we are reducing from in terms of those we are reducing to. For example, in reducing a velocity say from miles per hour, to centimetres per second, we have to multiply the number expressing the velocity in the former units by the number of centimetres in a mile, and divide the result by the number of seconds in an hour; that is, we have to multiply by the ratio of the number of centimetres in a mile to the number of seconds in an hour. The multiplier therefore, or *change-ratio* as it has been called by Professor James Thomson, is for velocity simply the number of the new units of velocity equivalent to one of the old units, and may be expressed by the formula $\frac{L}{T}$, where L is the number of new units of length contained in one of the old, and T is the corresponding number for the unit of time. In the same way the change-ratio for rate of change of velocity or acceleration is $\frac{L}{T}$; and the change-ratio of any other physical quantity may be found by determining from its definition the manner in which its unit involves the fundamental units of mass, length and time. Now the theory of the change-ratios of electrical and magnetic quantities, in the electro-magnetic system of units, shows that the change-ratio for resistance is the same as that for velocity; that in fact a resistance in electro-magnetic measure is expressible as a velocity; and hence we may with propriety speak of a resistance of one ohm as a velocity of 10^9 centimetres per second.

It is obvious from equation (14) that if V and R, each initially one unit, be increased in the same ratio, C will remain one unit of current; but that of V be, for example, 10^8 c.g.s. units of potential, or one volt, and R be a resistance of 10^9 cms. per second, or one ohm, C will be one-tenth of one c.g.s. unit of current. A current of this strength—that is, the current flowing in a wire of resistance one ohm, between the two ends of which a difference of potentials of one volt is maintained,—has been adopted as the practical unit of current and called one *ampere*. Hence it is to be remembered one ampere is one-tenth of one c.g.s. unit of current.

The amount of electricity conveyed in one second by a current of one ampere is called one *coulomb*. This unit although not quite so frequently required as the others, is very useful, as, for instance, for expressing the quantities of electricity which a secondary cell is capable of yielding in various circumstances. For example, in comparing different cells with one another their capacities, or the total quantities of electricity they are capable of yielding when fully charged, are very conveniently reckoned in coulombs per square centimetre of the area across which the electolytic action in each takes place.

The magneto-electric machine we have imagined gives us a very simple proof of the relation between the work done in maintaining a current, the strength of the current, and the electromotive force producing it. By the definitions given above of a magnetic pole and a magnetic field, a unit pole must produce at unit distance from itself a magnetic field of unit intensity. Again, unit current is defined as that current which flowing in a wire of unit length, bent into an arc of a circle of unit radius, acts on a unit magnetic pole at the centre of the circle with unit force. Hence, as the reaction of the pole on the current must be equal to the action of the current on the pole, this wire carrying the current is acted on by unit force tending to move it in the opposite direction to that in which the pole is moved, and it plainly does not matter which we suppose held fixed and which moved. Therefore a conductor in a magnetic field, and carrying a unit current which flows at right angles to the lines of force, is acted on by a force tending to move it in a direction at right angles to its length, and the magnitude of this force for unit length of conductor, and unit field, is by the definition of unit current equal to unity.

Applying this to our slider in which we may suppose a current of strength C to be kept flowing, say, from a battery in the circuit, let L be the length of the slider, v its velocity, and I the intensity of the field; we have for the force on the moving conductor the value ILC. Hence the rate at which work is done by the electro-magnetic action between the current and the field is $ILC\frac{dx}{dt}$ or $ILCv$, and this must be equal to the rate at which work is done in generating by motion of the slider a current of strength C. But as we have seen above ILv is the electromotive force produced by the motion of the slider. Calling this now E, the symbol usually employed to denote electromotive force, we have EC as the rate of working, that is, the rate at which electrical energy is given out in the circuit.

By Ohm's law this value for the rate of working may be put into either of the two other forms, namely: $\frac{E^2}{R}$, or C^2R. In the latter of these forms the law was discovered by Joule, who measured the amount of heat generated in wires of different resistances by currents flowing through them. This law holds for every electric circuit whether of dynamo, battery, or thermoelectric arrangement.

We have, in what has gone before, supposed the slider to have no resistance comparable with the whole resistance in the circuit. If it has a resistance r, and R be the remainder of the resistance in circuit, the actual difference of potentials between its two ends will not be ILv or E, but $E\frac{R}{R+r}$. The rate per unit of time at which work is given out in the circuit is however still EC, of which the part $EC\frac{r}{R+r}$ is given out in the slider, and the remainder, $EC\frac{R}{R+r}$, in the remainder of the circuit. In short, if V be the actual difference of potentials, as measured by an electrometer, between two points in a metallic wire connecting the terminals of a battery or dynamo, and C be the current flowing in the wire, the rate at which energy is given out is VC, or if R be the resistance of the wire between the two points, C^2R.

One of the great advantages of the system of units of which I have given this brief sketch, is that it gives the value of the rate at which work is given out in the circuit, without its being necessary to introduce any coefficient such as would have been necessary if the units had been arbitrarily chosen. When the quantities are measured in c.g.s. units, the value of EC is given in terms of the centimeter-dyne or *erg*, the recognized dynamical unit of work. Results thus expressed may be reduced to *horse-power* by dividing by the number $7{\cdot}46 \times 10^9$; or if E is measured in volts, and C in amperes, EC may be reduced to horse-power by dividing by 746. Thus, if 90 volts be maintained between the terminals of a pair of incandescent lamps joined in series, and a current of 1·3 ampere flows through these lamps, the rate at which energy is given out in the lamps is approximately ·157 horse-power.

ANDREW GRAY

(*To be continued.*)

NATURAL SCIENCE IN THE OPEN COMPETITIVE EXAMINATIONS FOR CLERKSHIPS (CLASS I.) IN THE CIVIL SERVICE

THE Civil Service Commissioners have done much to encourage the thorough study of natural science in our Universities by the weight which they have assigned to it in the competitive examinations for first-class clerkships in the Government service. These posts are of sufficient value to attract young men of one or two-and-twenty, fresh from the University. It will be seen from the list of marks assigned to subjects, which we print below, that 1000 marks may be made in two branches of natural science, for instance, Zoology and Geology; whilst Greek and Roman language, literature, and history only stand for 1500. Hence a candidate who makes science his strong side and can do something in either English, classical, or foreign literary subjects, is by no means at a disadvantage.

We take this opportunity of prominently drawing attention to the encouragement thus given to the pursuit of natural science as a branch of culture.

The schoolboy who is excused from verse-composition and sent into the chemical laboratory, is distinctly recognised, and has a fair chance given to him by the Commissioners. So too the Oxford undergraduate who breaks with the wearisome iteration of Greek play and Latin odes in the College lecture-room and escapes to the fascinating microscopes and dissecting troughs of Prof. Moseley, or the verniers and milligram-weighing pans of Prof. Clifton, is marked out for patronage. And not only indeed are Oxford and Cambridge students of science thus benefited.

The courses of instruction in scientific subjects given at the London Colleges, University and King's, are pre-eminently such as will enable a candidate to do justice to his abilities in this examination. The examination is practical, and no mere smattering of a subject will obtain any marks for a candidate. Hence the "crammers" are at a disadvantage, and the teachers in duly-organised and properly-furnished laboratories, are rightly encouraged in their efforts to carry on thorough courses of instruction. It is indeed, a matter for satisfaction that hitherto the various cramming establishments where young men are "prepared" for public examinations have failed to enable any candidate to gain a success in any branch of natural science in these higher competitive examinations, those candidates who have scored marks in natural science having been University students. We subjoin an extract from the Regulations issued by the Civil Service Commission, to the secretary of which body application for further information should be made.

1. The limits of age for these situations are 18 and 24, and candidates must be of the prescribed age on the first day of the competitive examination.

2. At the competitive examinations exercises will be set in the following subjects only; the maximum of marks for each subject being fixed as follows, viz. :—

	Marks.
English Composition (including Précis-writing)... ...	500
History of England—including that of the Laws and Constitution	500
English Language and Literature	500
Language, Literature, and History of Greece	750
,, ,, ,, Rome	750
,, ,, ,, France	375
,, ,, ,, Germany ...	375
,, ,, ,, Italy	375
Mathematics (pure and mixed)	1250
Natural Science: that is, (1) Chemistry, including Heat; (2) Electricity and Magnetism; (3) Geology and Mineralogy; (4) Zoology; (5) Botany	1000
*** The total (1000) marks may be obtained by adequate proficiency in any two or more of the five branches of science included under this head.	
Moral Sciences: that is, Logic, Mental and Moral Philosophy	500
Jurisprudence	375
Political Economy	375

Candidates will be at liberty to offer themselves for examination in any or all of these subjects. No subjects are obligatory.

No candidate will be allowed any marks in respect of any subject of examination unless he shall be considered to possess a *competent knowledge* of that subject.

NOTES

A TELEGRAM, dated December 21, has been received by the Finnish Academy of Sciences from Prof. S. Lemstrom, chief of the Finnish Meteorological Observatory at Sodankyla. He states that, having placed a galvanic battery with conductors covering an area of 900 square metres on the hill of Oratunturi, he found the cone to be generally surrounded by a halo, yellow-white in colour, which faintly but perfectly yields the spectrum of the aurora borealis. This, he states, furnishes a direct proof of the electrical nature of the aurora, and opens a new field in the study of the physical condition of the earth. A further telegram, dated Sodankyla, January 5, has been received, in which Prof. Lemstrom states that experiments with the aurora borealis made December 29, in Enare, near Kultala, on the hill of Pietarintunturi, confirm the results of those at Oratunturi. On that date a straight beam of aurora was seen over the galvanic apparatus. It also appears from the magnetic observations that the terrestrial current ceases below the aurora arc, while the atmospheric current rapidly increases, but depends on the area of the galvanic apparatus to which it seems to be proportional. The Professor regrets that with the means at his disposal further experiments cannot be made, and that he intended, on the 13th inst., to withdraw the apparatus.

THE Report of the Royal Gardens, Kew, for 1881, shows what a large amount of varied and highly useful work is got through in the space of a year at that great national establishment; perhaps *imperial* would be more accurate than national, for it is really the botanical and horticultural centre of the whole empire. One important feature is the lessons given during the year to the young gardeners in the science of these subjects; this will certainly tend to secure that the work of the gardens throughout are conducted with intelligence and on a sound scientific basis. The Report contains extracts from the reports of various Colonial curators, on the progress of experiment in the culture of certain important plants, such as Cinchona and india-rubber. Mr. Jamieson reports from the Nilgiris that he has found the Cape Coast and Liberian coffee-plants to be really two varieties. Queensland may yet add coffee to its other industries, a vastly important addition. The Report contains an illustration of *Cinchona Ledgeriana*, Moens.

IN preparation for the International Fisheries Exhibition there is a large number of artificers now employed in erecting and completing enormous buildings for the reception of the exhibits on the ground known as the Royal Horticultural Gardens, South Kensington. Some four or five immense structures have been already erected, two standing side by side on the western side of the gardens—one being about 180 yards, the other some 140 yards in length, with a width of about 20 yards, and of great height and capacity. Arched roofs contain in the centre, running the whole length of the building, a wide breadth of glass, which throws below as ample an amount of light as can be desired. Other similar buildings are in the course of completion at the north-eastern corner of the gardens, close to the Albert Hall; and when the capacity of all these structures is considered, some estimate can be formed of the enormous proportions the International Exhibition will assume. The arcade at the south-western side of the gardens, well known for the horticultural and other expositions which the Royal Horticultural Society has held in it, is being devoted to the purposes of an aquarium, which will soon be completed, and in which both fresh-water and sea fish will be exhibited. The spacious long arcade affords ample room for all the tanks that may be required, and it is expected that the aquarium will form one of the most attractive features of the exhibition. Arrangements will be made to provide easy access from one building to another, and such portions of the gardens as remain uncovered by the necessary structures will serve as an agreeable promenade. All the works are so forward that everything will be ready in good time for the reception of the exhibits of our own and of foreign countries.

CONSIDERABLE success has attended the Sunday Evening Association, its object being to bring together all persons who, estimating highly the elevating influence of music, the sister arts, literature and science, desire, by means of meetings on Sunday evenings, to see them more fully identified with the religious life of the people. The president is Dr. Geo. J. Romanes, F.R.S. The fifth series of meetings will be concluded next Sunday with a lecture by Dr. W. B. Carpenter, F.R.S., C.B., on "Niagara." A sixth series will be commenced on Sunday, February 11, and will include lectures by Dr. G. J. Romanes, F.R.S., on "Star

Fish;" J. Cotter Morison, M.A., on "A Glimpse of England in the Fifteenth Century;" Dr. P. Martin Duncan, F.R.S., on "Metamorphosis of Insects," and J. Norman Lockyer, F.R.S., on "The Recent Eclipse of the Sun." The meetings are held in the Working Men's College, Great Ormond Street.

THE Report of the Commissioner of the Imperial Japanese Mint, Ósaka, for the year ending June, 1882, being the twelfth report of the Japanese Mint, shows that the high standard of excellence of the work done at this establishment is still kept up. Rather more gold was coined than during the previous year, viz. 803,645 yen, all in 5 yen pieces; the silver coined during this year was all 1 yen pieces, and amounted to 3,294,988 yen; whilst the nominal value of the copper coins, in 2 sen, 1 sen, and half sen pieces was 1,130,548 yen. The total nominal value of the coins of all denominations struck since the commencement of the Mint to the end of the last financial year is 102,888,478 yen, of which more than one-half is gold and two-fifths silver. Besides this a large number of medals have been struck and refined ingots produced. This year a large number of old bronze guns and field pieces have been melted down, refined, and converted into copper coins, and also additional improvements and economies have been made in the treatment of old Japanese silver coins prior to their re-coinage. The sulphuric acid works in connection with the Mint have been more busy than last year, and nearly a million pounds of acid have been exported to China in addition to that produced for home consumption. The soda works are now in working order, and a considerable outturn of sulphate, black ash, white ash, and crystallised soda has been made; caustic and bicarbonate of soda will shortly be produced, and it is proposed to add works for the production of bleaching powder so as to utilise the whole of the hydrochloric acid formed. There was a considerable increase in the amount of Corean gold dust received during the year, but it was not generally of a high standard. The curve showing the variation in weight of the silver yen issued, as also the report of the trial of the pyx and the reports of the assays on the pyx pieces made by Prof. Chandler Roberts of the Mint in this country, and by Mr. Lawner, of the American Mint, show that the greatest care and attention is given to every department, both by the foreign *employés*, Mr. Wm. Gowland, chemist, assayer, and technical adviser, and Mr. R. MacLagan, engineer, and also by the native officials. The report affords abundant evidence that excellent work is being done by the above-named European technical advisers of the Japanese Government.

WE have received an excellent little pamphlet on "The Rudiments of Cookery, with some Account of Food and its Uses." It is called a manual for the use of schools and homes, is written by "A. C. M.," examiner to the Northern Union of Training Schools for Cookery, dedicated to the Countess of Derby, and published by Simpkin and Marshall. Besides conveying practical information on plain cookery, the writer is careful throughout to explain the why and the wherefore of every point by briefly stating the principles of elementary science which bear upon the subject. We can recommend the pamphlet to the "schools and homes" for whose use it is designed.

AT the meeting of the Royal Geographical Society on Monday evening, Sir Henry Rawlinson, who presided, stated that Mr. Leigh Smith, in acknowledgment of the assistance which the Royal Geographical Society had afforded him in fitting out his expedition, and also to mark the extent of the interest he takes in Arctic discovery, had presented 1000*l.* for the purpose of extended Arctic exploration. Sir Henry referred briefly, also, to the recent services of one of the native explorers which the Indian Government are in the habit of sending beyond the Himalayas, which are closed to Europeans by the jealousy of the natives. The paper from which he quoted said: "One of General Walker's native explorers has returned to India after an absence of four years through Thibet, in which he has obtained a large amount of new geographical information, and has finally disposed of the question of the Sanpo River, which does not, according to him, fall into the Irawaddy, as was generally supposed. The traveller got as far north as Santu, lat. 40° N., 92° E., which is supposed to be the Sorchia of Marco Polo. Returning, he proceeded to Batang, and tried to reach Assam by the direct route, but was stopped at the frontier of the Mishmi country by the assurance that the natives were savages, who would murder him. He, therefore, took a circuitous route to Lhassa, *via* Alanto and Gjamda. But from the latter place he turned and made for Chetang, on the Sanpo, thence by Giangze, Leng, and Phari, to Darjeeling. He reports that Sama is the place where two Europeans coming from Assam were murdered some thirty years ago. If so, it must be Wilcox's Simé, where the priests Kirch and Bsury were murdered in 1854. He is positive he only crossed the Sanpo once at Chetang. He says that on the road from Sama to Gjamda there is a great range of hills to the west, separating the basin of the affluents of the Sanpo, from that of the affluents of the Irawaddy to the east."

LIEUT.-COL. BERESFORD LOVETT, her Majesty's Consul at Astrabad, read at the same meeting a paper, which was illustrated by an itinerary map from his plane table survey of four inches to the mile. The route from Teheran northwards to Asolat is well known, but new ground was traversed between Asolat and the Lur Valley, on the south of Mount Damavand, and again between the Horas River and Finshuh, and onward to Kurrand, and also between Fulhad Mahala and Shu Kuh. The survey throws considerable light on the untrodden parts of the Elburz Mountains, and on the entire route no part of which had been previously delineated with any approach to accuracy. The author's route was from Teheran to Astrabad, *viâ* Ahar to Sarak, thence to Husan Ikdir, Gutchisir, Wohbad, Towar, and Arsmkern. The route was along the ridge of the Shamran mountain country, which runs south of the Caspian, the author desiring, as the journey was made in the middle of the summer heats, not to descend below 5000 feet, while on the journey an altitude of over 9000 feet was attained, and one mountain 12,500 feet was measured and ascended. The author found in one position a plateau of considerable height full of oyster shells, while in his paper and in the discussion which followed, it was shown that at one geological period the Caspian must have been a sea of very large extent to the north and east.

UNDER the presidency of the Marquis of Exeter, a National Fish Culture Association has been established, its object being to increase the supply of food by increasing the supply of fish of all kinds.

FROM the preliminary report of the Princeton Scientific Expedition (the third of its kind), whose ground was Wyoming, Colorado, and the west, it would seem that the students who formed the party covered a very considerable field, did some good work in geology and natural history, and endured just enough of hardship to give them the feeling of real explorers.

THE trial of the electro-magnetic engine, aërial screw, and bichromate elements constructed by MM. Tissandier for their directing balloon took place in their aeronautical work-shop at Point du Tour, on January 26, before a large number of electricians and aeronauts. It was shown that the twenty-four elements, each of which weighs about six kilogrammes, give during almost three hours a current which rotates a screw of 2·85m. diameter, and about 5 metres of path, with a velocity of 150 turns in a minute. The motive power really developed may be estimated at that of four horses per hour. The weight of all the machinery and elements is a little less than 250 kilogrammes. The real effect on the air can only be found by experiments in the air, but according to measurements taken with a dynamometer of the horizontal tendency to motion, it is about the same as in the experiment tried by Dupuy de Lome. The motive power of Dupuy de Lome having been obtained with eight men working his large screw, whose diameter was 9 metres, it may be inferred that the results in the present case will be more advantageous in the ratio of *two and a half* to *one*. These results are not very powerful when compared with the immense power of aerial currents. But MM. Tissandier have no intention of directing their balloon against strong winds. Their object is to organise an apparatus with which rational experiments may be made in the air, and they have taken advantage of the most recent improvements of science. If their elongated balloon answer their wishes, a real advance will be registered in the history of aeronautics.

EXCAVATIONS are being carried out on Blackheath for the purpose of exposing the "deneholes" which have puzzled geologists and archæologists, and of which we gave some account in vol. xxiii. p. 365.

IN 1884 a general Italian exhibition will be opened at Turin. Among the exhibits will be works in mathematics, physics, and general chemistry.

THE "Treatise on Marine Surveying," reviewed in last week's NATURE, is published by Messrs. Macmillan and Co., and not by Mr. Murray.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona* ♀) from West Africa, presented by Mr. J. N. Flatau; a Crested Porcupine (*Hystrix cristatus*) from West Africa, presented by Mr. Joseph J. Doke; two Pileated Jays (*Cyanocorax pileatus*) from La Plata, presented by Capt. Gamble; two Grey-breasted Parrakeets (*Bolborhynchus monachus*) from the Argentine Republic, presented by Mr. Tomas Peacock; an European Tree Frog (*Hyla arborea*), European, presented by Mrs. M. B. Manuel; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from East Africa, a Macaque Monkey (*Macacus cynomolgus* ♀) from India, deposited; a Water Chevrotain (*Hyomoschus aquaticus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—The following are Greenwich times of heliocentric minima of Algol:—

		h.	m			h	m
February	3,	8	47	Feb.	26,	7	18
	17,	16	52	March	12,	15	23
	20,	13	41		15,	12	12
	23,	10	29		18,	9	1

The light equation (geocentric—heliocentric) in seconds, may be found from the expression—

$$460{\cdot}2\text{s. R. sin } (S+35^\circ\ 28'\ 7),$$

where R is the earth's radius-vector, and S the longitude of the sun. S Cancri will be at a minimum about the following times:—February 2, at 9h. 40m.; February 21, at 8h. 55m.; and March 12, at 8h. 2m. A minimum of U Cephei occurs on February 5, about 13h. 26m. χ Cygni is at minimum on March 17. This year's maximum of Mira Ceti is not observable. According to the observations of Mr. Knott in 1881 and 1882, a maximum of T Cephei, when the star is about 6·5m., may be expected towards February 17; the position of this variable for 1880 is in R.A. 21h. 7m. 57s, Decl. +68° 0′·1; it is No. 3731 in Fedorenko's catalogue from Lalande.

REPORTED DISCOVERY OF A COMET.—A Reuter's telegram from Puebla, Mexico, January 23, states that a comet had been discovered there near the planet Jupiter, of which no further account has been received at the time we write, nor has a somewhat hurried examination of the vicinity between clouds revealed anything brighter or more cometary in aspect than our very old friend, the first nebula of Messier's catalogue near ζ Tauri, which has proved "a mare's nest" for more than one incipient comet-hunter. Jupiter was close at hand on January 22, but there was a full moon on that date, which hardly favours the suggested explanation. Messier 1, it may be remembered, led to more than a single false alarm when observers were on the look out for Halley's comet in 1835.

THE NEXT RETURN OF D'ARREST'S COMET.—At the sitting of the Paris Academy of Sciences on January 22, M. Leveau communicated elements of the orbit of D'Arrest's comet of short period, for the approaching return to perihelion. He states that on account of the great perturbations suffered by the comet from its passage near Jupiter during the period 1859-1863 (in April, 1861, it passed within 0·36 of the earth's mean distance from the planet), and the want of observations at its third appearance in 1864, it has not been possible to combine in the same system of elements the observations made in 1851 and 1857 with those of 1870 and 1877. He has consequently been obliged to determine the osculating orbit in 1883, from the elements which best represent the observations of 1870 and 1877 alone. The following are the elements of the comet's orbit for 1883, June 12·0, M.T. at Paris:—

	°	′	″	
Mean anomaly	328	13	20·3	
Longitude of perihelion	319	11	10·8	Mean equinox 1880·0
,, ascending node ...	146	7	21·0	
Inclination	15	41	47·1	
Angle of eccentricity	38	46	33·4	
Mean daily sidereal motion ..	530″·65245			

It is M. Leveau's intention to prepare and circulate among astronomers an ephemeris for what appears to be the most likely period during which to obtain observations, or from April 23 to November 25 in the present year, but from the comet's great distance or unfavourable position it is probable that only the largest telescopes will command it. By the above elements the comet will not arrive at perihelion until 1884, January 13·5765 Greenwich M.T.

MERIDIAN OBSERVATIONS OF NEBULÆ.—Dr. Engelmann publishes the positions of about 120 nebulæ, determined with the 6-inch meridian circle of the Leipsic Observatory, and reduced to the beginning of the year 1870, with the mean epoch of observation and the annual precessions, thus aiding by meridian observations the extension of our knowledge of accurate places of these bodies, which has engaged the attention of d'Arrest, Vogel, Schonfeld, Schultz, and others, with equatorial instruments. Valuable material is thus being collected for the investigation of proper motion amongst the nebulæ, which for want of reliable positions in past times, is not practicable at present, except perhaps in a few isolated cases.

ERRATUM.—In last week's "Astronomical Column," p. 300, lines seven and six from bottom, for *Washington* read *Washburn*.

PHYSICAL NOTES

A DOUBLE-ACTION mercury air-pump, invented by Signor Serravalle, who was awarded a gold medal for it at a recent exhibition in Messina, is described in the *Rivista Scientifico-Industriale* (Nos. 21–22). By a simple mechanical method two similar vessels are raised and lowered alternately with each other on opposite sides of a vertical support. A long caoutchouc tube connecting their bottoms lets mercury pass from one to the other. Each has at top a three-way cock; one port of which in a certain position leads into a small open vessel to receive any excess of mercury, and another is connected by means of a caoutchouc tube with a spherical piece fixed laterally about the middle of the vertical support. This piece has three passages, communicating together; two of them are opposite each other, and lead into the tubes from the mercury vessels; the other is connected by tubing to the vessel to be exhausted of air. The three-way cocks at the tops of the vessels are mechanically shifted at the top and bottom of their course by means of a toothed sector and rack in the one case, and a pin and projecting piece in the other.

To observe directly the action of gravity on gases, M. Kraievitsch, of the Russian Chemical Society (*Jour. de Phys.*, December, 1882), sets up two baro-manometers, one on the (low) ground, the other at the top of a high building, or of a hill. The manometric branches are connected by means of a long metallic tube. On rarefying the air in the tube through an adjutage adapted near one of the manometers, the rarefaction is propagated towards the other, but owing to gravity, the lower one always shows a greater pressure than the other. By varying the conditions, the hypsometric formula may be established directly. To ascertain whether gases have or have not a limit of elasticity, two baro-manometers are placed below and connected by separate tubes with the one above. On rarefying through a tube near one of the lower manometers, a limit is reached at which gravity prevents the air of the latter manometer from rising, and it remains stationary while the other continues to fall, if the limit of elasticity exist. The author was fitting up his apparatus for these experiments on a very high old building at St. Petersburg.

In another paper to the same Society (*loc. cit.*), M. Piltschikoff describes an arrangement for measuring the refractive index of liquids of which one has but small quantities. A hollow lens is filled with the liquid, and with the aid of a graduated scale and a microscope, one measures exactly the focal distance of a monochromatic flame placed at a given distance from the lens. The author gives a simple formula for calculating the index of the liquid, when the constants of the apparatus have been determined once for all. In one set of experiments, the index of glycerine was found = 1·47298, with a probable error estimated at ± 0·00001.

In the common practice of referring the electromotive force of galvanic combinations to the Daniell element as unit, some difficulty and confusion have arisen from differences in the construction of that element by different physicists. In a recent investigation of this matter (*Wied. Ann.*, 13, 1882), Herr Kittler gives the name of "normal element" to a combination, which is as follows:—Amalgamated, chemically pure zinc, in dilute sulphuric acid of specific gravity 1·075 at 18° C.; and chemically pure copper in concentrated copper sulphate solution of specific gravity 1·190 to 1·200 He finds that the electromotive force of the Daniell element (Zn, H_2SO_4, $CuSO_4$, Cu) increases with percentage proportion of the acid to a maximum occurring at the same place, whether the copper sulphate solution be concentrated or dilute, viz. with 25 to 30 per cent. of the acid; with further hydration of the acid there is decrease. The increase is greater, however, the more dilute the $CuSO_4$ solution used, and greatest with pure water. It is further found that, if very weak acids are used, there is decrease of the electromotive force with dilution of the copper-sulphate solution. Accordingly, there is a degree of concentration of the acid, with which a Daniell element furnishes the same tension, whether the $CuSO_4$ be concentrated or diluted to any extent. The solution in question has the specific gravity 1·0011 at 16° C., and is compounded of 750 ccm. H_2O and 100 ccm. dilute H_2SO_4 of sp. gr. 1·007. Herr Kittler compares the action of his "normal element" with that of other practical units.

In a recent paper to the Vienna Academy (*Wied. Ann.*, 13, 1882), Prof. Stephan describes an investigation of the magnetic screening action (*Schirmwirkung*) of iron (which is exemplified in Thomson's marine galvanometer and the Gramme machine). His experiments were made with hollow iron cylinders and iron rings, and were of three kinds, viz. deflection, oscillation, and induction.

The sound-vibrations of solid bodies (glass cylinders) in contact with liquids has been lately studied by Herr Auerbach (*Wied. Ann.*, No. 13, 1882). He finds that the *geometrical lowering of tone*, represented by the ratio of the vibration number (n_0) of the empty vessel to that of the same vessel filled with water (n), is smaller the higher the tone of the empty vessel, and greater the narrower the vessel. The *arithmetical lowering of tone* (represented by $(n_0 - n)n_0$) in a vessel of mean pitch, is inversely proportional to the square root of the vibration-number of the empty glass, and (approximately) to that of the number of wave-lengths which the sound of the empty vessel traverses from the wall to the axis. In glasses of different width it is (approximately) inversely proportional to the square root of the width. The specific lowering of tone of a liquid depends primarily on the density, and is greater, the greater this is, though it does not increase so quickly; next, on the compressibility, being greater the smaller this is.

AN INQUIRY INTO THE DEGREE OF SOLUBILITY REQUISITE IN MANURES, WITH SPECIAL REFERENCE TO PRECIPITATED CALCIC AND MAGNESIC PHOSPHATES

Some remarkable field trials, recently conducted in Scotland by Jamieson and others, have tended to raise serious doubts concerning the correctness of the high relative values, hitherto assigned by chemists to dissolved phosphates, commonly termed super-phosphates, for manurial purposes. We propose, therefore, to examine briefly the action of phosphates in the soil; the conditions under which they become available for the nutrition of plants, and the degree of solubility which, considering these facts, would appear to be most advantageous for the purposes of the agriculturist. We hope to be able to show the great value of precipitated calcic and magnesic phosphates as manure-ingredients, and to assign some reasons for the comparative neglect which the salts of magnesia have hitherto received from agricultural chemists.

The careful and elaborate series of experiments undertaken by Dr. Voelcker respecting the "solubility of phosphatic materials" may be said to constitute the basis of our present inquiry, as the behaviour of phosphates in water is perhaps the readiest test of their activity as manures. Dr. Voelcker ascertained that one gallon of distilled water will dissolve the following amounts of calcic phosphates, derived from the sources quoted:—

	Per gallon.	
Estremadura phosphorite...	0·10	grains.
Norwegian apatite	0·44	,,
Coprolites (mean of Suffolk and Cambridgeshire)	0·62	,,
Monk's Island phosphate	1·00	,,
Pure bone ash (from very hard bone)	1·18	,,
Pure tribasic phosphate of lime, precipitated, burnt and finely ground	2·20	,,
Guano	2·52	,,
Pure tribasic phosphate, precipitated and still moist	5·56	,,

The general deductions arrived at from these experiments, made about fifteen years ago, were that the phosphates in coprolites, apatite, and other phosphatic minerals were very little acted upon by water, and that "for agricultural purposes phosphatic minerals, as well as bone ash, should be treated with a quantity of sulphuric acid sufficient to convert the whole of the insoluble phosphates, therein contained, as completely as possible into soluble combinations. It is a waste of good raw materials to leave much of the insoluble phosphates unacted upon by acid." Broadly speaking, the above may be said to constitute the creed of the agricultural chemist at the present day, and the farmer buys his manure at a relatively high price, per unit of soluble phosphate.

On applying manures containing dissolved phosphates to the soil, nearly the whole of the phosphoric acid is at once neutralised by the various salts present therein, be they lime, alumina, or iron, and the chemist assures us that the superior estimation in which soluble salts are held arises from the property possessed by these salts of becoming rapidly diffused through the soil and precipitated therein, in an extremely fine state of sub-division. Voelcker, in some recent observations on this question, lays down certain propositions which are thus set forth in the abstract of the *Journal* of the Chemical Society, vol. xl. (1881) p. 640. These appear to us to state very clearly and briefly the accepted theories respecting the action of phosphates in manure.

1. Phosphates are not readily taken up by plants in a soluble form, but must be returned to an insoluble condition before they yield their useful properties.

2. The efficacy of insoluble calcium phosphate corresponds with the minuteness of division in which it is found in a manure.

3 The finer the particles in a phosphatic material, the easier it is dissolved in water, and the more energetic its action as a manure. Coarsely-ground coprolites and other minerals are less useful than the same materials in fine powder.

4. Calcium phosphate in porous soft bones is more soluble and energetic than in hard bones, and is more available in bone meal than in crushed bones.

5. Calcium phosphate in crystallised mineral phosphates—Norwegian, Canadian, and Spanish apatites, for example—is less

soluble and energetic than the same amount contained in porous phosphatic materials, such as certain descriptions of phospho-guano.

6. Treatment with acids renders the material completely soluble in water, and the so-formed superphosphate, when put into the ground, is precipitated in a very fine state of division.

7. In the precipitated state the insoluble phosphate is immeasurably more finely-divided than it could be obtained by mechanical means, and is consequently more energetic than any raw material mechanically ground.

8. The author's conclusion is that the chemical treatment with acid is the cheapest and best way of rendering mineral phosphates useful for agricultural purposes.

We think that it will be generally admitted that these propositions give a very reasonable statement of the case; but for the purposes of our inquiry we must supplement them with the following additional proposition. This has reference to a matter which has escaped the attention of Dr. Voelcker, but which is strongly supported by the results of the numerous recently-recorded practical trials.

"By reprecipitating the acid in a super-phosphate previous to its employment for agriculture by means of a suitable base, it becomes possible to obtain a neutral phosphate, possessed of a sufficient degree of solubility to be readily distributed through the soil, in an extremely fine state of subdivision, and capable of affording nutriment to the plant under highly favourable conditions."

It is to this further proposition to which we now desire to call special attention, and we may allude first to the assumed loss of the power of spontaneous diffusion through the soil, which is stated by Sibson, in his work on "Artificial Manures," to render the precipitated phosphates inferior in value to soluble acid phosphate. We think that no chemist will doubt that the phosphates in guano are sufficiently soluble to be available for plant food, and precipitated phosphate is certainly more soluble than the earthy phosphates in Peruvian guano. It must be remembered, moreover, when studying the table of solubilities of phosphates, as ascertained by Dr. Voelcker, that these are stated with reference to distilled water, which does not occur in nature, whereas in water containing small percentages of many of the salts, commonly present in the soil, the solubility of phosphates is largely increased. Thus the addition to the water of a trifling amount of ammonic chloride (1 per cent.) increases the solubility of precipitated calcic phosphate fourfold.

This matter has not then received a due share of attention, for, as we have seen, arguing on the analogy of guano, phosphates, in the precipitated form, are undoubtedly so far soluble as to possess the power of diffusion to an extent amply sufficient for agricultural purposes, and there must be a point, short of perfect solubility, which adequately satisfies all requirements in this respect. A careful consideration of the subject has led us to the conclusion that the effect of phosphoric acid added to the soil, after having been fixed by a suitable base, in a condition sufficiently soluble for every need of the plant, and in a state of subdivision far finer than anything which could be obtained by mechanical means, would be in theory, if not superior, at least equal to that of a similar amount of soluble phosphate, applied to a soil promiscuously, in cases in which it is impossible to predict by what bases the phosphoric acid will be fixed, or even whether it will be fixed at all. Indeed, the foregoing considerations would almost lead us to the belief that the employment of such ready-formed compounds as calcic or magnesic phosphates would be preferable to the haphazard use of soluble phosphoric acid in a super-phosphate.

Chemists in treating of the magnesic phosphates appear to have overlooked the dibasic phosphate and to have conducted their experiments and to have founded their observations mainly, if not entirely, on the behaviour of the far less soluble tribasic phosphate. The freshly-precipitated magnesic phosphate is soluble in about 322 times its weight of pure water, while calcic phosphate, as we have seen when newly precipitated, is soluble to the extent of 5·56 grains per gallon. Both of these salts are therefore much more soluble than the earthy phosphates present in guano. We must not overlook the fact also that although it has not yet received much attention, the magnesia would appear to possess in itself considerable manurial value. A recent French authority assigns to it a value approaching 5*s.* 8*d.* per unit, almost three-fourths of the price he sets down for phosphoric acid, and we are convinced from the study of the composition of numerous fertile soils, the ashes of plants, and recent field-trials, that the day is not far distant when the magnesia will rank as high in a manure as a salt of potash.

Another fact which the foregoing considerations have forcibly brought before us is the value of organic matters, in bringing about the solubility of the phosphates. This is perhaps scarcely within our present scope, but we have mentioned, incidentally, that small quantities of ammonia and carbonic acid, dissolved in the water, produce a very marked effect on the solubility of the phosphates. So valuable is their office in this respect, that it seems a false system to deny that organic matter, when present in a manure, possesses any value whatever. It was formerly the practice with agricultural chemists to allow 1*l.* per ton (2·4*d.* per unit) for organic matter, and we think that the important office which it fulfils in supplying carbonic acid for bringing into solution additional quantities of the phosphates, fully justifies the assignment to it of the above valuation.

We have thus endeavoured to explain the true conditions under which phosphoric acid becomes, in the soil, a source of plant-food. We have shown that there must be a limit to the value of solubility, merely considered as a means of securing diffusion through the soil, because partially soluble salts also possess the property to a degree sufficient for all practical purposes. In conclusion we have claimed for a ready-formed, partially-soluble phosphate, in a finely divided condition, and, in the case of the magnesic phosphate, possessing the property of fixing at the same time a portion of the ammonia, a value at least as great as that of a soluble acid phosphate, which runs the risk of being fixed by iron, or alumina (should lime be deficient in the soil), or, which may sink below the roots of the plants before it is neutralised. We trust we have thus shown a good case for a more liberal valuation of precipitated phosphates, and have indicated, with some measure of success, the reasons for the excellent results that have been recently obtained by the use of manures containing phosphoric acid in this form.

THE ELECTROLYTIC BALANCE OF CHEMICAL CORROSION[1]

THIS paper treats of some fundamental points in silver electro-plating, and shows how a large amount of the electric power may be wasted by the use of too large a proportion of free potassic cyanide in the plating solution, or by using the liquid in a heated state.

In it is also described a method of ascertaining the degree of energy of chemical corrosion of metals in electrolytes, by means of the strength of electric current per unit of surface necessary to prevent such corrosion; the metals and liquids employed for the purpose in the present research being silver, and solutions of argento cyanide of potassium containing free potassic cyanide. Numerous examples, chiefly in the form of tables, are given of the strength of current required to enter cathodes of a given amount of surface, in order to exactly balance the chemical corrosive effect upon them at atmospheric temperatures, and at higher ones, of solutions of potassic cyanide of various degrees of strength.

The method employed was to take a given solution of cyanide of potassium, pass through it by means of a sheet of platinum anode and a burnished sheet of silver cathode, a weak electric current, and add gradually to the liquid (with stirring) small portions of argento potassic cyanide, until the faintest perceptible deposit of silver occurred. The verge of deposition thus attained was called "the balance point;" and the conditions which determine and influence it, constitute the subject of this research.

The effect of various conditions upon the point of balance of electric and chemical energy were investigated, and the experiments are described. The influences examined were: composition of the liquid, strength of current, size of cathode and density of current, electro-motive force, temperature, ordinary chemical corrosion, nature of the cathode, etc. The circumstances were also investigated which affect the measurement of the current by the method employed in this research, viz. by depositing silver from a solution of argento potassic cyanide; and the sources of error, (and their limit), in that method, are pointed out. The effect of varying the proportions of free potassic cyanide, and of argento potassic cyanide, upon the strength of current at the balance point, are shown in tables of results. The strengths of current just sufficient to prevent all corrosion and to deposit the whole of the silver from a solution of argento potassic cyanide of given composition and containing free cyanide are also shown. The influence of varying the proportions both of argento potassic cyanide, and free cyanide of potassium, upon the transfer resistance[1] of the solution, and thereby upon the balance point, are also investigated and the results described.

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[1] Abstract of paper by G. Gore LL.D., F.R.S., read before the Birmingham Philosophical Society, Dec. 14, 1882.

A number of results and conclusions were arrived at, some of which are as follows:—variation either of the number of battery elements, the proportion of water, of free potassic cyanide, or of argento potassic cyanide, destroys the balance. The effect of altering the proportion of water is opposite with strong solutions to what it is with weak ones. The electric current at the point of balance appears to be entirely conveyed by the free potassic cyanide, and does not divide itself between the two salts until the liquid contains a certain proportion of argentic salt. In strong solutions of potassic cyanide, decreasing the number of battery cells, necessitates more cyanide of silver to restore the balance. The alteration of the point of balance by alteration of proportion of free potassic cyanide cannot be much accounted for by alteration of corrosive power of the liquid. A current from ten Smee's elements is about sufficiently strong to prevent all corrosion of silver at 60° F. in a solution of cyanide of potassium containing a mere trace of argento potassic cyanide. The addition of nitrate, chloride, iodide, or sulphate of potassium to the cyanide solution has but little effect upon the balance point. Variation of strength and of "density" of current affect greatly the point of balance. Greater "density" irrespective of strength of current usually increases the amount of silver deposited. Difference of electro-motive force of current had no conspicuous effect in altering the balance point. Rise of temperature of the liquid acts in two opposite ways, it increases the corrosive action, and by diminishing conduction-resistance it increases the current, and as the latter effect is usually a little stronger than the former one, rise of temperature alters slightly the point of balance, and enables the current to produce a sparing deposit of silver. The ordinary chemical corrosion of silver in a solution of potassic cyanide without an electric current is increased slightly by partial immersion (through capillary corrosion), and greatly by rise of temperature; it is also slightly greater in a weak solution than in a strong one, with solutions of a certain range of strength; and it is distinctly increased by contact with platinum. In consequence of the latter circumstance, a platinum cathode requires a somewhat stronger current than a silver one to enable the point of balance to be attained. In a mixed solution of potassic and argento potassic cyanides, even the smallest proportion of the former salt conveys a portion of the current, and if the cathode is large or the current is sufficiently weak, the whole of it is conveyed by that salt, however. much of the double salt is present, an error is thereby introduced when deposition of silver in such a liquid is used as a measure of current. But with a large amount of the double salt, a small amount of potassic cyanide, and a current sufficiently strong, the proportionate amount of error is small. During the act of deposition the cathode surface is not at all corroded, and any deficiency in the weight of deposit is not due to corrosion, but to a portion of the current being conveyed by other ingredients of the liquid than the argentic salt. A current which produces deposition of silver, prevents all corrosion of a silver cathode in the same liquid. The addition of free potassic cyanide to a solution of the double cyanide alters both the resistance and the balance point. The quantity of current diverted from the argentic salt in solution is directly proportional to the amount of free potassic cyanide present, but not always in the same ratio. The presence of a large proportion of free cyanide, together with the employment of a feeble current conduce to the passage of a large amount of current through the liquid without depositing silver; and a current of ·001057 Ampere (which would deposit ·132 grain of silver in two hours) was hardly strong enough to prevent all corrosion or to deposit any silver from a solution composed of 37·5 grains of argento potassic cyanide and 112·5 grains of free cyanide of potassium in three ounces of water. Whilst also a current if sufficiently weak, may traverse a solution of potassic cyanide containing double cyanide, without any of the current decomposing the latter, it cannot traverse a solution of double salt containing free potassic cyanide without some of it traversing the cyanide of potassium. With a very dense current also, a portion of it enters the cathode without depositing silver, and evolves gas.

It requires a much stronger current to balance the corrosion in a hot solution of the two cyanides than in a cold one, and in an instance given, a rise of temperature from 60 to 120° F. was attended by the passage of 21 per cent increase of current without deposition of silver. Addition of free potassic cyanide to a weak solution of the double salt *at the balance point*, first decreases and then increases the current by altering the transfer resistance, probably at the cathode. An amount of current equal to ·14857 Ampere, entering a surface of ⅞ths. of a square inch, was found to be sufficiently strong to deposit nearly the whole of the silver from a solution at 60° F. composed of 70·11 grains of free potassic cyanide, ·0297 grain of double cyanide, and three ounces of water, the liquid retaining dissolved a little less than that amount of silver at its balance point under those conditions. The strength of current *at the balance point* in a weak solution of potassic cyanide, varies inversely as the amount of silver salt added, and at about eight times the rate. A certain strength of current must enter a given surface of silver in a given liquid under stated conditions in order to prevent all corrosion and produce deposition. The addition of the double cyanide reduces the amount of current conveyed by the free potassic cyanide into the cathode *at the balance point*. Successive additions of double salt to a solution of potassic cyanide *not at the balance point*, first decreases and then increases the current by altering the transfer resistance; it alters the relation of the molecules of potassic cyanide to the cathode so as to diminish their power of transmitting current into that surface without depositing silver. The greater the proportion of double salt present, the greater the tendency to the deposition of silver. Addition of potassic cyanide to a weak solution of the double salt *not at the balance point*, first decreases and then increases the current by altering the transfer resistance at the cathode; in this respect it behaves like addition of the double salt to a weak solution of potassic cyanide. With cathodes of platinum, a solution of potassic cyanide offered less resistance to the current (*not at the balance point*) than one of the double cyanide, but with silver cathodes the reverse effect occurred.

The balance point is a case of equalization of molecular influences, including ordinary chemical corrosion, density of current, nature of cathode, temperature, proportions of water, argento potassic cyanide, free potassic cyanide, and the soluble salts present as impurities, either of which by being disturbed, alters all the others. All these influences also have separate numerical values. A rise of temperature of 60° F. requires an increase of ·000976 Ampere to restore the equipoise. The experiments illustrate the dynamics of electro silver plating; and the method employed in the research is applicable to the detection and measurement of molecular influence in electrolytes. In consequence of the alteration of any one of the conditions having the effect of altering all the remainder, all the above conclusions are limited in their application and are only correct under the conditions given in the paper. The fundamental explanation underlying these conclusions is, that the phenomena are essentially molecular; and that the mere presence and admixture of the double cyanide alters the molecular arrangement of the free cyanide *not at the balance point*, in such a way as to enable the latter to transmit a greater quantity of current into a cathode of given size, notwithstanding its being more diluted by the other salt.

The phenomena of the "balance point" constitute an interesting example of molecular equilibrium, in which the balance point may be compared to a ball suspended by an elastic cord, and having attached to it, a number of other similar cords, each drawing it in a different direction, and all of them being kept in a state of tension. In such a case an alteration of the degree of strain of any one of the cords, changes that of all others, and alters the position of the ball.

The research has a practical bearing both upon the measurement of electric currents by means of deposition of silver from a cyanide solution, and upon the technical process of electro plating. In the former it shows how a large proportion, or even the whole of a current may pass without being measured, and how the error may be reduced to the smallest amount; and in the latter, how a similar waste of current may occur, and how to prevent it.

It is manifest from the foregoing research, that the electrolytic balance of chemical corrosion of cathodes in other depositing solutions, such as those of gold, copper, nickel, etc., might form an extensive subject of experimental investigation.

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[1] By transfer resistance is meant the resistance to transfer of the current into the cathode.

Appended Note.—It was constantly found that in using a non-corrodible anode such as platinum, the amount of current passing was very much more easily regulated by varying the size of the anode than that of the cathode, with a corrodible anode however, such as silver, this effect was not observed.

THE ETHER AND ITS FUNCTIONS[1]

II.

Consider the effect of wind on sound. Sound is travelling through the air at a certain definite rate depending simply on the average speed of the atoms in their excursions, and the rate at which they therefore pass the knocks on; if there is a wind carrying all the atoms bodily in one direction, naturally the sound will travel quicker in that direction than in the opposite. Sound travels quicker with the wind than against it. Now is it the same with light: does it too travel quicker with the wind? Well that altogether depends on whether the ether is blowing along as well as the air; if it is, then its motion must help the light on a little; but if the ether is at rest no motion of air or matter of any kind can make any difference. But according to Fresnel's hypothesis it is not wholly at rest nor wholly in motion; the free is at rest, the bound is in motion; and therefore the speed of light with the wind should be increased by an addition of $\left(1 - \frac{1}{\mu^2}\right)$th of the velocity of the wind. Utterly infinitesimal, of course, in the case of air, whose μ is but a trifle greater than 1; but for water the fraction is 7-16ths, and Fizeau thought this not quite hopeless to look for. He accordingly devised a beautiful experiment, executed it successfully, and proved that when light travels with a stream of water, 7-16ths of the velocity of the water must be added to the velocity of the light, and when it travels against the stream the same quantity must be subtracted, to get the true resultant velocity.

Arago suggested another experiment. When light passes through a prism, it is bent out of its course by reason of its diminished velocity inside the glass, and the refraction is strictly dependent on the retardation; now suppose a prism carried rapidly forward through space, say at the rate of eighteen miles a second by the earth in its orbit, which is the quickest accessible carriage; if the ether is streaming freely through the glass, light passing through will be less retarded when going with the ether than when going against it, and hence the bending will be different.

Maxwell tried the experiment in a very perfect form, but found no difference. If all the ether were free there would have been a difference; if all the ether were bound to the glass there would have been a difference the other way; but according to Fresnel's hypothesis there should be no difference, because according to it, the free ether, which is the portion in relative motion, has nothing to do with the refraction; it is the addition of the bound ether which causes the refraction, and this part is stationary relatively to the glass, and is not streaming through it at all. Hence the refraction is the same whether the prism be at rest or in motion through space.

An atom imbedded in ether is vibrating and sending out waves in all directions; the length of the wave depends on the period of the vibration, and different lengths of wave produce the different colour sensations. Now through free ether all kinds of waves appear to travel at the same rate; not so through bound ether; inside matter the short waves are more retarded than the long, and hence the different sizes of waves can be sorted out by a prism. Now a free atom has its own definite period of vibration, like a tuning-fork has, and accordingly sends out light of a certain definite colour or of a few definite colours, just as a tuning-fork emits sound of a certain definite pitch or of a few different pitches called harmonics. By the pitch of the sound it is easy to calculate the rate of vibration of the fork; by the colour of the light one can determine the rate of vibration of the atom.

When we speak of the atoms vibrating, we do not mean that they are wagging to and fro as a whole, but that they are crimping themselves, that they are vibrating as a tuning-fork or or a bell vibrates; we know this because it is easy to make the free atoms of a gas vibrate. It is only in the gaseous state, indeed, that we can study the rate of vibration of an atom; when they are packed closely together in a solid or liquid, they are cramped, and all manner of secondary vibrations are induced. They then, no doubt, wag to and fro also, and in fact these constrained vibrations are executed in every variety, and the simple periodicity of the free atom is lost.

To study the free atoms we take a gas—the rarer the better—heat it, and then sort out the waves it produces in the ether by putting a triangular prism of bound ether in their path.

Why the bound ether retards different waves differently; or *disperses* the light, is quite unknown. It is not easy accurately to explain refraction, but it is extremely difficult to explain dispersion. However, the fact is undoubted, and more light will doubtless soon fall upon its theory.

The result of the prismatic analysis is to prove that every atom of matter has its own definite rate of vibration, as a bell has; it may emit several colours or only one, and the number it emits may depend upon how much it is struck (or heated), but those it can emit are a perfectly definite selection, and depend in no way on the previous history of the atom. Every free atom of sodium, for instance, vibrates in the same way, and has always vibrated in the same way, whatever other element it may have been at intervals combined with, and whether it exists in the sun or in the earth, or in the most distant star. The same is true of every other kind of matter, each has its own mode of vibration which nothing changes; and hence has arisen a new chemical analysis, wherein substances are detected simply by observing the rate of vibration of their free atoms, a branch of physical chemistry called spectrum analysis.

The atoms are small bodies, and accordingly vibrate with inconceivable rapidity.

An atom of sodium vibrates 5×10^{14} times in a second; that is, it executes five hundred million complete vibrations in the millionth part of a second.

This is about a medium pace, and the waves it emits produce in the eye the sensation of a deep yellow.

4×10^{14} corresponds to red light, 7×10^{14} to blue.

An atom of hydrogen has three different periods, viz. 4·577, 6·179, and 6·973, each multiplied by the inevitable 10^{14}.

Atoms may indeed vibrate more slowly than this, but the retina is not constructed so as to be sensible of slower vibrations; however, thanks to Capt. Abney, there are ways now of photographing the effect of much slower vibrations, and thus of making them indirectly visible; so we can now hope to observe the motion of atoms over a much greater range than the purely optical ones and so learn much more about them.

The distinction between free and bound ether is forced on our notice by other phenomena than those of light. When we come to electricity, we find that some kind of matter has more electricity associated with it than others, so that for a given electromotive force we get a greater electric displacement; that the electricity is, as it were, denser in some kinds of matter than in others. The density of electricity in space being 1, that inside matter is called K, the specific inductive capacity. In optics the density of the ether inside matter was μ^2. These numbers appear to be the same.

Is the ether electricity then? I do not say so, neither do I think that in that coarse statement lies the truth; but that they are connected there can be no doubt.

What I have to suggest is that positive and negative electricity together may make up the ether, or that the ether may be sheared by electromotive forces into positive and negative electricity. Transverse vibrations are carried on by shearing forces acting in matter which resists them, or which possesses rigidity. The bound ether inside a conductor has no rigidity; it cannot resist shear; such a body is opaque. Transparent bodies are those whose bound ether, when sheared, resists and springs back again; such bodies are dielectrics.

We have no direct way of exerting force upon ether at all; we can, however, act on it in a very indirect manner, for we have learnt how to arrange matter so as to cause it to exert the required shearing (or electromotive) force upon the ether associated with it. Continuous shearing force applied to the ether in metals produces a continuous and barely resisted stream of the two electricities in opposite directions, or a conduction current.

Continuous shearing force applied to the ether in transparent bodies produces an electric displacement accompanied by elastic resilience, and thus all the phenomena of electric induction.

Some chemical compounds, consisting of binary molecules, *distribute* the bound ether of the molecule, at any rate as soon as it is split up by dissociation; and, instead of each nascent radicle or atom taking with it neutral ether, one takes a certain definite quantity of positive, the other the same amount of negative, electricity. In the liquid state the atoms are capable of locomotion; and a continuous shearing force applied to the ether in such liquids causes a continual procession of the matter and associated electricity, the positive one way, and the negative the other, and thus all the phenomena of electrolysis.

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[1] A lecture by Prof. Oliver Lodge at the London Institution, on December 28, 1882. Continued from p. 306.

What I say about electricity, however, is not to be taken without salt, you will not regard it as recognised truth, but as a tentative belief of your lecturer's which may be found to be more or less, and possibly more rather than less, out of accordance with facts. I can only say that it hangs phenomena together, and that it has been forced upon my belief in various ways.

Now what about the free ether of space, is it a conductor of electricity? There are certain facts which suggest that it is, and Edlund has suggested that it is an almost perfect conductor. When a sun-spot or other disturbance breaks out on the sun, accompanied as it is, no doubt, by violent electric storms, the electric condition of the earth is affected, and we have auroræ and magnetic disturbances. Is this by induction through space? or can it be due to conduction and the arrival of some microscopic portion of a derived current travelling our way?

For my part I cannot think the ether a conductor. Maxwell has shown that conductors must be opaque, and ether is nothing if not transparent; one is driven, then, to conclude that what we call conduction does not go on except in the presence of ordinary matter—in other words, perhaps, that it is a phenomena more connected with bound ether than with free.

But now, looking back to Fresnel's hypothesis of the extra density of the ether inside gross matter, and also to the fact that it must be regarded as incompressible, the question naturally arises how can it be densified by matter or anything else? Perhaps it is not; perhaps matter only strains the ether towards itself, thus slackening its tension, as it were, inside bodies, not producing any real increase of density; and this is roughly McCullagh's form of the undulatory theory. In this form gravitation may be held to be partially explained; for two bodies straining at the ether in this way will tend to pull themselves together. In fact Newton himself pointed out that gravitation could be produced if only matter exerted this kind of strain on all pervading ether, the tension varying as the inverse distance.

He did not follow the idea up, however, because he had then no other facts to confirm him in his impression of the existence of such an ether, or to inform him concerning its properties. We now not only feel sure that an ether exists, but we know something of its properties; and we also have learnt from light and from electricity, that some such action between matter and ether actually occurs, though how or why it occurs we do not yet know. I am therefore compelled to believe that this is certainly the direction in which an ultimate explanation of gravitation and of cohesion is to be looked for.

In thinking over the Fresnel and McCullagh forms of the undulatory theory, with a view to the reconciliation between them which appears necessary and imminent, one naturally asks, is there any such clear distinction to be drawn between ether and matter as we have hitherto tacitly assumed? may they not be different modifications, or even manifestations, of the same thing?

Again, when we speak of atoms vibrating, how can they vibrate? of what are their parts composed?

And now we come to one of the most remarkable and suggestive speculations of modern times—a speculation based on this experimental fact, that the elasticity of a solid may be accounted for by the motion of a fluid; that a fluid in motion may possess rigidity.

I said that rigidity was precisely what no fluid possessed; at rest this is true; in motion it is not true.

Consider a perfectly flexible india-rubber O-shaped tube full of water; nothing is more flaccid and limp. But set the water rapidly circulating, and it becomes at once stiff; it will stand on end for a time without support; kinks in it take force to make, and are more or less permanent. A practicable form of this experiment is the well-known one of a flexible chain over a pulley, which becomes stiff as soon as it is set in rapid motion.

This is called a vortex filament, and a vortex is a thing built up of a number of such filaments. If they are arranged parallel to one another about a straight axis or core, we have a vortex cylinder such as is easily produced by stirring a vessel of water, or by pulling the plug out of a wash-hand basin; or such as are made in the air on a large scale in America, and telegraphed over here, when they are called "cyclones," or "depressions." The *depression* is visible enough in the middle of revolving water. These vortices are wonderfully permanent things, and last a long time, though they sometimes break up unexpectedly.

Vortices need not have straight cores, though they may have cores of various ring forms, the simplest being a circle. To make a vortex ring, we must take a plane disk of the fluid, and at a certain instant give to every atom in the disk a certain velocity forward, graduating the velocity according to its distance from the edge of the disk. We have as yet no means of doing this in a frictionless fluid, but with a fluid such as air and water it happens to be easy; we have only to knock a little of the fluid suddenly out of a box through a sharp-edged hole, and the friction of the edges of the hole does what we want. The central portion travels rapidly forward, and returns round outside the core, rolling back towards the hole. But the impetus sends the whole forward, and none really returns; it rolls on its outer circumference as a wheel rolls along a road. In a perfect fluid it need not so roll forward, as there would be no friction, but in air or water a vortex-ring has always a definite forward velocity, just as a locomotive driving-wheel has when it does not slip on the rails.

We have in these rings a real mass of air moving bodily forward, and it impinges on a face or a gas flame with some force. It is differentiated from the rest of the atmosphere by reason of its peculiar rotational motion.

The cores of these rings are elastic—they possess rigidity; the circular is their stable form, and if this is altered, they oscillate about it. Thus when two vortex rings impinge or even approach fairly near one another, they visibly deflect each other, and also cause each other to vibrate.

The theory of the impact or interference of vortex rings whose paths cross, but which do not come very near together has been quite recently worked out by Mr. J. J. Thomson. It is quite possible to make the rings vibrate without any impact, by serrating the opening out of which they are knocked. The simplest serration of a circle turns it into an ellipse, and here you have an elliptic ring oscillating from a tall to a squat ellipse and back again. Here is a four-waved opening, and the vibrations are by this very well shown. A six-waved opening makes the vibrations almost too small to be perceived at a distance but still they are sometimes distinct.

The rings vibrate very much like a bell vibrates, perhaps very much like an atom vibrates. They have rigidity, although composed of fluid; they are composed of fluid in motion. These vortices, are imperfect they increase in size, and decrease in energy; in a perfect fluid they would not do this, they would then be permanent and indestructible, but then also you would not be able to make them.

Now does not the idea strike you that atoms of matter may be vortices like these—vortices in a perfect fluid, vortices in the ether. This is Sir William Thomson's theory of matter. It is not yet proved to be true, but is it not highly beautiful?—a theory about which one may almost dare to say that it deserves to be true. The atoms of matter according to it are not so much foreign particles imbedded in the all-pervading ether as portions of it differentiated off from the rest by reason of their vortex motion, thus becoming virtually solid particles, yet with no transition of substance; atoms indestructible and not able to be manufactured, not mere hard rigid specks, but each composed of whirling ether; elastic, capable of definite vibration, of free movement, of collision. The crispations or crimpings of these rings illustrate the kind of way in which we may suppose an atom to vibrate. They appear to have all the properties of atoms except one, viz. gravitation; and before the theory can be accepted, I think it must account for gravitation. This fundamental property of matter cannot be left over to be explained by an artificial battery of ultra-mundane corpuscles. We cannot go back to mere impact of hard bodies after having allowed ourselves a continuous medium. Vortex atoms must be shown to gravitate.

But then remember how small a force gravitation is. Ask any educated man whether two pound-masses of lead attract each other, and he will reply no. He is wrong, of course, but the force is exceedingly small. Yet it is the aggregate attraction of trillions upon trillions of atoms; the *slightest* effect of each upon the ether would be sufficient to account for gravitation; and no one can say that vortices do not exert some such residual, but uniform, effect on the fluid in which they exist, till second, third, and every other order of small quantities have been taken into account, and the theory of vortices in a perfect fluid worked out with the most final accuracy.

At present, however, the Thomsonian theory of matter is not a verified one, it is, perhaps, little more than a speculation, but it is one that it is well worth knowing about, working at, and inquiring into. It may stand or it may fall, but if it is the case, as I believe it is, that our notions of natural phenomena, though they often fall short, yet never exceed in grandeur the real truth of things, how splendid must be the real nature of matter if the Thomsonian hypothesis turns out to be inadequate and untrue.

I have now endeavoured to introduce you to the simplest conception of the material universe which has yet occurred to man. The conception that is of one universal substance, perfectly homogeneous and continuous and simple in structure, extending to the furthest limits of space of which we have any knowledge, existing equally everywhere. Some portions either at rest or in simple irrotational motion transmitting the undulations which we call light. Other portions in rotational motion, in vortices that is, and differentiated permanently from the rest of the medium by reason of this motion.

These whirling portions constitute what we call matter; their motion gives them rigidity, and of them our bodies and all other material bodies with which we are acquainted are built up.

One continuous substance filling all space: which can vibrate as light; which can be sheared into positive and negative electricity; which in whirls constitutes matter; and which transmits by continuity, and not by impact, every action and reaction of which matter is capable. This is the modern view of the ether and its functions.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Lord Rayleigh has resumed his course of lectures on Electrical Measurements.

Dr. Gaskell's lectures this term deal with the Physiology of the Circulation; Mr. Langley is lecturing on the Physiology of Muscle and Nerve, and the Histology and Pathology of the Secretory Organs.

SCIENTIFIC SERIALS

Transactions of the New York Academy of Sciences, Nos. 2-5, 1881-82.—Outlines of the geology of the North-eastern West India Islands, by Prof. Cleve.—The excavation of the bed of the Kaaterskill, New York, by Dr. Julien.—On the cell-doctrine and the bioplasson doctrine, by Prof. Elsberg.—The discovery of the North Pole practicable, by Commander Cheyne.—The volcanic tuffs of Challis, Idaho, and other western localities, by Dr. Jullien.—The mammoth cave of Kentucky, by Mr. Stevens.—On the determination of the heating-surface required in steam pipes employed to produce any required discharge of air through ventilating chimneys, by Prof. Trowbridge.—On a peculiar coal-like transformation of peat, recently discovered at Scranton, Penn., by Prof. Fairchild.—The parallel drift-hills of Western New York, by Dr. Johnson.—Hypothetical high tides as agents of geological action, by Dr. Newberry.—The international time-system, by Prof. Rees. The moral bearing of recent physical theories, by Prof. Martin.—The discovery of emeralds in South Carolina, by Mr. Hidden.—Obituary notice of Prof. J. W. Draper.—On the behaviour of steam in the steam-engine cylinder, and on curves of efficiency, by Prof. Thurston.—Stereoscopic notes, by Prof. Hines.—A new reversible stereoscope, by Mr. Stevens.—Diphenylamine-acrolein, by Prof. Leeds.

Annalen der Physik und Chemie, No. 1, 1883.—On the radiometer, by E. Pringsheim.—A wave-length measurement in the ultra-red solar spectrum, by the same.—Fluorescence according to Stokes' law, by E. Hagenbach.—The isogyrous surfaces of doubly-refractive crystals; general theory of the curves of like direction of vibration, by E. Lommel.—On the heat-conducting power of liquids, by L. Graetz.—On the ratio of the specific heats in gases and vapours, by P. A. Müller.—The product of internal friction and galvanic conduction of liquids is constant with reference to the temperature, by L. Grossmann.—On M. Guébhard's proposed method of determination of equipotential lines, by H. Meyer.—Further researches on the relation of molecular refraction of liquid compounds to their chemical constitution, by H. Schröder.—On the preservation of oxygen gas in the zinc-gasometer, by J. Loewe.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 11.—"On the Skeleton of the Marsipobranch Fishes. Part I. The Myxinoids (*Myxine* and *Bdellostoma*)." By W. K. Parker, F.R.S. Abstract.

In their cranio-facial skeleton the Myxinoids are very remarkable; where segmentation is perfect in other fishy types there they only exhibit a lattice-work of continuous growth; in the median region of the skull-base, where other types show but little or only temporary distinctness of parts, these fishes develop and retain large independent cartilages.

The lamprey has a large superficial basket-work of soft cartilage (*extra-branchial*), and its gill-pouches keep this related to the rest of the structures of the mouth and throat. But in the Myxinoids the basket-work is *intra-branchial*, and corresponds to the system of segmented arches of the higher Cartilaginous, the Ganoid, and the Osseous fishes. But these non-segmented arches soon lose all relation to the branchial pouches, which are removed so far backwards that they begin under the *twentieth myotome;* whilst the end of the pericardium is under the *fortieth.*

In seeking light upon the primordial condition of the Vertebrata, one naturally looks to such forms as the Myxinoids. For in these types, even in the adult state, there are neither limbs nor vertebræ, and no distinction between head and body, except the beginning, in the head, of a cartilaginous skull; a *continuous structure*—not showing the least sign of secondary segmentation, and by far the greater part of which is in front of the notochord, or axis of the organism. But here our *gradational* work agrees with the *developmental*, for the continuous skull-bars constantly arise before the secondary cartilaginous segments that are found between the myotomes behind the head. Evidently, therefore, the early "Craniata" grew supports to the enlarged and subdivided front end of their neural axis, long before anything beyond strong fibrous septa were developed between the muscular segments of the body. As for the linear growth, the greater or less extension backwards of the main organs—circulatory, respiratory, digestive, urogenital—that, in the evolution of the primary form, was a thing to be determined by the "surroundings" of the type. "Thereafter as *they* may be" was the tentative idea in this case.

Certainly, in the Marsipobranchs and in their relations, the larval "Anura," we have the most archaic "Craniata" now existing; in these the organs may be extended far backwards in a vermiform creature, as in these low fishes, or kept well swung beneath the head—the body and tail together forming merely a propelling organ, as is seen in Tadpoles, especially the gigantic Tadpole of *Pseudis.*

Thus we see that in low limbless types there is no necessity for the development of more than fibrous "metameres"; but the vesicular brain, the suctorial lips, the branchial pouches, and the special organs of sense—these all call for support from some tissue more dense than a mere fibrous mat or web. In the *Myxinoids* we find that *four* special modifications of the connective tissue series are developed for the support of the properly *cephalic* organs, and for them only; thus these fishes are *Craniata*, but are not *Vertebrata;* that is, if we stick to the letter, which of course we do not.

At first some disappointment is felt, after careful study of these types, for, notwithstanding the low level in which they remain, they are mere specialised *Ammocœtes*, keeping on the same "platform" as the larval Lamprey; yet some parts of their organisation do undergo a marvellous amount of transformation, and are, indeed, as much specialised in conformity with their peculiar habits of life *as any Vertebrates whatever, the highest not excepted.*

Yet, on the whole, the Msxinoids are a sort of *Ammocœtine* type, whilst the transformed Ammocœte, the larval Lamprey, comes nearest to the untransformed Frog or Toad—the *Tadpole.* But the mere putting of this shows (suggests at any rate) what *losses* the fauna of the world has sustained during the evolution of the Craniate forms; *now*, the Myxinoids, Petromyzoids, and anurous Amphibia, must all be kept "within call" of each other; but the types that have been culled out between them cannot be numbered. Some other kind of fish are evidently the descendants of primordial "Marsipobranchs," notably *Lepidosteus*, the development of which has been lately studied, and the results are being published in the *Philosophical Transactions*. But the *Chimæroids*, *Dipnoi*, and, still more important, the *Myxinoids* themselves, have still to be followed through their early stages. If the present paper is of any value to the morphologist, one on the embryology of these low forms would be worth much more.

The Myxinoids keep on the low "platform" of the larval Lamprey (*Ammocœte*) in the following particulars, namely:—

a. The notochord has no paired cartilaginous vertebral rudiments in the spinal region.

b. The trabeculæ end in the ethnoidal region, without growing forwards into a cornu (or *two continuous* cornua).

c. There are merely "barbels" round the mouth; no *labial cartilages*.

d. The last character involves this, namely, that the special armature of horny teeth, attached to the labials in the adult *Petromyzon*, is absent.

e. The organs of vision are very feeble, and probably almost useless; in the *Ammocœte* they are arrested for a time.

f. The cranium is a mere *floor*, without side-walls or roof.

The Myxinoids come near to the adult Lamprey in the following particulars, namely:—

a. There are developed outside the skull proper, but not segmented from it, palato-pterygoid and hyoid cartilages.

b. There is a very large median cartilage belonging to both the hyoid and branchial regions.

c. The cranium acquires a floor by the development of a special "hinder intertrabecula."

d. There is a large median cartilaginous olfactory capsule.

The Myxinoids go beyond even the adult Lamprey in the following particulars, namely:—

a. The facial basket-work is much more perfect; and as this is a generalised condition of the true *intra-visceral* system of cartilages, it is a very important character; there is not only an equal development of the "suspensorium," but the *suspensorial part* of the hyoid is developed also (it is suppressed in the Lamprey); and there is, in *Bdellostoma* a large complete first branchial arch, and in both kinds pharyngo-branchial rudiments of the second branchial arch.

b. The respiratory (branchial) pouches are much more specialised by being carried far back under the spine.

c. There is not only a distinct sub-cranial intertrabecula, but also a large pre-cranial or nasal median cartilage of the same nature.

d. The opening of the median olfactory sac is not a mere short membranous passage, but a long tube, encased in a series of cartilaginous (imperfect) rings.

e. Correlated with the non-development of the suctorial labial cartilages, there is an enormous development of the lingual, the basal bar becoming not only double, but, in front, quadruple, and the "supra-lingual" cartilages, which are very small in the Lamprey, and carry only one pair of rows of small second teeth, in the Myxinoids are very large, and carry two pairs of rows of large teeth, with the addition of a median antagonistic "ethnoidal tooth."

Lastly, the greater development of the intra-visceral (= "intra-branchial") cartilages is correlated with the suppression of the extra-visceral basket-work seen both in the larval and adult Lamprey, and also in the larvæ of the "Anura" generally.

January 18.—"On the Skeleton of the Marsipobranch Fishes. Part II. The Lamprey." By W. K. Parker, F.R.S.

The suctorial mouth has its highest development in the Lamprey; in the Myxinoids (*Myxine*), and Bdellostoma, there is no circular disk with horny teeth, but merely an oral fissure surrounded by barbels, and having inside it a huge tongue beset with two oblique rows of recurved and inturned horny teeth, antagonised by a single ethnoidal tooth. In the larva of the Lamprey the mouth is not circular, and the *lower* lip is far back, covered by the *upper*, which is like a hood; there are no teeth of any kind, only moss-like "barbels" or *papillæ* under the upper lip.

In the Tadpole the mouth is suctorial, the *lower* lip being converted into an imperfect ring, which is completed by the *upper* lip. Here the cartilage of the lower lip is not a perfect ring, as in the Lamprey, but is in two parts, and is formed into a sort of *horseshoe*. Inside this compound ring there are sharp horny plates or teeth, and the folds of the lips, all round the mouth, are covered with a horny rasp.

Correlated with the perfectly suctorial *lower* lip of the Lamprey, which, is a *post-oral* structure, entirely, we have the perfectest form of the superficial branchial skeleton, a basket-work of soft cartilage which appears in the early embryo, and only gains enlargement fore and aft, and all its snags and outgrowths, after metamorphosis. Besides this there are no rudiments of *internal* branchial arches, such as we find in the Tadpole. The only parts developed *inside* the head-cavities and branchial arches are the generalised and rudimentary mandibular and hyoid arches. In the Tadpole there is no *pier* to the hyoid arch, and the *first cleft* is arrested as a small blind pouch; this state is persistent in the Lamprey. But, after metamorphosis—as the lingering latter part of that profound change of structure—the young Frog and Toad acquire a pier to their hyoid arch, right and left. This, however, does not become functional to the arch, much less assist in supporting the mandible, as a "hyomandibular," but is transformed into an osseo-cartilaginous chain —a *stapedio-incudal* series, specialised correlatively with the expanded rudiment of the first cleft, now enlarged into a *cavum tympani*, with a large "Eustachian opening." The little mandibles of the Tadpole, which served as arms to carry the divided suctorial disk, and lay across the fore face, become very long, and are often hinged on to their pier behind the occiput, and the cartilages of the suctorial disk straighten out and add to the length of the lower jaw in front. These things show how this *temporary* "Petromyzoid," the Tadpole, blossoms out into unthought-of specialisations; it becomes a *quasi-reptile*, worthy of a place far above the Lamprey, and even far above all other *Ichthyopsida*.

Geological Society, January 10.—J. W. Hulke, F.R.S., president in the chair.—T. W. Edgeworth David, the Earl of Dysart, John James Hamilton, Francis Alfred Lucas, and Meaburn Staniland, were elected Fellows, and Dr. Otto Torell, F.C.G.S., of Stockholm, a Foreign Member of the Society.—The following communications were read:—On the Lower Eocene section between Reculvers and Herne Bay, and some modifications in the classification of the Lower London Tertiaries, by J. S. Gardner, F.G.S.—The author noticed Prof. Prestwich's classification of the Lower London Tertiaries, and the introduction by the Survey of the term "Oldhaven Beds" for some of his basement beds of the London Clay. He next discussed the conditions under which the Lower Tertiaries were produced, and showed that throughout the Eocenes there are indications of the close proximity of land and of the access of fresh water. Two types of faunas are to be recognised, namely, those of the Calcaire Grossier and the London Clay, the latter indicating more temperate climatal conditions. The former is represented in England by the Bracklesham series. The areas of these two faunas were separated by land forming an isthmus, as each formation is bounded by a shore-line and separated from its neighbours by freshwater formations; but this isthmus probably shifted its position to the north and south without ever being broken through. A vast Eocene river existed, draining a great continent stretching westward; the indications of this river in Hampshire and Dorsetshire would show it to have been there seventeen or eighteen miles wide.—The Lower Tertiaries have been divided by Prof. Prestwich and the Survey into the marine Thanet beds, the fluviatile, estuarine and marine Woolwich and Reading Beds, and the marine Oldhaven Beds. The mode of occurrence of these was described by the author, with especial reference to the section between Herne Bay and the Reculvers, from his investigation of which he was led to the following conclusions:—The Thanet Sands were probably deposited by a rough sea outside the estuary of the great Eocene river, but within its influence. This area became silted up, rose above the surface, and became covered with shingle and sand. The Thanet Beds closed with a period of elevation, during which the Reading Beds were formed, and this was followed by a subsidence during the Woolwich period, which finally ushered in the Oldhaven and London Clay deposits. The formation of the Oldhaven Beds may be compared with that of the modern beach at Shellness; and during the period of depression the beaches would advance steadily over the flat area of Sheppey, and the earlier formed ones would sink and become covered up by the silt of the great Eocene river. These beaches, forming vast aggregations of sand and shingle between the Thanet Beds and the London Clay, form integral portions of one or other formation, and cannot be recognised as forming a separate formation at all equivalent to the other divisions of the Eocene.—On Mr. Dunn's Notes on the Diamond-fields of South Africa, 1880, by Francis Oates, F.G.S.

Anthropological Institute, January 23.—Anniversary Meeting.—John Evans, V.P., D.C.L., F.R.S., in the chair.—The Treasurer's report and the report of the Council were read and adopted.—The Chairman delivered an address, in which he briefly reviewed the work of the past year, and enlarged on the subject of the antiquity of man, discussing the evidence for and against his existence in Tertiary times.—The following Officers and Council for 1883 were elected:—President, Prof. W. H. Flower, F.R.S. Vice-presidents: Hyde Clarke, John Evans, F.R.S., Francis Galton, F.R.S., Major-Gen. Pitt-Rivers, F.R.S., A. Thomson, F.R.S., E. B. Tylor, F.R.S. Director, F. W. Rudler, F.G.S. Treasurer, F. G. H. Price, F.S.A. Council: J. Beddoe, F.R.S., S. E. B. Bouverie-Pusey, E. W. Brabrook, F.S.A., C. H. E. Carmichael, M.A., W. Boyd Dawkins, F.R.S., W. L. Distant, A. W. Franks, F.R.S., Lieut.-Col. H. H. Godwin-Austen, F.R.S., Prof. Huxley, F.R.S., A. H. Keane, B.A., A. L. Lewis, Sir J. Lubbock, M.P., R. Biddulph Martin, M.P., Henry Muirhead, M.D., J. E. Price, F.S.A., Lord Arthur Russell, M.P., Prof. G. D. Thane, Alfred Tylor, F.G.S., M. J. Walhouse, F.R.A.S., R. Worsley.

PARIS

Academy of Sciences, January 22.—M. Blanchard in the chair.—The following papers were read:—On metasulphites, by M. Berthelot.—On selenide of nitrogen, by MM. Berthelot and Vieille.—On the characters of induced currents resulting from reciprocal movements of two magnetic bodies parallel to their axis, by M. du Moncel. Polarisation of an iron core immobilises a certain quantity of magnetism, which thus remains indifferent to exterior magnetic excitation, and is only affected when, being able to act on the inducing body, which over-excites its energy, it may polarise it in its turn, so that action and reaction are in concordance.—On complex units (continued), by M. Kronecker.—Theory of the most general electro-dynamic actions that can be observed, by M. Le Cordier.—On the construction of a dynamo-electric propeller on a long balloon, by M. Tissandier. The system, with a total weight = three men, gives during three hours the work of twelve to fifteen men. The two-vaned propeller (of steel wire and varnished silk) is driven by a small Siemens' dynamo (120 turns of the former to 1200 of the latter); the battery being of thirty-four elements mounted in tension, and divided into four series. An element consists of a vulcanite box (four litres capacity) holding ten zinc and eleven carbon plates. Strong bichromate solution is let in or drawn off by raising or lowering a separate vessel connected by a tube with the battery.—Observations of the transit of Venus at Bragado (Argentine Republic), by M. Perrin. He observed two direct contacts (the second and the fourth), and a certain number of artificial contacts which will supplement the others. The phenomenon was of distinct and well characterised geometrical appearance.—On the approaching return of the periodic comet of d'Arrest, by M. Leveau. He has calculated an ephemerides (which will be communicated to all astronomers) for the period most favourable to observation, viz. April 23 to November 25 this year. Values for the relative brightness are deduced.—Addition to a note on prime numbers, by M. de Jonquières.—On the relations between covariants and invariants of like character, of a binary form of the sixth order, by M. Stephanos.—On the functions of several imaginary variables, by M. Combescure.—On the functions of two variables, by M. Poincaré.—On the curves of the sextant, by M. Gruey.—Mode of distribution among various points of its small supporting base, of the weight of a hard body, of polished and convex surface, placed on an elastic horizontal ground, by M. Boussinesq.—On a communication of MM. Mercadier and Vaschy on consequences deducible from relations between electric magnitudes, by M. Lévy.—Remarks on the expression of electric magnitudes &c. (continued), by MM. Mercadier and Vaschy.—Observations on Dr. Siemens' last paper, by M. Violle.—Photographic positives on paper obtained directly, by MM. Cros and Vorgerand. Paper is covered with a solution of 2 gr. bichromate of ammonia, 15 gr. glucose, and 100 gr. water, is dried, and exposed to light under a positive (*e.g.* a drawing). When the (yellow) bare parts of the paper have become grey, the paper is immersed in a bath of 1 gr. nitrate of silver to 100 gr. of water, with 10 gr. acetic acid. The image appears at once, with reddish tint, produced by bichromate of silver. Drying in light gives a dark brown tint.—On hydraulic silica, by M. Le Chatelier. The only new fact given by M. Landrin (he says) is the non-hydraulicity of silica obtained from manufacture of hydrofluosilicic acid.—On mutual displacements of bases in neutral salts, the systems remaining homogeneous, by M. Menschutkin.—On the causes capable of affecting the amount of ammonia in rain-water, by M. Houzeau. One important consideration is the time that has elapsed between obtaining and analysing; another the monthly quantity of water (the less the rain, the more ammonia present).—On the action of certain metals on oils, by M. Livache. Instead of metallic plates (which M. Chevreul experimented with), he used metals finely divided, as in precipitation, and got much better effects. Of the three, lead, copper, tin—lead acts most strongly. If some of it be moistened with oil and exposed to air, an increase of weight very soon occurs through oxidation, and it is greater the more siccative the oil. A solid and elastic product is formed. The increments of weight with different oils are sensibly proportional to those in fatty acids of the same oils exposed to air several months (cotton-seed oil alone is anomalous; it is siccative, but its fatty acids increase very little in weight). The transformation of the oil is attributed to direct action of the metal, not to that of the air. It suggests a rapid means of distinguishing siccative and non-siccative oils, and an advantageous substitute for the heating of oils.—Calcification of kidneys, parallel to the decalcification of the bones, in subacute poisoning by corrosive sublimate; increase of the proportion of mineral parts of a tibia, following disarticulation of the other tibia, by MM. Prevost and Frutiger.—Physiological action of sulphate of quinine on the circulatory apparatus in men and animals, by MM. Lée and Bochefontaine. It preserves and increases the force of the heart, and is a powerful antipyretic.—Medullary origin of paralyses following cerebral lesions, by M. Couty.—On the lymphatic system of tadpoles, by M. Jourdain.—On the development of the reproductive apparatus of pulmonate molluscs, by M. Rouzaud.—On Suctociliate Infusoria (a reply), by M. de Merejkowsky.—On the morphological nature of the subterranean branches of the root of adult *Psilotum*, by M. Bertrand.—Contribution to the stratigraphic history of the relief of Sinai, and especially on the age of porphyries of that country, by Abbé Raboisson. The last dislocations of the Sinaitic system were posterior to the eocene.

CONTENTS

PAGE
POPULAR ASTRONOMY 309
THE ZOOLOGICAL RECORD 310
OUR BOOK SHELF:—
  "The Brewer, Distiller and Wine Manufacturer" 311
  Serpieri's "Il Potentiale Elettrico nell' Insegnamento Elementare della Elettrostatica" 312
LETTERS TO THE EDITOR:—
  Hovering of Birds—THE DUKE OF ARGYLL; HENRY T. WHARTON 312
  Action of Light on India-rubber—Prof. HERBERT MCLEOD, F.R.S. 312
  A Possible Cause of the Extinction of the Horses of the Post-Tertiary—S. GARMAN 313
  Suicide of Scorpions—C. LLOYD MORGAN 313
  Mimicry in Moths.—Commander DUNCAN STEWART 314
  Clerk-Maxwell on Stress.—T. 314
  The Comet.—E. RISTORI 314
  The Aurora of November 17, 1882—THOS. WM. BACKHOUSE; W. M. F. PETRIE; Dr. HENRY MUIRHEAD 315
  The Sea Serpent—JOSEPH SIDEBOTHAM 315
  Influence of "Environment" upon Plants.—HOWARD FOX . . . 315
THE PEAK OF TENERIFFE ACTIVE AGAIN. By Prof. C. PIAZZI SMYTH 315
JOHANN BENEDICT LISTING 316
CLAUDE BERNARD 317
THE FINSBURY TECHNICAL COLLEGE 318
ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE, III. By ANDREW GRAY 319
NATURAL SCIENCE IN THE OPEN COMPETITIVE EXAMINATIONS FOR CLERKSHIPS (CLASS I.) IN THE CIVIL SERVICE 321
NOTES 322
OUR ASTRONOMICAL COLUMN:—
  Variable Stars 324
  Reported Discovery of a Comet 324
  The next Return of D'Arrest's Comet 324
  Meridian Observations of Nebulæ 324
PHYSICAL NOTES 324
AN INQUIRY INTO THE DEGREE OF SOLUBILITY REQUISITE IN MANURES, WITH SPECIAL REFERENCE TO PRECIPITATED CALCIC AND MAGNESIC PHOSPHATES 325
THE ELECTROLYTIC BALANCE OF CHEMICAL CORROSION. By G. GORE, LL.D., F.R.S. 326
THE ETHER AND ITS FUNCTIONS, II. By Prof. OLIVER LODGE . . 328
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 330
SCIENTIFIC SERIALS 330
SOCIETIES AND ACADEMIES 330

THURSDAY, FEBRUARY 8, 1883.

ZOOLOGICAL SKETCHES

Zoological Sketches. A Contribution to the Out-door Study of Natural History. By Felix L. Oswald. With Thirty-six Illustrations by Hermann Faber. (London: W. H. Allen and Co., 1883.)

Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals; with Anecdotes concerning and Adventures among them; and some Account of their Fossil Representatives. By Arthur Nicols, F.G.S., F.R.G.S. Illustrated by J. W. Wood and F. Babbage. (London: L. Upcott Gill, 1883.)

WE cannot say much in favour of the first of these two works. The engravings are good, but the subjects chosen scarcely justify the care which has been taken in their execution. For these subjects are nearly all chosen for the sake of a comical or sensational effect, without any reference to utility as illustrating zoological facts or principles. And the essentially unscientific spirit which has led to the choice of the "thirty-six illustrations," is no less apparent throughout the letter-press. We are always ready to welcome any attempt at popularising zoology, more especially when the writer has any first-hand "contributions to the study of natural history" to supply; but surely such study admits of being made sufficiently interesting in itself, without the need of lame attempts at a kind of pleasantry, which in being always forced and never witty, must necessarily become irksome even to the least intelligent of unintelligent readers. We are the more disposed to regret the author's mistake in adopting this artificial style, because in his short preface, where it is not adopted, he shows that he is able to write with marked ability.

Concerning the facts of natural history which are detailed, the most interesting, in our opinion, are those which refer to the intelligence of monkeys and the stupidity of sloth bears. We shall, therefore, give one quotation on each of these topics.

Speaking of a domesticated sloth, the author says:—

"Though fed daily by the same hands, the old pensioner still fails to identify his benefactor, or to recognise his obligations in any way. To his ear the human voice in its most endearing tones is a grunt *et prœterea nihil*; you might as well appeal to the affections of a cockroach. You may frighten a pig, a goose, a frog, and even a fly, but you cannot frighten or surprise a sloth. On my last trip to Vera Cruz I procured a pair of black tardos, full grown, and in a normal state of health, so far as I could judge, but after a series of careful experiments I have to conclude that their instinct of self-preservation cannot be acted upon through the medium of their optic or acoustic nerves. They can distinguish their favourite food at a distance of ten or twelve yards, and the female is not deaf, for she answers the call of her mate from an adjoining room; but the approach of a ferocious-looking dog leaves her as calm as the sudden descent of a meat-axe within an inch of her nose. The he-sloth witnessed the accidental conflagration of his 'straw couch' with the coolness of a veteran fireman. War-whoops do not affect his composure. I tried him with French-horn blasts and detonating powder, but he would not budge. One of my visitors exploded some pyrotechnic mixtures of wondrous colours and odours, but the tardo declined to marvel; he is a *nil-admirari* philosopher of the ultra-Horatian school."

Very different was the philosophical temper displayed by another of the author's pets. This was a young Siamese bonnet-macque monkey (*Macacus radiatus*), of which he says:—

"His conduct under circumstances to which no possible ancestral experiences could have furnished any precedent has often convinced me that his intelligence differs from the instinct of the most sagacious dog as essentially as from the routine knack of a cell-building insect. His predilection for a frugal diet equals that of his Buddhistic countrymen, and I have seen him overhaul a large medicine-chest in search of a little vial with tamarind jelly. He remembered the shape of the bottle, for he rejected all the larger and square ones, and after piling the round ones on the floor, began to hold them up against the light, and sub-divide them according to the fluid or pulverous condition of their contents. Having thus reduced the number of the doubtful receptacles to something like a dozen and a half, he proceeded to scrutinise these more closely, and finally selected four, which he managed to uncork by means of his teeth. Number three proved to be the bonanza bottle, and, waiving all precautions in the joy of his discovery, Prince Gautama left the medical miscellanies to their fate, and bolted into the next room to enjoy the fruits of his enterprise."

We have only observed one actual error in natural history, but as it is frequently repeated, we may point it out. The writer speaks of vampire bats as those which suck the blood of sleeping persons, whereas the truth is, as Belt has remarked, "the vampire is the most harmless of all bats."

The over-burdened title of the second of the above-named books serves to show its general character. The author is known from his previous works on "Chapters from the Physical History of the Earth," "The Puzzle of Life, and How it has been Put Together," &c. He is therefore already known as an ardent sportsman, a good naturalist, and an accurate observer; but in our opinion his latest work is his best. Indeed, we have seldom read a more successful and entertaining account of wanderings in which science has been combined with sport. Whether Mr. Nicols is writing about the snakes of India, the marsupials and monotremata of Australia, or the birds of South America, he manages equally to convey such vivid and interesting pictures of the animals, with their habits and surroundings, that while not a few of his first-hand observations are of importance to the scientific zoologist, nearly every page of his book is delightful and instructive to the general reader.

The plan of his work is a simple one. The first four chapters are devoted to Snakes, the second four to Marsupials, and the remaining eleven to Birds. In each case all the more interesting features of classification, anatomy, development, habits, distribution, intelligence, &c., are given with accuracy, and sandwiched between very readable descriptions of scenery, sporting adventure, &c. As an example of the latter, we may give one quotation, and for this purpose we choose one of the scenes in Australia:—

"On every side was desolation and salt-saturated earth, but here, in the midst of it, stately trees, luxuriant vegetation, and, above all, fresh water! This delightful site was selected by myself and my friend as a camping ground for a fortnight's holiday in the height of summer. Beyond a few pounds of biscuit and the usual allowance of tea and sugar, we had nothing, intending to live on whatever the gun and fishing-line brought to bag, and pick up as much natural history as possible. At a short distance from camp a creek entered the bay, noted for the abundance of wild fowl to be found upon it in the autumn and winter; but now all the birds were away breeding in the vast impenetrable swamps to the southward of the bay, except a few barren or unmated stragglers. . . . The sole result of the day's sport had been a pelican and a small shark, obtained in the first hour after sunrise, when alone it was possible to face the heat. Neither of us being acquainted with the method (if any exist) of rendering pelican a culinary delicacy, and having made the acquaintance of fried shark to such purpose that we would not willingly renew it, nothing remained but to watch for a chance duck. This duty being allotted to me, while my companion prepared the tea, I placed myself towards evening in hiding behind a mangrove stump, with the retriever beside me. For nearly an hour I endured the torture of a mosquito assault on face, hands, and legs, when suddenly a duck turned the seaward bend of the creek, and came skimming along the water to me. I stepped forward and shouted at him, with the usual result of making him hesitate in his flight, and rise well into the air, exposing the lower and most vulnerable side of his body to the charge, which in another instant laid him dead upon the water. The retriever dashed in to do his part of the work, and our supper might have been considered secure, had not a swift-winged cloud passed between the very nose of the dog and the bird, and with a splendid swoop a sea eagle bore off the duck, grasped by one strong foot. The sudden and unexpected action deprived me momentarily of all thought but admiration for the consummate ease and grace of a movement performed without any apparent effort, yet so expressive of immense power."

Among the novel or otherwise interesting observations in natural history which are recorded, we may note the following. The young kangaroo, while carried in the pouch of its mother, swallows the nipple, the end of which rests in the stomach; if forcibly withdrawn, the young animal makes no attempt to regain it, nor does it seem to have any idea where to seek for it. Again, speaking of Prof. Owen's theory concerning the object of the pouch as a "perambulator," necessitated by the long droughts in Australia to enable the mother to carry about her young in her search for water, Mr. Nicols observes that this view can only at best apply to the case of kangaroos, and therefore can scarcely be taken as the true *raison d'être* of the marsupium. Moreover, as a matter of fact, the "arboreal marsupials *do not* migrate in the severest drought, and in all probability they depend entirely upon the copious dew which falls at night upon leaves and grass."

The following observation on the king lory seems worth quoting:—

"In a few minutes he flew on to a tree well within range of the binocular, and shortly afterwards a female joined him in answer to his call. The swain was ardent, the damsel coy; they flitted from branch to branch, and whenever she perched he circled round her, threw himself underneath the branch, and swung to and fro with outspread wings, displaying the full glory of his scarlet breast. In every movement, whether on the wing or swaying at the end of a bough, he studied to present in the most effective manner the brilliant adornments of his plumage. . . . I do not think it possible for any one who had seen this little episode in bird life to have resisted the conclusion that the male was conscious of his beautiful breast, and that he adopted the best method of showing it by swinging himself beneath the branch, whence the female could look down and admire the display."

In the course of a discussion on the theory of flight Mr. Nicols has occasion to correct several errors of observation which have been made by previous writers. Thus, Captain F. W. Hutton has said that he has "sometimes watched narrowly one of these birds (albatross) sailing and wheeling about in all directions for more than an hour without seeing the slightest movement of the wings"; and the Duke of Argyll, in his "Reign of Law," repeats the statement, on the testimony, as he says in reply to a letter from Mr. Nicols on the subject, "of many writers and of some friends." But Mr. Nicols is very positive in his assertion that "the soaring does not continue for more than four minutes without a wholly new departure of from twenty to thirty powerful wing impulses, and it would seem an importation of the supernatural into Natural History to admit the possibility of sustaining the [soaring] flight for an hour." Mr. Nicols also questions the accuracy of another statement which was published by the Duke in this Journal concerning the habits of *Picus minor*. His Grace said that, having had an opportunity of closely observing these habits, he found that the cock bird drums upon the hollow parts of trees with his beak, in order to produce "instrumental music" wherewith to charm the sitting hen. On this it is remarked—

"The above is dated May, 1880. Although everything from the pen of so attentive an observer will be received with respect, it must obviously be very difficult to determine whether this is really a substitute for vocal music, or simply the ordinary beating of the tree to procure food."

We shall now conclude this brief selection from Mr. Nicols' observations, by quoting a somewhat remarkable one which he made in the Zoological Gardens of London.

"I saw a most extraordinary performance on the part of a flamingo towards a *cariama*. The two birds were in adjoining pens in the open air, separated by close wire fencing, over which the flamingo could easily reach. The cariama was constantly uttering its harsh metallic cry while standing opposite to the flamingo, both birds being as near the fence as they could stand. They remained gazing at each other thus for at least half an hour, each replying to the cry of the other. The demeanour was not in the least suggestive of anger, but what passion moved the flamingo I cannot imagine, unless it had some reference to the sexual instinct, and may indicate a habit unknown to naturalists. During the whole time that I watched them—and perhaps for long before and after—the flamingo continued to drop from its beak upon or in front of the cariama a bright scarlet fluid, which I cannot doubt was blood. The bird was certainly not wounded or hurt, and the action, from whatever impulse it proceeded, was voluntary, and appeared to afford gratification to the performer and to the recipient of this singular attention. In reply to a note on the subject, which I at once sent to Mr. Darwin, he expressed his surprise at the peculiar behaviour of the flamingo, which had not previously been brought under his notice, but did not see his way to an explanation of it. The keeper told me he had witnessed the performance once before, and assured me that both birds were in perfect health, and on most friendly terms with each other."

From the care with which Mr. Nicols has made most of his other observations, we should have expected him in this case to have procured some of this scarlet fluid for examination, and also to have obtained permission to place both birds in the same pen; but we have said enough to show that his book seldom lays itself open to criticism of this kind, and we sincerely hope that it will, in the words of its concluding sentence, "find favour with the public," not only because it well deserves to do so, but also because the author promises that if it does, other Series of similar Zoological Notes will be issued by him from time to time. Lastly, we must not take leave of this, the First Series, without noticing the life-like drawings of the illustrations—particularly that of the cobra—and also the novel and effective character of the binding, the boards being covered with thin but continuous plates or shavings of wood, which, without at all adding to their stiffness, give so strikingly pretty a finish that we should like to see this new idea in book-binding largely adopted by other publishers.

GEORGE J. ROMANES

THE GOLD COAST

To the Gold Coast for Gold. By Richard F. Burton and Verney Lovett Cameron. Two Vols. Maps and Illustrations. (London: Chatto and Windus, 1883.)

THE exploration undertaken by Captains Burton and Cameron was for the purpose of examining and reporting upon the condition and prospects of certain gold-mining properties in Western Africa, for which early in 1882 upwards of seventy distinct concessions had already been obtained from various native owners. Five only out of these concessions are reported on as having upon them mines actually in operation, while it is more than probable that of the remaining sixty-five a large proportion might be acquired by the British public for a valuable consideration.

It had originally been intended to explore the so-called Kong Mountains, relative to which no additional information has for many years been acquired; but so much work had to be accomplished on the Ancobra river that the latter portion of the programme had of necessity to be relinquished; under this disappointment Capt. Burton philosophically consoles himself with the reflection that "Geography is good but Gold is better.'

Capt. Burton left Trieste on November 18, 1881, for Gibraltar, and thence proceeded to Madeira, about which he very pleasantly gossips through some sixty pages, and where he is joined, on January 8, 1882, by his travelling companion, Capt. Cameron. From Madeira they proceed together to Teneriffe, *à propos* of which place we are given the translation of a Spanish account of the repulse of Lord Nelson from Santa Cruz de Teneriffe, which extends through at least another fifty pages.

From Teneriffe the travellers continued their voyage, touching at Bathurst, Sierra Leone, Cape Palmas, &c., eventually landing at Axim on January 24, 1882. Of Axim Capt. Burton says:—

> "There is no better landing-place than Axim upon this part of the African coast. The surf renders it impracticable only on the few days of the worst weather. We hugged the north of the Bobowusúa rock-islet. When the water here breaks there is a clear way further north; the southern passage, paved with rocks and shoals, can be used only when the seas are at their smoothest. A regular and well-defined channel placed us on the shingly and sandy beach. We had a succulent breakfast with Messrs. Gillet and Selby (Linott and Spink), to whose unceasing kindness and hospitality we afterwards ran heavily in debt. There we bade adieu to our genial captain and our jovial fellow-travellers."

From Axim the travellers visited a stretch of country in the vicinity of the coast, extending some distance west of that town, and Prince's River to the east of it; and then ascended the Ancobra as far as Tumentu, situated about twenty-two miles from its mouth. After inspecting this last locality Capt. Burton returned to Madeira, leaving Capt. Cameron to continue his surveys inland as far as Crockerville, a mining settlement about forty-five miles north-east of Axim. Capt. Cameron left the African coast on March 28, 1882, and having picked up Capt. Burton at Madeira they proceeded together to Liverpool.

Rich alluvial deposits as well as auriferous quartz reefs are described as occurring abundantly, but the hydraulicing of the former is strongly recommended in preference to the mining and crushing of the latter. This opinion is so constantly reiterated that the explorers would appear to believe that a large portion of the "Dark Continent" would pay for washing. Water is stated to be plentiful, and facilities for hydraulic mining are represented as being very general.

> "Wherever *catas* or 'women's washings' are found we can profitably apply the hydraulic system of sluicing and fluming, not by an upper reservoir only, but also from below, by a force pump. Water is procurable at all seasons by means of Norton's Abyssinian tubes, and the brook-beds dammed above and below will form perennial tanks."

In California as much as fifteen hundred and seventy cubic feet, or forty-three tons of water, under a pressure of four hundred feet, are sometimes discharged per minute from a single nozzle; it is therefore evident that any supply to be obtained from Abyssinian tubes, and pumped against an auriferous bank, would, in comparison with such a power as that described, possess only the force of an ordinary garden squirt.

About five-sixths of the letterpress are from the facile pen of Capt. Burton, the remainder being contributed by his companion, who appears to have done good work by determining the position of various places, of which the exact situation was previously unknown. Collections of plants and birds were also made.

Although somewhat diffuse, these volumes contain much information very pleasantly conveyed, but even in the case of gold-mining, caution is often to be recommended.

OUR BOOK SHELF

A Handbook of Vertebrate Dissection. By H. Newell Martin, D.Sc., M.D., M.A., and William A. Moule, M.D. Part I. How to Dissect a Chelonian. (New York: Macmillan and Co., 1881.)

SINCE the publication of Huxley and Martin's "Elementary Biology," there has been a growing want of more books constructed on the same model. Without such books it is exceedingly difficult for a student to dissect and thoroughly understand the anatomy of an animal which he has never examined before; and though many of the large teaching centres have their own laboratory directions, which, with the help of demonstrators, fulfil all requirements, there are few reliable practical books which can be bought by the private student.

Drs. Martin and Moule have lately brought out a pamphlet in which full directions are given "how to dissect a Chelonian," and we are pleased to see from the preface that they intend to follow it by a series of similar works. It is almost a pity that so exceedingly specialised a reptile as a Chelonian should be the first they treat, especially as they intend to include a lizard in the series, for it is doubtless far better to begin the study of reptiles with the latter than with the former.

The species on which the work is based is *Pseudemys rugosa*, but, as Dr. Martin states, "the end in view is not to provide a monograph on any one species, but to show a student 'how to dissect a Chelonian,'" the fact that, when dissecting another species, the description in the book cannot altogether be relied upon, makes the student examine everything carefully for himself.

Without working through the anatomy of a Chelonian with the help of the book it is impossible fully to appreciate its value, but from the arrangement, accuracy, and clearness of description, it will doubtless prove a great boon to the young herpetologist.

The only fault we can find with it is one under which the "Elementary Biology" equally suffers; and that is the want of illustrations. There is a frontispiece with four rather rough woodcuts of the skull (to which no reference letters are given in the text), but this is all.

Of late several illustrated students' biological books, intended as guides for practical work, have been brought out, but most of these are so inaccurate as to be practically useless. It is therefore to be regretted that a book by so eminent a teacher as Dr. Martin should be so poorly provided with figures, and we hope that the rest of the series will be more fully illustrated, as their value would be thereby greatly increased.

Ferns of Kentucky. By John Williamson. (Louisville, Kentucky: J. P. Morton and Co., 1878).

ALTHOUGH this little volume has only just reached us, it cannot be said to be out of date; for the number of popular works on ferns—those published in England excluded—is so small, that an addition to their number is at any time welcome to the fern-lover who has become well acquainted with the common British species, and would gladly increase his knowledge of the tribe. There is, indeed, no parallel in French or German literature to the number of fern-books which have been issued in England; but it would appear from the volume before us that in America fern-hunting is as popular as it is among ourselves; for Mr. Williamson asks in his preface "Who would think now of going to the country to spend a few days, or even one day, without first inquiring whether ferns are to be found in the locality?"

Mr. Williamson has given descriptions of the species found in Kentucky, and the letterpress is accompanied by sixty plates, in which the characters of the genera and the habit of each species is represented. The descriptions, although short and couched in simple language, seem carefully done; and the absence of pretence about the work does not render it the less attractive. Although, of course, primarily intended for local use, the "Ferns of Kentucky" contains much to interest the British lover of ferns.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Hovering of Birds

I AM much obliged to his Grace the Duke of Argyll and to Mr. H. T. Wharton for the notice they have taken of my letter on the hovering of birds. It may be hoped that the question will not be allowed to rest until it has received its quietus at the hands of some mathematician who shall tell us with authority whether it is possible, according to the received laws of mechanics, for a rigid body, by any disposition of its surfaces, to remain motionless (relative to the earth) for a minute in mid-air in a perfectly horizontal wind.

It is disappointing to find that the 3rd chapter of the "Reign of Law," to which the Duke refers me, contains no satisfactory explanation of the phenomenon of motionless hovering. We read there (p. 161, 5th edition, 1868). "When there is a strong breeze, no flapping is required at all, the force of the wind supplying the whole force necessary to counteract the force of gravity." This is hardly a sufficient explanation. Let us imagine a bird at rest in a horizontal wind. Neglecting friction, the only forces acting on the bird are (1) the vertical force of gravity, and (2) the resultant of the air-pressures on the different surfaces of the bird caused by the horizontal velocity of the wind. These pressures may be resolved parallel and perpendicular to the general plane of the wings, and the direction of their resultant will vary with the slope of the wing-plane; but in every case it will lie to rearward of the vertical, and therefore in every case there will be a resolved horizontal force pushing the bird backwards. Yet in spite of this force the bird is to remain at rest! Shade of Archimedes!

Consider, again, what must be the necessary corollary to the Duke's proposition. If a bird can remain at rest in a horizontal wind, it necessarily follows that in still air a bird can float horizontally without losing velocity. We do indeed see rooks and other birds float long distances descending on outspread wings in still air, and it is marvellous how slow is the descent, so great is the resistance of air to a plane surface when at every successive instant the plane surface covers a fresh body of air that has not yet begun to yield. But no one ever saw a bird maintain a horizontal floating flight in still air. Either the descent is continuous, or the bird loses velocity.

It might be wished, in a matter of such importance, involving as it does nothing less than the establishment of a miracle, that the Duke of Argyll were more precise in his statement, for I feel curious to know by what resolution of forces he would demonstrate the existence of a forward horizontal force to balance the backward force which he cannot deny to be present. A diagram setting forth the Duke's views would be exceedingly welcome. My own diagram, I fear, will not serve his purpose any better than my words, in spite of the attempt he makes to press them into his service. The Duke says: "The bird has only to slope his wing-surfaces to the [horizontal] current, and precisely the same effect is produced as if the current had been otherwise sloped upwards against a horizontal wing-surface." Perfectly true, provided his Grace can tilt the direction of gravity through the same angle. Otherwise not (at the same time there is nothing about a "horizontal wing surface" in my letter).

Mr. Wharton will do good service if, in recording any future observations he will note precisely the local circumstances under which they are made, bearing in mind that such an obstacle as a stack, a barn, a high hedge, or a thick tree might be enough to give the wind an upward throw. I presume he understands that the phenomenon to be explained is that of a bird remaining at rest in mid-air, with wings motionless, not fluttering.

Woodbridge, February 5 HUBERT AIRY

THIS is a very interesting problem, and one which has been very clearly treated by Mr. Hubert Airy, so far as he discusses it.

I think, however, that birds are able to "hover" in other conditions than that he mentions, namely, where the wind is diverted upwards by blowing straight against cliffs or rising ground.

The wind in this country at least is generally of a cyclonic character. Now such wind blows in toward the centre of depression, as has been shown by the Rev. W. Clement Ley, at an average angle of 25 degrees (inward from the tangent to the isobar). If we assume the wind to blow at a distance of 600 miles from such centre at a rate of 20 miles per hour—on an average for one mile in depth—then we have a volume of air converging upon the centre, which must rise into the upper regions there. If we assume, for the sake of obtaining a definite idea of this amount, that the centre of depression covers an area of 70 miles in diameter, say about 4000 square miles, then the air would rise vertically at an average rate of 400 feet per minute over the area. Owing to the friction which the wind experiences in passing over the surface of the earth, however, this upward current could exist at its maximum only at a considerable height. But it is important to observe that there may frequently be a considerable amount of current upwards in the regions where birds "hover," at least in the neighbourhood of a cyclonic vortex. But how much lifting force is necessary to sustain, say a gull, in the air? A gull moves its wings in ordinary flight at from 160 to 200 (double) strokes per minute, and reckoning 12 inches as the greatest vertical depth through which the bird can raise itself by one (double) stroke, we find that it possesses the power of raising itself about 180 vertical feet per minute. This, however, is less than one-half of the rate at which we have found the currents to rise near the centre of a cyclonic depression. From this we may judge it is likely enough that birds "hover," or suspend themselves motionless in the air through the influence of upgoing currents, which are masked to our observation by the fresh winds which accompany them.

A kestrel may, however, support itself largely by its peculiar quivering play of the wings, and I think it must be difficult to determine how much support a bird may contribute by such motion, when at a height where it is difficult to observe it.

I have frequently observed gulls "hovering" upon currents of air which were heaped up by the wind striking obliquely upon a rising coast line, in which case the head is turned at an angle to the general direction of the wind, so as to face the heaped-up and rising currents. Such passing over irregular ground are irregular or gusty, and tax the bird's utmost muscular agility to prevent a sudden lateral turning to leeward, in which case the rapid flight with the currents may be compared to the fall of a stone to the ground. The same upward direction to the atmospheric currents must be imparted by the contracting sides of a converging valley. But when such local forces derive aid from the upward currents peculiar to cyclonic winds, atmospheric conditions favourable to "hovering" must, I think, frequently occur.

On the other hand, I cannot conceive it to be possible for birds (and I do not think that the third chapter of the "Reign of Law," gives any sufficient explanation) to sustain themselves motionless on currents of air which are purely horizontal, for in such case there is nothing to compensate—when the wings are slanted at the necessary angle to prevent falling—the backward horizontal force, and the creature must inevitably be driven backwards. DAVID CUNNINGHAM

Dundee, February 5

ON August 12, 1881, I observed a hawk maintaining an apparently stationary position at a height of about 200 feet above the surface of flat ground. He was *as a matter of course* facing the wind, which blew, if I remember rightly, from the west. For the most part his wings remained motionless, but now and then he fluttered them for a little while. This was over the sensibly level plain which lies between Machrihanish Bay and Campbeltown Loch, at the southern end of the Mull of Cantire, and, curiously enough, on or close to the Duke of Argyll's property. The exact spot was about a mile and a half eastwards from Machrihanish Bay, and about three-quarters of a mile northwards from the southern boundary of the plain. There could not be any "slant upward current," such as Mr. Airy supposes, maintaining him in that position; at any rate, there was no sloping ground near.

I watched this bird for about ten minutes, and he verified in a remarkable manner the views I had held on this subject for many years, namely, that, given a steady wind blowing with a velocity which lies somewhere between certain possibly calculable limits, a hawk can remain for a time apparently motionless above a point; he is, in reality, descending a slightly inclined plane, and requires to recover vertically lost ground by the occasional use of his wings. WILLIAM GALLOWAY

Cardiff, January 30

IN the letters on the above subject that have appeared in some recent numbers of NATURE, the writers lead us to believe that a current of air is *necessary* to enable a bird to "hover" or retain, when on wing a motionless position. My observations lead to an opposite conclusion, as I have often seen both hawks and terns remain steadily poised, when there was not a breath of wind. That there was no wind where the birds (*terns*) were, was shown by their heads, when hovering, being turned in different directions, although at only a short distance from each other. Generally, if not always on these occasions, I noticed that the birds spread out their tails in a more or less depressed position, as if to counteract any forward movement likely to be caused by the wing-motion. J. RAE

4, Addison Gardens, February 3

IN reading the letters published in your last issues of NATURE with regard to the hovering of birds, it struck me that a very similar thing can be seen sometimes, among inanimate objects when an imperfect attempt is made to cause "ducks and drakes" with a flat stone. I have commonly noticed that the missile curves sharply upwards and for a moment "hovers" as it were in mid-air before dropping. In this case and also in the similar one of the motion of the boomerang, the slanting upwards and the apparent hovering do not require, and need not be due to, upward currents, but merely depend upon the force of a horizontal current of air meeting the inherent force of the moving body. It is not unreasonable to suppose a similar simple solution of bird hovering. C. S. MIDDLEMISS

Linnæus Street, Hull, February 3

IS there not an error in the letter of NATURE (p. 312)? The writer there suggests, as it seems to me, that a bird could maintain a position of rest, with respect to the earth, by a suitable slope of the wings against a horizontal wind. Now, as I pointed out in NATURE, vol. xxiii. p. 78, such lifting action on the part of the wind can only take place in the interval between the time when the bird is first launched from the cliff, and the time when it has by friction attained the velocity of the wind. That this interval is not a long one, is shown when balloons or other objects are launched.

[It may be well to notice, that if there were *no* friction there is no lifting power; so that if we object to the above, that "the bird gives such a *very* small friction with the wind," we thereby do away to the same extent with the lifting power; just as a frictionless ship in a constant stream would be unmoved were it sufficiently tapering.]

From the above considerations I have been compelled, since writing my last letter, to ascribe the hovering power of birds—

1. To the "exquisite muscular sense" by which they can take advantage of all upward currents of air, shifting their positions for this purpose. In an elastic fluid as the air, I imagine that the stream-lines, even over the sea, are far from horizontal. I believe the evidence of balloons over the sea goes to show this.

2. There is, to use a common expression, "flying *and* flying," just as a man can skate without striking out, so can a bird give itself some support by quiet movements of wings and tail.

I may remark that kestrels keep fluttering their wings at short intervals while hovering; they are never still for long. So also terns and gulls, as seen from the fixed point of a cliff, are always moving and shifting in a quiet way, which may disguise both a seeking of upward currents and the quiet sort of "flying." W. LARDEN

Cheltenham College

Science and Theology

CAN you tell me by what right the authorities of Cooper's Hill Engineering College, who are in want of a Professor of Physics, make it a condition that he should "be a Protestant," and should "attend morning chapel and Sunday services with reasonable regularity, showing in this respect a good example to the students?" The institution is one supported by the State, and is surely bound to respect the principles which underlie the State's dealings with religious matters. The president (or whoever is responsible for these preposterous conditions) may have forgotten this fact; but I cannot believe that the present Government will allow an appointment to be made until all "religious" limitations are cancelled from its conditions. As the memorandum stands at present, it appears little short of insulting to scientific men. C.

Intelligence in Animals

MR. GRENFELL'S letter in NATURE, vol. xxvii. p. 292, reminded me of a statement in vol. iii. p. 308 of Cook's last voyage, where Capt. King refers to the ordinary sagacity of bears, described in a "thousand stories" which he heard in

Kamtschatka. He gives a single instance, which, he says, "the natives speak of as a well-known fact, and that is, the stratagem they (the bears) have recourse to in order to catch the bareins, which are considerably too swift of foot for them. These animals keep together in large herds, and love to browse at the feet of rocks and precipices. The bear hunts them by scent till he comes in sight, when he advances warily, keeping above them, and concealing himself among the rocks as he makes his approaches, till he gets immediately over them, and nigh enough for his purpose. He then begins to push down with his paws pieces of rock among the herd below. This manœuvre is not followed by any attempt to pursue, until he has maimed one of the flock, upon which a course immediately ensues, that proves successful, or otherwise, according to the hurt the barein receives. I cannot conclude this digression without observing that the Kamtschadales very thankfully acknowledge their obligations to the bears for what little advancement they have hitherto made either in the sciences or polite arts. They confess that they owe to them all their skill in physic and surgery; that by remarking with what herbs these animals rub the wounds they have received, and what they have recourse to when sick and languid, they have become acquainted with most of the simples in use among them, either in the way of internal medicine or external application."

After this we are not surprised when we are told that the Kamtschadales receive instruction from the bears even in the "polite arts," and imitate in their dances the various attitudes and gestures of these animals. It seems that in the *rôles* of master and pupil the proverbial Savoyard and dancing bear would find matters reversed in Kamtschatka.

Millbrook, Tuam, February 3 J. BIRMINGHAM

Electric Railways

PROF. AYRTON speaks as to the advantages obtainable from an electric system of railways. He says:—"The mass of the locomotive adds at least 50 per cent. to the horse-power absolutely necessary to propel the carriages along" (NATURE, vol. xxvii. p. 255). In short, he speaks of the weight of the ordinary locomotive as superfluous, and considers that "a far larger number of passengers may travel at a greater speed and with less fear of danger than at present." Now, speaking practically, it is difficult to conceive of a train of carriages running sixty miles per hour without any massive locomotive in front. It would be easy enough to get up the requisite speed, but the train would certainly leave the road, there being nothing tending to keep the carriages steady, unless they were very heavy. The grip on the rails is directly as the weight of rolling stock, and it is generally found that the light coaches leave the road more readily than the modern heavy carriages. Of course the cant of the rails must not be neglected. I wish that Prof. Ayrton would favour NATURE with a few remarks on these points. V.

The Channel Tunnel

WILL you allow me to correct an unfortunate slip of the pen in my article on "The Silver Streak and the Channel Tunnel" in the current number of the *Contemporary Review?* The rate of the progress of the French Channel Tunnel from the little village of Sangatte towards the English shores was, in November last, 18 yards per day and not "per week." At the present time the Beaumont and English boring machine is cutting the 7-foot driftway at the rate of more than 20 metres per day, and has not arrived at the limit of its capacity. W. BOYD DAWKINS

Owens College, Manchester

The Great Comet of 1882

THE comet not having been visible to my naked eye during the last lunation, I was astonished to find last night that (doubtless owing to its increasing altitude, and the clear, dark sky) its tail is still so visible, quite distinctly, though very faint. I saw it best with a pair of field-glasses, aperture 2·05 inches, power 4; with which it reached to ν^2 Canis Majoris, and was therefore 5½° long; unless part was really a wisp of the Milky Way, undoubtedly the greater part was the comet. Its axis (which was nearer the north than the south edge) was straight for 3½°, and then appeared to curve southwards somewhat. Its south edge was straight, but its north edge, which was more definite, was convex. Its width was nearly 2°. I could not detect any of the definite features which were so remarkable formerly.

The tail was nearly as long with the naked eye. Its head and two neighbouring stars were plainly visible to the naked eye as one star. One of these stars (Lalande 12,056) was decidedly brighter than the comet's head, which would be about of the 7th mag.

With a 4½-inch refractor the head continues elongated. With a power of 20, its major axis (which was in the direction of the tail) was 16′ long, and its minor axis 11′. With a power of 38 it was 13′ by 8½′. THOS. WM. BACKHOUSE

Sunderland, January 31

Meteor of November 17

IT is perhaps rather late to revert to the auroral cloud of November 17, but I am away from home, and have only now gained the requisite information. The path which I ventured to assign for it in your issue of November 30, from a digest of the printed reports, as compared with my own observations at Clevedon, proves to have been substantially correct. The cloud passed in the zenith at Brussels, as witnessed by M. Montigny, an eminent Belgian savant; and at Laon it was seen to the northward, as it were, gliding round the upper edge of the great main arch of the aurora. The actual elevation above the surface of the earth may therefore, without much risk of error, be considered as between forty and forty-five miles.

Montreux, February 3 STEPHEN H. SAXBY

The Sea Serpent

LIKE your correspondent, Mr. Sidebotham (in NATURE, vol. xxvii. p. 315), I have frequently seen a shoal of porpoises in Llandudno Bay, as well as in other places, and on the occasion referred to by Mr. Mott, in NATURE, vol. xxvii. p. 293, the idea of porpoises was at first started but immediately abandoned. I will venture to suggest that no one has seen a shoal of these creatures travel at the rate of from twenty-five to thirty miles an hour. I have seen whales in the ocean, and large flocks of sea-birds, such as those of the eider duck, skimming its surface; but the strange appearance seen at Llandudno on September 3 was not to be accounted for by porpoises, whales, birds, or breakers, an opinion which was shared by all present.

WILLIAM BARFOOT

Welford Place, Leicester, February 2

IN the summer of 1881 I was staying for some weeks at Veulettes, on the coast of Normandy. While there, on several occasions, several members of my party, as well as myself, saw, at a distance of three or four miles out at sea, what had the appearance of a huge serpent. Its length was many times that of the largest steamer that ever passed, and its velocity equally exceeded that of the swiftest. What seemed its head was lifted and lowered, and sometimes appeared to show signs of an open mouth. The general appearance of the monster was almost exactly similar to that of the figure in your correspondent's letter published on the 25th ult. Not the slightest appearance of discontinuity in its structure could be perceived by the eye, although it seemed incredible that any muscular mechanism could really drive such an enormous mass through the water with such a prodigious velocity. I carefully watched all that any of us caught sight of, and one day, just as one of these serpent forms was nearly opposite our hotel, it instantaneously turned through a right angle, but instead of going forward in the new direction of its length, proceeded with the same velocity broadside forward. With the same movement it resolved itself into a flock of birds.

We often saw the sea-serpent again without this resolution being effected, and, knowing what it was, could with difficulty still perceive that it was not a continuous body; thus having a new illustration of Herschel's remark, that it is easier to see what has been once discovered than to discover what is unknown. Possibly this experience may afford the solution of your correspondent's difficulty. W. STEADMAN ALDIS

College of Physical Science, Newcastle-upon-Tyne, Feb. 3

Natural Enemies of Butterflies

IT would be very interesting to ascertain what testimony can be brought forward to show that the Rhopalocera are commonly the prey of insect-eating birds. The return of a gentleman who has been collecting butterflies and studying their transformations for the last five years in Brazil, and who by my request has given especial attention to the matter, is the immediate occasion of my inquiry. Protective mimicry is a fact too well established to admit of its supporters feeling the question a delicate one. Admission into your paper will speedily settle the question.

Liverpool, February 2 HENRY H. HIGGINS

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE[1]

IV.

WE shall now consider, very briefly, the graduation of instruments for measuring volts and amperes in practical work, and we shall take as our example Sir William Thomson's graded galvanometers. The graduation of these instruments is effected by a comparison of their indications with those of a standard galvanometer such as that described above. We shall consider first the graduation of a potential galvanometer, or galvanometer the resistance of which is so high, that the attachment of its terminals to two points in a conductor carrying a current does not perceptibly change the difference of potentials formerly existing between these points. Of course any galvanometer which measures currents also measures potentials, for, if its resistance is known, the difference of potentials between its terminals can be calculated from Ohm's law; but the convenience of a galvanometer specially made with a high resistance coil is that the difference of potentials, thus calculated as existing between the two points at which its terminals are applied while they are in contact, may be taken as the actual difference of potentials which exists between those two points when nothing but the ordinary conductor connects them. For, let V be this actual difference of potentials in volts, let r ohms be the resistance of the conductor, and R ohms the resistance of the galvanometer. Then by the application of R, V is diminished in the ratio of R to $R + r$, and therefore the difference of potentials between the ends of the coil is now $V\frac{R}{R+r}$. Hence by Ohm's law we have for the current through the galvanometer the value $\frac{V}{R}\frac{R}{R+r}$, or $\frac{V}{R(1+\frac{r}{R})}$. If r be only a small fraction of R, $\frac{r}{R}$ is inappreciable, and the difference of potentials calculated from the equation $C = \frac{V}{R}$ will be nearly enough the true value.

The instrument to be graduated is first tested as to the adjustment of its coil, needle, &c. The standard galvanometer and it are then properly set up with their needles pointing to zero, in positions near which there is no iron, and at which the values of H have been determined. The high resistance coil of the standard galvanometer and the coil of the potential instrument are joined in series with a constant battery of as many Daniell's cells as gives a deflection of about 45° on the standard galvanometer, and the magnetometer is adjusted with its index at zero, in such a position on its platform that a deflection of its needle also of nearly 45° is produced. The current actually flowing in the circuit is calculated by equation (11) or (12) from the reading obtained on the standard, and reduced to amperes by multiplying the result by 10. The difference of potentials between the two ends of the coil of the potential galvanometer is found in volts by multiplying the number of amperes, thus found by the resistance of the coil in ohms. We can then obtain, by an obvious calculation, the number of divisions of deflection which corresponds to one volt between the two ends of the coil, and thence from the value of H the number of divisions which would correspond to one volt if the intensity of the field were one c.g.s. unit. This would be the number which would be marked at that position on the platform of the instrument; but, except for the position of the magnetometer nearest to the coil, positions the corresponding numbers of which are multiples and submultiples of 2 are alone marked. The numbers corresponding to the two of these positions adjacent on the two sides of the position of the magnetometer in our experiment, may be readily found by keeping the same difference of potential on the coil, and moving the magnetometer nearer to or further from the coil until the deflection is increased or diminished in the proper ratio. For example, let the deflection be 40 divisions for 20 volts, and let the value of H at the instrument which is being graduated be ·17. The number of divisions of deflection which would correspond to one volt for that position, if the field were of unit intensity, would be $\frac{40}{20} \times \cdot 17 = \cdot 34$. Hence the marked positions nearest to this in the two sides of it, are to be those for which the corresponding numbers are ½ and ¼. Therefore the magnetometer must be moved further from the coil for the latter until the deflection produced by 20 volts becomes 29·4. This is the position at which the number ¼ is to be marked. To find the position at which ½ should be marked a smaller difference of potentials must be used, as the deflection with the same battery as before would be beyond the limits of the scale. Suppose that when its number of cells is diminished to one half; we get a deflection for our first position of 20. While the potential difference in the coil remains constant, the magnetometer is pushed in until the deflection again becomes 29·4. At this position the number ½ is to be marked. From this it is easy to see how the position for the number 1 can be found, and in the same way those for the other numbers of the series, 2, 4, &c. The number corresponding to the position of the magnetometer nearest to the coil, although not one of the terms of this series, is determined in the same way, and marked at that position on the platform. This is the method adopted in practice in the graduation of these instruments.

Another method sometimes convenient is as follows. The standard instrument, a few good Daniell's cells, and a resistance which gives a deflection of about 45° on the standard are joined in series, and the galvanometer to be graduated is applied at two points in the circuit which include between them such a portion of the resistance as gives a deflection of about the same amount. Let R ohms be the portion of the resistance included between the terminals of the galvanometer, and let G ohms be the resistance of the galvanometer coil. Let the current calculated from the deflection on the standard be C amperes, then if V be the potential difference in volts between the terminals of the potential instrument, we have by Ohm's law—

$$C = \frac{V}{R'},$$

where R' is the resistance equivalent to the divided circuit of R and G. But $R' = \frac{RG}{R+G}$, and therefore

$$C = V\frac{R+G}{RG}.$$

Hence,

$$V = C\frac{RG}{R+G}.$$

This last equation gives the number of volts indicated by the deflection on the potential instrument, for the position at which its magnetometer is placed; and from this in precisely the same manner as described above, the series of positions of the magnetometer on its platform are determined and numbered.

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[1] Continued from p. 3[illegible]1.

For verifying the accuracy of the graduation of the potential instruments when performed by either of these methods, a standard Daniell's cell of the form proposed by Sir William Thomson at the Southampton meeting of the British Association is used. It is represented in the annexed cut (Fig. 4). It consists of a zinc plate at the bottom of the vessel resting in a stratum of saturated zinc sulphate, on which has been poured, so gently as to give a clear surface of separation, a stratum of half saturated sulphate of copper solution, in which is immersed a horizontal plate of copper. The copper-sulphate solution is introduced by means of the glass tube shown in the diagram dipping down into the liquid, and terminating in a fine point, which is bent into a horizontal direction so as to deliver the liquid with as little disturbance as possible. This tube is connected by a piece of indiarubber tubing with a funnel, by the raising or lowering of which the sulphate of copper can be run into or run out of the cell. By this means the sulphate of copper is run in when the cell is to be used, and at once removed when the cell is no longer wanted.

The electro-motive force of this cell has been determined very carefully and found, according to Lord Rayleigh's latest determination of the ohm, to be 1·07 volt at ordinary temperatures. The direct application of this cell to the galvanometer gives at once a check on the graduation. As the resistance of the galvanometer is always over 6,000 ohms, there is practically no polarization.

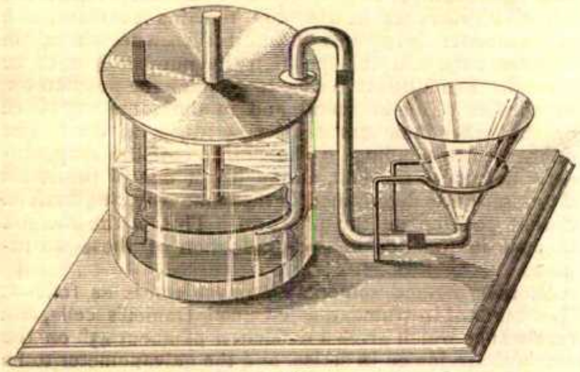

Fig. 4.

The method adopted for the graduation of the current galvanometer is precisely the same as that first described for the potential instrument. The standard galvanometer, of which in this case the low resistance coil is used, and the current galvanometer to be graduated are joined in series with a battery, which with some resistance in circuit is sufficient to give a deflection in each of about 45°, when the magnetometer of the current instrument is at a convenient position on its platform. The current flowing in amperes is given by the standard, and this, of course, is the number of amperes which is indicated by the deflection of the current instrument. By an obvious calculation from the value of H at the current instrument, precisely similar to that above described, the number of divisions of deflection corresponding to a current of one ampere for a field of unit strength is found, and from this the series of positions on the platform and their numbers are found.

The value of the field intensity given at the needles of the magnetometer when in position has generally been determined in the following manner. A battery of about 30 of Sir William Thomson's Tray Daniell's is joined in series with a resistance of about 7,000 ohms. The electrodes of a potential galvanometer, on which the magnetometer is placed without its magnet, are attached at two such points in this resistance that the deflection of the needle produced is from 30 to 40 division on the scale. The current through the galvanometer is now stopped and the magnet placed in position, and the index brought to zero by turning the magnet by means of its screw. The electrodes are now placed so as to include a resistance which makes the deflection nearly what it was in the former case. Let E be the electromotive force of the battery, I the intensity of the horizontal component of the field produced at the needles by the magnet; R_1 R_2 the amount of this resistance included between the electrodes of the galvanometer in the first and second cases respectively; V_1 and V_2 the potential difference in volts on the instrument in the same two cases; D_1 and D_2 the corresponding deflections, and G the resistance of the galvanometer. We have by Ohm's law

$$V_1 = \frac{E R_1 G}{(B + R - R_1)(R_1 + G) + R_1 G} = m H D_1$$

and

$$V_2 = \frac{E R_2 G}{(B + R - R_2)(R_2 + G) + R_2 G} = m I D_2$$

where m is a constant. Therefore we have

$$I = H \frac{D_1 R_2 \{(B + R - R_1)(R_1 + G) + R_1 G\}}{D_2 R_1 \{(B + R - R_2)(R_2 + G) + R_2 G\}}$$

If the resistance B of the battery be small in comparison with G, or if the galvanometer is sensitive enough to allow $\frac{B}{G}$ to be made sufficiently small by resistance added to G, B may be neglected; and it is generally possible, by properly choosing R, R_1, R_2, to simplify very much this formula. The number I thus found, diminished by H, is the number of c.g.s. units which measures the horizontal intensity of the magnetic field produced at the needle by the action of the magnet alone. The value of $I - H$ is the number painted on the magnet, and is generally about 9 or 10.

$I - H$ may be determined by means of a current galvanometer very easily by keeping a constant current flowing through the instrument, and using without the magnet one of the less sensitive positions of the magnetometer, and with the magnet one of the more sensitive positions. If D_1 and D_2 be the deflections and n_1 n_2 the number of divisions of deflections corresponding to one ampere at the two respective positions, the value of I is at once found from the obvious equation

$$\frac{D_1}{n_1 H} = \frac{D_2}{n_2 I}, \text{ or, } I = \frac{n_1 D_2}{n_2 D_1} H.$$

When either instrument has been graduated for a field of intensity equal to 1 c.g.s. unit, and the intensity of the field given by the magnet at the needles has been determined, the graduation of the instrument is complete. In the practical use of the instrument with the magnet in position, the number of volts, or the number of amperes (according as the instrument used is a potential or a current galvanometer) corresponding to a deflection of any number of divisions is found by the following rule:—

Multiply the number of divisions in the deflection by the number on the magnet increased by the horizontal intensity of the earth's field,[1] *and divide by the number at the division on the platform scale exactly under the front of the magnetometer.*

When the magnet is not used the rule is the same as the above, except that the divisor to be used is the value of H for the place of the galvanometer.

For convenience in the ordinary use of either instrument a position of the magnetometer on its platform, which may not be one of the series described above, is determined at which the deflection with the magnet in position, for one volt or one ampere, is one or some other convenient number of divisions. For this position lines are drawn on the sides of the platform so as to prolong the white lines on the sides of the magnetometer. By this means currents in amperes or potentials in volts can be at once read off without any calculation.

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[1] The mean value of H for Great Britain may, *when the magnet is used*, be taken with sufficient accuracy as ·17 c.g.s.

The graduation of the current galvanometer may also be performed by means of electrolysis. The electrochemical equivalents of a large number of the metals have been determined, and it is only necessary therefore, in order to graduate the instrument, to join it in series with a proper electrolytic cell and a constant battery, and to compare the amount of metal deposited on the negative plate of the cell with the total quantity of electricity which flows through the circuit in a certain time. A convenient cell may be made with two plates of copper, each about 50 square cms. in area, held parallel to one another about 2 cms. apart, and immersed in a nearly neutral solution of copper-sulphate in a glass beaker. A current of one ampere through such a cell gives a very good result. This current will deposit about 1·2 grammes of copper per hour, which, when light plates are used, can be very accurately weighed. The plates should be carefully cleaned, dried, and weighed before being immersed in the liquid, and care must be taken not to send the current through the cell at all until it is made to flow continuously for the experiment. The instant the current through the cell is completed should be noted, and readings of the current galvanometer taken at equal short intervals during the time allowed for the experiment. The average of these readings is to be taken as the average reading of the galvanometer. When the experiment has been completed the plates are to be taken out and very carefully washed in clean water and dried before being weighed. It is generally better to calculate the quantity of electricity from the gain of the negative plate than from the loss of the positive. The electro-chemical equivalent of copper has been recently determined afresh with great care in the Physical Laboratory of the University of Glasgow by Mr. Thomas Gray, and as the result of his experiments ·000331 of a gramme of copper is deposited by the passage of one coulomb of electricity. Dividing the gain of weight of the negative plate by this number, we obtain the total number of coulombs which have flowed through the galvanometer during the experiment. This divided by the time in seconds gives the average current in amperes; from which the number corresponding to the position of the magnetometer on the platform can be determined, and the graduation completed in the manner described above.

Errata in Part II. of this Article—P. 106, line 2 from top, for $2\pi nCA$ read nCA; line 5, for $CD(=x)$ read $CD(=y)$; line 6, for $DE(=y)$ read $DE(=x)$.

ANDREW GRAY

NORWEGIAN GEODETICAL OPERATIONS[1]

THIS publication of the Norwegian Committee of the Association for the Measurement of Degrees in Europe is a report of the observations made at two tidal stations in Norway—Oscarsborg and Drontheim. The report opens with a summary of the previous tidal operations carried out in Norway. From June 8 to 28, 1835, a series of observations were made at a large number of stations at the request of the English Admiralty. These observations, together with those from numerous other stations in Europe, along the North Sea, and Atlantic coasts, were reduced and compared by the Rev. Dr. Whewell, and were published by him in the *Philosophical Transactions* of 1836.

The next tidal operations recorded were undertaken in 1839, to ascertain whether, as supposed, the Norwegian coast is slowly rising. With this object, marks were cut in the rock at twenty-six points along the coast, and the date was inscribed in each case. On the eastern coast, where there is little or no tide, only one mark was cut, at the level of the water; but along the western and northern coasts two marks were made, one at high water, and the other at low water. The report gives a detailed description of the position of these marks. In 1865 observations were made to ascertain if any alteration had taken place in the relative positions of these marks and of the sea, and it was found that on an average the land was rising at the rate of 3″ in 26 years. It was, however, seen that such determinations were very inadequate, since the mean level of the sea had not been taken into account, and the marks were not connected by lines of levelling. The whole question was accordingly placed before a Committee of the University. This Committee, however, came to no conclusion, principally on the ground of the expense of the necessary levelling operations. It was then pointed out by Prof. Fearnley, the chairman of the Committee of the Association for the Measurement of Degrees in Europe, that the resolutions of the Association at the General Assembly in 1864, at Berlin, had given an increased importance to the determination of the mean tide level. Nevertheless, it was not until 1876 that any steps were taken to establish a series of tidal stations provided with self-registering apparatus, as enjoined by the Association; six stations have now been established, and two more are to be formed.

Previously however, namely, in 1872, two stations had been established, one at Oscarsborg, in the fortified island of Kaholmen, situated in the Christiana fiord, in connection with submarine mining, and where observations have been recorded from 1872 to 1879; the other at Drontheim in connection with the harbour works, the observations at this station have extended from 1872 to 1878. These two sets of observations form, as already mentioned, the subject of the present report.

At both stations self-registering apparatus of simple designs were used. At Oscarsborg the rise and fall of the tide was marked on a *plane* sheet of paper attached to a frame moving horizontally. The motion was imparted to this frame by a weight, which, in order that the motion might be uniform, was connected to a clock. The float was inclosed in a tube, so as to annul the disturbing action of the waves. At Drontheim the paper was attached to a drum rotating uniformly by means of clock-work. The apparatus was placed at the end of a pier, and the scribing pencil was connected by a system of rods to a bridge, the other end of which was supported by a pontoon. In this manner the action of the waves was eliminated. The mean tide level was obtained at these two stations by taking the arithmetic mean of the heights of the tide recorded on the diagrams for each hour, and not by the more accurate method based on the areas described. But since these observations extended over several years, the result is no doubt practically the same; moreover, neither apparatus was provided with any means of obtaining the areas automatically, and to calculate them would have involved an enormous amount of labour.

The observations at both stations are arranged in a series of tables.[1] Table I. gives the heights of the tide at each hour, commencing at noon, for every day during one month.[2] The arithmetic mean of the heights of the tide at the *same* hour each day during the month, are also given for every hour of the day; the moon's influence does not appear in these means, they therefore show the influence of the sun. These means, for every month during which the observations lasted, are collected together in Table IV., and the mean of these means is also given. Table II. gives the height of high and low water, the time of their occurrence, and the range of the tide for every day of the same month as before. Table III. is similar to Table I., but the hours are counted each day from the upper culmination of the moon, instead of from noon. By thus arranging the table, the sun's influence is eliminated in the means for each hour, and the moon's influence on the tide is thus made apparent. Table V. corresponds to Table IV. The remaining tables, VI. to XII., give information as regards the interval of time between the moon's upper or lower culmination and high water, and similarly for low water; also the height of the highest and lowest high and low water recorded each month, specifying the direction of the wind and the height of the barometer; and lastly, the greatest and least range of the tide during each month.

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[1] Publication of the Norwegian Committee of the Association for the Measurement of Degrees in Europe, Part I. (Christiana, 1882.)

[1] The corresponding tables in each set of observations are denoted by the same number.

[2] This table was not extended further, in order to save space in the Report.

The report concludes with a comparison of the semimenstrual inequality as calculated by Whewell's formulæ, and the actual observed inequalities. The agreement is very close.

There is no discussion in the report of the results obtained, this being reserved for future publications, when the observations at the other stations are available.

The report is accompanied by five plates, showing the registering apparatus, and giving specimens of the diagrams.

SCIENTIFIC HERESIES IN CHINA

THE belief of the old writer who explained the disappearance of the swallows in autumn, by the assumption that they all rolled themselves together into a mass, beak to beak and wing to wing, and plunged "fluminibus lacubusque," there to remain until the return of spring, is but an instance of the thousand and one theories which have from all time been held by unscientific observers on the subject of the annual appearance and disappearance of migrating birds. It is not so very long ago that even among ourselves the question of the migration of sand martens was a moot point; and it need not be a matter of surprise, therefore, that the Chinese, though keen observers of nature, should be guilty of holding heretical hypotheses to account for the presence in summer and the absence in winter of birds of passage, as well as for other processes of nature which are enacted before them. Like most common fallacies, those current in China on these subjects are derived from ancient authorities, but, unfortunately, in the case of the Chinese, whose respect for everything that is old is supreme, this antiquity only entails upon them the more unquestioning faith. They are, therefore, perfectly content to believe that the disappearance of quails in autumn is sufficiently accounted for by the assumption that at that time of the year they are transformed into moles, and that in spring they succeed in reappearing again in beaks and feathers. The explanation of this fallacy is simple enough. The ploughman who in the spring and summer has seen the quails flitting among the mole-hills, finds that when, the birds having disappeared, he ploughs over the same land in winter, the moles, which he has not before seen, are the sole occupants of the ground.

Another generally accepted belief is based on what Max Müller calls a disease of language. At the opening of spring, hawks are said to become pigeons, and at midsummer to be reconverted to their original shapes. Now it happens that *Kiu*, the Chinese name for a pigeon, forms the second syllable of the word for a crested hawk (*Shwang-kiu*), and it would appear that by a corruption of terms and a confusion of ideas the first syllable has been dropped and the last has been allowed to stand in its literal and isolated meaning. Thus the original assertion that during the breeding season hawks become crested has been perverted into a meaningless and self-condemnatory myth. Chinese writers on natural history lay stress on the fact that during the spring, when "the rearing instinct in birds becomes excessive, birds of prey become excited," and when excited or angry, hawks, as is well known, erect the feathers on the head, giving the appearance of a crest.

Much in the same way has arisen the legend that in late autumn certain small birds go into the sea and become crustaceæ. In this case the error depends on the word *wei*, which is here translated "become," but which also means "for," and which, when so read, converts an absurdity into the record of a fact concerning the birds probably referred to, namely, sandpipers. These birds, we know, "frequent sandy sea-shores, some of them congregating in numerous flocks in autumn and winter, and seek their food by probing the sand with their bills, and by catching small crustaceans in pools or within the margin of the sea itself."[1] The same mistake, which is, however, complicated by a further misunderstanding, makes it incumbent on Chinamen to believe that in winter pheasants go into the lakes and become clams. The word here translated "clams" means also "sweet flags and water rushes," and in search of these hungry pheasants might very probably be tempted to seek the margins of swamps and lakes. A curious and unaccountable superstition was anciently and is still connected with this habit of Chinese pheasants. We have it on the authority of one of the Classics that "if within ten days from the beginning of winter pheasants do not go to the great waters, lascivious women will multiply in the country."

Otters and polecats, again, are the subjects of a more sentimental belief. They are said to be in the habit of offering up, the one fish, and the other animals, in sacrifice. This strange myth is accentuated in the "Imperial Encyclopædia," published by order of the Emperor K'ang-he (1661-1722), by an illustration attached to the chapter on otters, in which one of those animals is represented as squatting down on the bank of a river with his two forepaws on a newly-captured trout, and with a most devout expression on his upturned face, which is directed towards the moon. The explanation of this legend is not far to seek.

The habit common to both otters and polecats of destroying many more creatures than they are able to devour, and of leaving their victims apparently untouched after having satisfied their appetites with the flakes at the back of the fishes' necks, and with the blood of the animals, suggests to careless and superstitious observers the semblance of propitiatory sacrifices offered up to the patron saints of vermin.

Many other generally accepted myths might be quoted, which are but the perverted representations of facts. But if we descend to a lower level, to the vulgar superstitions of the masses, we find ourselves in a region where—

> "Wisdom and Wit are little seen,
> But Folly at full length."

A resemblance in outward form, or even in disposition, is enough to give rise to a belief that the animals are interchangeable. Thus eels are said at times to be transformed into serpents, mice into bats, and sharks into tigers, and *vice versâ*. By a curious connection of ideas, between kings and whales, also comets, those stars so abhorred by rulers, are considered to be as destructive to the lives of the monarchs of the deep as of the sovereigns of the soil.

NOTES OF TRAVEL IN SARDINIA

WHILE Sicily possesses classical associations and remains of the highest interest, together with physical features more or less dependent upon the presence of the most famous volcano in the world; and while Lipari and the associated islands are remarkable for their evidences of past and present volcanic action,

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[1] *Chambers' Encyclopædia.*

Sardinia can lay claim to neither the one interest nor the other, to any marked degree. Neither can we compare it with Iceland, or with Majorca, and perhaps the only special interest which belongs to it is the occurrence of large numbers of *nuraghi*—conical stone mounds of prehistoric construction, hollow within, and probably designed as tombs by the earliest inhabitants of the island. These are scattered over the island in large numbers, particularly near Torralba.

In Roman times Sardinia never rose to much importance, hence the relics of that period are but few. The most important is an amphitheatre near Cagliari hewn out of the rock, the major axis of which is 153 feet in length, and the minor axis 98 feet. It is now in a very dilapidated condition, far more so indeed than that of Puzzuoli. There are also a few Roman tombs. The most remarkable is in a suburb of Cagliari called Santa Tenera, and it is known as the *Grotta dessa Pibera*, that is the Grotto of the Viper, from the serpents which are sculptured over the entrance. It was the tomb of Attilia Pamphilla, a noble Roman lady. On the walls there are some interesting inscriptions, which have been published by General La Marmora and by Muratori.

The few travellers who visit Sardinia nowadays are tempted rather by prospects of sport than by anything else. Moufflons still exist among the Gennargentu Mountains, also wild boar, and smaller game, but the amount of sport afforded by the island has been exaggerated, and the sportsman will commonly prefer to go to the north of Norway or to Iceland to running the risk of catching malarious fever in Sardinia.

Malaria has always been very prevalent in the island. There is a large extent of marsh land, and in the autumn a great deal of decomposing vegetable matter. We were glad to notice that some of the Englishmen who have recently acquired land in the island have not only commenced draining operations, but have also planted numbers of Eucalyptus trees, the effect of both operations being undoubtedly to diminish malaria. On the other hand, many of the native landowners are converting their timber into charcoal, for which they can obtain about fifty francs a ton in France and elsewhere. Some thousands of tons are annually shipped, and unfortunately new trees are not planted in place of the old ones. If this wholesale destruction of forests continues, there can be no doubt that the climate of the country will eventually be seriously affected. The exporters do not in the least realise that they are shipping *vis viva* in a very condensed form from their shores, and at the same time diminishing the rainfall.

The chief wealth of Sardinia lies in her mines of argentiferous galena, and of calamine. As much as 120 ounces per ton of silver have been extracted from some of the lead ores. The principal mines are those of Monte Poni, near Iglesias, in the south-west of the island, and of Monte Vecchio, in the west centre. At Monte Poni we noticed that the newest forms of Belgian and German machinery for crushing and washing the ores were in use. At the present time, the operations are very much impeded by the flow of water into the principal shaft, which will probably be to a great extent obviated, by boring a tunnel through the side of the mountain in which the shaft is sunk.

A railway constructed, and to a great extent owned by an English company, now connects the two capital cities of the island—Cagliari and Sassari—a distance of 260 kilometres, with branches to Iglesias, in the south-west, and to Terranova in the north-east, and the line is continued from Sassari for 12½ miles to Portotorres, its port, a miserable and fever-stricken village. Fifty years ago, there were scarcely any roads at all in Sardinia. The Roman roads had become obliterated, and no attempt had been made to construct them afresh.

The railway is well constructed, but the trains are extremely slow, and do not average more than seventeen miles an hour. Between Macomer and Chilirani there are many cuttings and some very steep gradients. The railway connecting the two capitals—Cagliari and Sassari—has only recently been completed. Sassari, a town of 33,000 inhabitants, is nearly as large as Cagliari, and in some respects preferable to it. It stands upon a hill 650 feet above the sea. It has a clean, bright appearance, but in reality is very badly drained and extremely unhealthy. So recently as 1855 the cholera carried off nearly one-third of the population in less than three weeks, at the rate of more than 500 a day.

In Torralba there are a number of *nuraghi*, and in the neighbourhood several extinct volcanoes, the most important of which is Keremule. Nearly midway between Mores and Torralba we saw exposed, in a recent railway cutting, a fine mass of columnar basalt overlying chalk. There is a good deal of pale green, pale pink, and grey trachite in the neighbourhood. The only geological map of the island which now exists is in General La Marmora's fine monograph published in Paris and Turin between 1839 and 1860, and entitled *Voyage en Sardaigne*.

G. F. RODWELL

MATHEMATICS IN SCANDINAVIA[1]

THE first part of the new mathematical journal has reached us. We have not quite reproduced the title; the words *Zeitschrift herausgegeben von* on one side of the axis of symmetry (let us say) of the page, are matched by the words *Journal rédigé par* on the other side. This is significant of one part of the Editor's plan: the journal, though printed and published in Stockholm, is to have its articles written in what the Editor styles the *principal* languages. It may be that English is one of these languages; there is not, however, in the preface, anything definite to relieve our doubts. The prospectus (unintentionally, we hope) is somewhat more informatory. In the language of our "lucid" neighbours it says: "In Germany, in France, in Italy, in Scandinavia, everywhere in fact, where science is held in honour (other side of the line of symmetry—'*überall wo mathematisches Leben herrscht*'), the idea of starting the journal was received with the most lively sympathy." Apparently in regard to the English language and English science, the less said the better. We, for our part, say nothing.

In outward appearance the new journal closely resembles Crelle's. The paper is equally good, the margin equally broad, and the size of page and the number of pages in a part substantially the same in the two serials. Neither is quite so handsome as a third member of the same family, the now five-year old *American Journal of Mathematics*; but then we must not forget the ratio of five dollars to twelve marks.

The list of the editorial staff supporting M. Mittag-Leffler contains many distinguished names. There are five Swedish mathematicians, four Norwegians, three Danish, and one from Finland; and scarcely one of these but is well known far beyond his native country.

The contents of the first part are all that could be expected from such a brotherhood, headed by such a chief. The first paper is by Prof. Poincaré, of Paris, its subject being the *Théorie des groupes fuchsiens*. It extends to 62 pp., and is altogether worthy of its place of honour. One does not know which to admire most—the author's grasp of his subject, or the clearness and simplicity of his exposition. Following this, comes a contribution of 14 pp. by Prof. Malmsten, of Upsala, "Zur Theorie der Leibrenten;" then there is a paper of 16 pp. on "Eine Annäherungsmethode im Probleme der drei Körper," by M. Gyldén, the head of the Stockholm Observatory; and lastly, to complete the 96 pages, there is a short communication entitled "Das Problem der Configurationen," by Prof. Reye, of Strassburg. The Editor may be most heartily congratulated on the start he has made: taking everything into account, we have little doubt but that his efforts will be crowned with abundant success.

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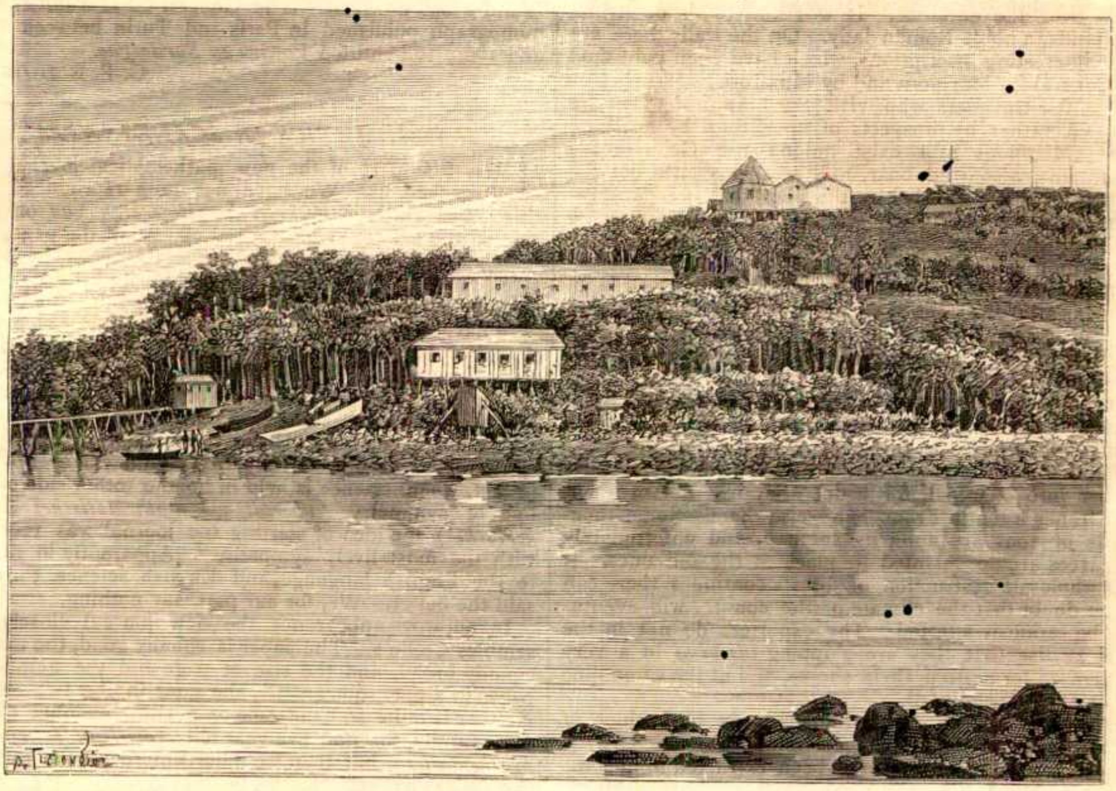
[1] Acta Mathematica: Zeitschrift herausgegeben, von G. Mittag-Leffler. (Stockholm, 1882.)

The most serious difficulty about such an undertaking is that of finance. The *American Journal* began its career with the Johns Hopkins trustees at its back: we suppose, however, that by this time it walks alone. In the present instance, the mainstay is the enlightened King Oscar the Second. Long may he live! The journal is rightly dedicated to him, the dedication being made appropriately in one of the second-rank languages, which it is cheering for us to see, have sometimes their uses.

M. D.

THE FRENCH MISSION TO CAPE HORN

THE members of the French Magnetic and Meteorological Expedition to Cape Horn have taken up their quarters at Orange Bay, and have already begun work. The accompanying illustration, reproduced from *La Nature*, after a photograph transmitted to the Paris Academy of Sciences, will give an idea of the aspect of the station occupied by the expedition. On the summit of the hill are the astronomical cabins, beside which are placed a pluviometer and an actinometer. The large house in the middle distance forms the officers' quarters, while the lower building is for the sailors. Along the shore are other structures partly shown in the illustration, a stockade for the tidal register, and an isolated tent for absolute determinations.



Station of the French Mission to Cape Horn.

The mission arrived at Orange Bay, Terra del Fuego, on September 6 last. They found the country marshy, and were compelled to select a wooded spot in order to obtain firm ground. No time was lost in erecting inclosings and installing the various instruments; and on September 26, the meteorological and magnetical observations were begun. Since the arrival of the party the temperature at Orange Bay has been very mild; the thermometer has never been below 0° C., and several times it has been as high as 16°. The air is very moist, and there has been plentiful rain almost every day, though not lasting long. Squalls have been rare. The magnetic observations will be made partly by instruments which will be read directly,—absolute determinations of declination, inclination, horizontal force, &c., and partly by means of regulating apparatus, which, so far, have worked very satisfactorily, and have given indications agreeing with those obtained from direct-reading magnetometers. The other duties of the expedition consist in astronomical and meteorological observations.

The expedition has been well received by the natives, one of whom speaks and reads English fluently. Indeed, twenty miles off, in Beagle Channel, is an English mission station, which is reported to be very prosperous. On the whole, the French expedition has been very successful; it may be regarded as one of the International Polar Observing Stations.

HEATING BY ACETATE OF SODA

M. A. ANCELIN, Civil Engineer, describes in *La Nature* a method he has devised of heating for domestic purposes, travelling, &c., by means of acetate of soda. His object has been to devise a method that will possess all the advantages of heating by means of hot water, without any of its inconveniences. For this purpose he sought for a vehicle having a great latent heat of fusion, and after several preliminary experiments, he, in September, 1878, took out a patent for heating carriages, &c., by means of the latent heat stored in solid substances previously liquefied by heat.

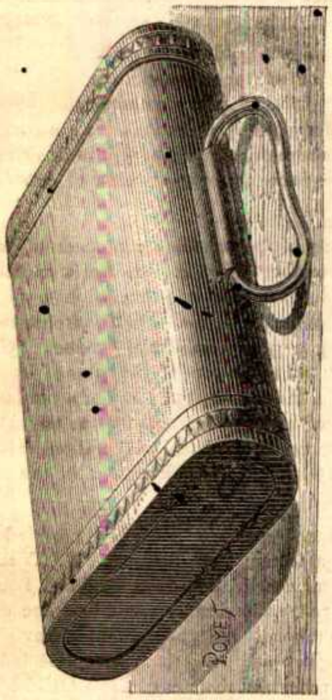


FIG. 1.—Warming-pans with acetate of soda for domestic use; the large form is for apartments or beds, the smaller for a lady's muff.

In the course of his experiments, M. Ancelin's attention was called by M. Camille Vincent to the acetate of soda, the very slow cooling of which during manufacture had struck him. M. Ancelin then experimented with this substance, and obtained satisfactory results.

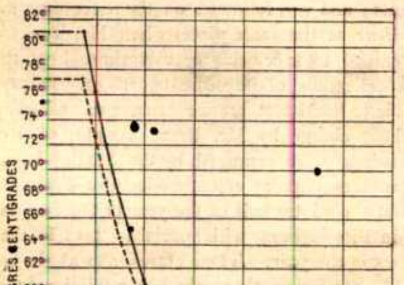


FIG. 2.—Curves of cooling of warming-pans with water.

The duration of the heat in a warming-pan with acetate of soda he finds to be four times that of a warming-pan with hot water in spite of the great calorific capacity of water. This is due to the enormous quantity of heat which must be applied to the acetate of soda in order to change it from the solid to the liquid state, a heat which it again gives off as it resumes the solid state. As the result of his experiments, M. Ancelin finds that the quantity of useful heat is in fact four times greater in acetate of soda than in water. A railway warming-pan containing 11 litres of water, in passing from 80° C., the mean temperature at which it is put in the carriage, to 40° the temperature below which the heat is no longer perceptible, disengages 440 calories (11×40). The same pan containing about 50 kilogrammes of acetate, in passing from 80° to 40° disengages 1731 calories instead of 440. Practice is in accord with theory, as may be seen from the curves in Figs. 2 and 3. We see how rapid is the decrease in the temperature of the water warming-pan, while for the acetate pan the curve, at first parallel to that of water, suddenly changes at the point which corresponds to the temperature of crystallisation.

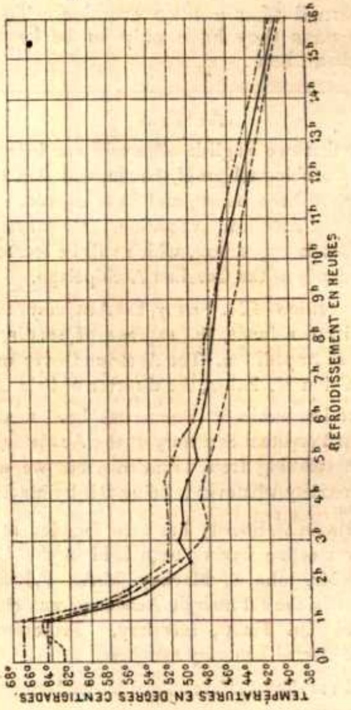


FIG. 3.—Curves of cooling of acetate of soda.

The curve then remains almost horizontal, and falls very gently, rendering evident to the eye what takes place inside the pan. We obtain this result at a much less expenditure of heat for the acetate than for water. To raise the pan of water of 11 litres from 10° to 90°, four times, there is required 3520 calories. For the same quantity of acetate only 1987 calories are required, showing a saving of 1500 calories in favour of the acetate. In reality the saving is much greater. In the case of the water-pans raised to 90°, they are only at a maximum of 80° when put in the carriage, and for four heatings we get only 1760 calorics, or 50 per cent. of the heat stored. In the case of the acetate, there are only 256 calories unutilised, or about 12 per cent. of the quantity stored. M. Ancelin claims for his method that it required almost one-half less expenditure of heat than in the case of the usual warming-pans, especially when we consider that the water requires four separate heatings, and the acetate only one. Long journeys can thus be made by rail, say from Paris to Havre, Lyons, Bordeaux, &c., without having to change the warming-pans, a great saving of labour and of annoyance to passengers. Several companies both in France and in other countries now employ M. Ancelin's method of heating; the French Western Railway Company use it in their carriages from Paris to Havre, and also to Dieppe. In England, M. Ancelin states, the London and North Western Company had, last winter, 3000 of his acetate pans in use, and double that number during the present year.

He shows that his system may be applied to domestic purposes as well as on railways. It is certainly preferable to charcoal, which, in France, is a fertile cause of death by asphyxia. Fig. 1 shows a portable apparatus, which may be used in private carriages, and even as a foot-pan in bed, and several other purposes, its heat lasting five hours. The smaller figure shows a form of heater which may be used in a lady's muff or even in the pocket. The openings by which the acetate is introduced are hermetically closed, and the substance does not require renewal except at very long intervals. In filling the receptacle certain precautions must be used, which may be easily learned after a little practice. To renew the heat in the pans they have only to be kept in boiling water for half an hour.

NOTES

We believe that two English observers are being sent out to record the approaching eclipse of the sun, and that the American Government have been asked, and have agreed, to find places for them with the American expedition. M. Janssen will be the head of the French expedition, which will be located on one of the smallest islands of the Caroline Archipelago.

On the proposition of M. Fremy, the Academy of Sciences will defray, from its own funds, the expense of sending a naturalist with the Eclipse Expedition. The Austrian Government will send to the same station M. Palisa, the director of Pola Observatory.

For the two last weeks M. Dumas has been unable to attend to his duties of Perpetual Secretary of the Academy of Sciences. He has been suffering from bronchitis, but we are happy to state that no anxiety whatever is being felt by his friends.

A Mathematical Society has been founded at Edinburgh, the initiatory meeting having been held in the University on Friday last. Mr. John S. Mackay, M.A., F.R.S.E., Mathematical Master in the Edinburgh Academy, was chosen the first president, and Dr. Knott, secretary. Professors Tait and Chrystal were elected honorary members.

General Pitt-Rivers has offered his well-known and invaluable collection, now in the South Kensington Museum, to the University of Oxford, on condition that the University provides a suitable building for it. It is to be hoped, for the sake of the University and in the interests of science, that the authorities will accept the collection on the conditions imposed by the generous donor, though we should deeply regret its removal from London.

We are not surprised that the London School Board should have hesitated last week to commit itself to the importation at once of technical education into elementary schools. The adoption of Dr. Gladstone's motion, that a committee be appointed to consider how best the Board could help in the matter, seems to us to be the judicious course to follow.

The following premiums are offered by the Society of Arts for the 129th Session of the Society (1882-83):—Benjamin Shaw Prize.—1. A Society's gold medal, or 20*l.*, for the best plan for "obviating or diminishing risk to life in the operations of coal mining." 2. A Society's gold medal, or 20*l.*, for the best plan for "obviating or diminishing risk to life in the manufacture, storage, and transport of explosives." The Council of the Society leave it to the competitors to bring the plans under their notice in any way they may think proper, whether by model, written description, or otherwise. Howard Prize.—A prize of 100*l.* for the best essay on the Utilisation of Electricity for Motive Power. Preference is to be given to that essay which, besides setting forth the theory of the subject, contains records with detailed results of actual working or experiment. The Society reserves the right of publishing the prize essay. Fothergill Prize.—A Society's gold medal, or 20*l.*, for the best invention having for its object the prevention or extinction of fires in theatres or other places of public amusement. Designs, plans, models, essays, descriptions, inventions, &c., intended to compete for any of the above prizes, must be sent in on or before October 31, 1883, to the Secretary of the Society of Arts, John Street, Adelphi, London.

The Industrial Society of Berlin offers a number of prizes, amongst which we note the following:—1. For a method of precipitating zinc by galvanism from its dilute sulphate solution, 50*l.* 2. For the examination of German crude petroleum, with directions for preparing a good commercial product, 75*l.* 3. For a criticism of the usual indications of the value of iron and a proposal of more useful indications, 15*l.* 4. For a plan of the technical arrangements of a public institute for the examination of tissues, in order to oppose the frequent adulterations met with in textile industries, 15*l.* 5. For ameliorations in salt mines and salt works, 75*l.*

The Birmingham papers report the Town Hall crowded with working men to hear a lecture from the Rev. W. Tuckwell, Rector of Stockton, near Rugby, on "The Midland Boulders and the Great Ice Age." The lecturer described the erratic blocks of the neighbourhood, and some recent discoveries of boulder clay at Birmingham and Stockton. He presented in a popular form the discoveries and theories of Croll, Geikie, Boyd Dawkins, Lubbock, Evans; and drew a picture of early man and his brute contemporaries as revealed by the bones and implements of the caves and river gravels. The lecture was illustrated by lime-light views of glaciers, extinct animals, and human implements; and was followed on the succeeding evening by a *conversazione* at the same place, when Mr. Tuckwell exhibited and explained to successive crowds throughout the evening a splendid collection of implements, ranging from the earliest palæolithic to the latest bronze ornaments and weapons, kindly lent without charge by Mr. Bryce Wright, of Regent Street. Both evenings were arranged by the Sunday Lecture Society, which provides popular scientific lectures every Sunday night throughout the winter half of the year in the four largest Board Schools in Birmingham, with special lectures in the Town Hall three times in the year. The lectures, as was the case with Mr. Tuckwell's, are frequently marked by a religious, though not by a sectarian tone; and the crowded audiences consist of persons rarely or never seen in church or chapel.

The Berlin Academy of Sciences is about to send Dr. Lepsius, Professor of Geology at Darmstadt, with an assistant, to Athens, to make a geological survey of the neighbourhood, and endeavour to decide the question of the origin of the Athenian marbles.

Under the presidency of Count Hans Wilczek and the Baron Victor Erlanger, an International Electric Exhibition will be held in Vienna, opening on August 1, and closing on October 31. It will be an undertaking of a private nature, but is specially favoured by the Government.

There seems to be a serious decline in the once flourishing oyster fisheries of Denmark. Last year only about two million oysters were taken, which is far below the average, nor was the quality so good as usual. There were no new banks discovered during the year. The most important are now those in the Gulf of Vendsel and at Fladstrand.

THE Smithsonian Institution have received from Dr. Stejneger, who was despatched on a scientific mission to Behring Island some time since, *eleven* fairly perfect crania of the extinct Sirenian Mammal, *Rhytina stelleri*, together with sets of nearly all the other bones of the skeleton.

THE Royal Swedish Geographical Society has decided to appoint a committee, consisting of Professors Nordenskjöld and Gyldén, and Consul Elfwing, to consider the proposal for an international meridian and a common time. This committee has requested Consul Elfwing to draw up the Society's report on the question.

THE eminent Swedish palæontologist, Nathorst, has published the results of his examination of the fossil plants collected by the *Vega* expedition at Mogi, in South Japan. According to Hr. Nathorst these remains belong to the Tertiary period, and while they are older than the Glacial age, they would yet seem to be referable to a time when the climate was colder than at present. This instance of the presence in Eastern Asia of a flora of polar origin shortly before the advent of the Glacial period, is considered especially interesting from the strong evidence which it affords in favour of the theory of the migrations of plants in the early ages of the world's history.

FROM a communication of the results of Herr Håkonson-Hansen's observations of the November auroral displays which is printed in the last number of *Naturen*, we learn that while there were as many as nineteen specially vivid and one ordinary manifestation at Trondhjem in that month, the extraordinary length of time during which the auroræ retained their brightness gave a special character to the phenomena. On every night of the week from the 12th to the 18th inclusive, the heavens were illuminated with the auroral light, which on the 13th appeared as early as 4 p.m., while on the 17th it continued visible from 8 p.m. till 6 a.m. on the following morning. During this period it was found almost impossible to work the telegraphic wires by day or night. The most striking display occurred on the 18th, at 4.30 a.m. when a brilliant corona appeared in the zenith, from which vivid streams of light stretched to the horizon, while luminous waves flowed uninterruptedly from the latter towards the corona, diffusing so strong a light as to enable one with ease to read moderately clear print. On the same day, at Gothenburg, it was found impossible to make the wires act.

Naturen warns its readers not to put implicit faith in the statements made in reference to the large yields of silver, which may be expected from the old silver mines in Nordland. According to the prospectus of a company, which offers the public one thousand shares in the Svenningdal mine, near the little town Mosjo, 162,000 kroner's worth of ore was purchased from the owners in 1881 for the silver works at Freiberg in Saxony, yet, as the writer points out, there were only twenty-five men employed in the mine in the course of that year! It is well known that traces of silver are constantly found in the galena and cadmium yielded by these mines, but this fact even the most daring of speculators must admit to be a very insufficient basis, on which to maintain the numerous companies, amounting, it is said, to one hundred, which are offering the public at home and abroad shares in one, or other of the more or less exhausted mines of Nordland.

THE Anthropological Society of Paris has received from one of its members, Dr. Benzengre, a report of the autopsy of General Skobeleff, from which it would appear that the weight of the brain, according to Broca's system, was 1457, which is considerably above the mean for ordinary adult Europeans of his height (1·73m.) and even slightly above that hitherto given for men of exceptionally great intellect.

DR. WILLIAM H. STONE will give the first of three lectures, at the Royal Institution, on Singing, Speaking, and Stammering, on Saturday next (February 17); and Prof. Robert S. Ball will give the first of four lectures on the Supreme Discoveries in Astronomy, on Tuesday (February 20).

THE Norwegian Government have commissioned Dr. S. A. Buch during the present year to make researches, practical as well as scientific, into the great herring fisheries which annually take place on the west coast of Norway.

THE new mathematical journal, *Acta mathematica* (noticed in another column), published in Stockholm, Berlin, and Paris, has received a subsidy of kr. 1000 from the Swedish Government, while the Danish have granted a sum of 60*l.* a year to the editors in Denmark—Prof. Lorenz, and Drs. Petersen and Zeuthen.

THE U.S. Commissioners of Fish and Fisheries propose to build a large aquarium at Wood's Hole, Mass., in connection with their new station there. The aquarium will be devoted to biological researches of every description. At the adjoining station preparations are being made for the artificial propagation of cod, mackerel, halibut, and other fishes useful for food, and it is expected to hatch annually a thousand millions of cod and of other kinds in proportion.

AT the meeting of the Essex Field Club held on January 27, a resolution was passed condemning the proposed extension of the Great Eastern Railway from Chingford to High Beach. The Club regards it as "wholly unnecessary for the railway to take the route projected, and that it would not fail to prejudicially affect the advantages secured by the Epping Forest Act, which directs that the forest is to be preserved as far as possible in its natural aspect, and the Society hereby authorises the Council to petition Parliament against the project, and to send copies of this Resolution to the Press."

PROF. VERGA has been recently investigating the subject of intemperance in Milan (*Reale Ist. Lomb.*), a vice which seems to be on the increase there. Among other things he notes that suicide and kleptomania are very rare among the drunkards. With regard to the two sexes, he says that women fall much less easily into intemperance than men, and that drunken women belong to the lowest social strata, and show true brutification. Men appear to give themselves to excessive drinking more in the cold season; women in the mild season. Women relapse more frequently and shamefully into drunkenness than men, and more easily remain victims. The female drunkard excites disgust or laughter by her behaviour; but is not dangerous either to herself or to others; the male drunkard alarms by his excesses, and has often to be severely punished.

THE behaviour of the virus of anthrax in the form of spores and in that of the bacterium (*Bacillus anthracis*) under heat and various reagents, has been investigated fully by Signor Perroncito. The results are published in the *Atti della R. Accademia dei Lincei*, and show well how much greater power of resistance the spore has than the bacterium.

THE Jablochkoff light of the Avenue de l'Opera having been extinguished, the Place de Carrousel is the only street now electrically lighted in Paris at the expense of the Municipal Exchequer. But the electric light is every day finding new fields of display; a building company who are erecting a large structure in the Paris will manufacture the light on their own premises for their own use, and sell it to the tenants at moderate rates.

M. THOLLON has sent from Nice to the Academy of Sciences a determination of the velocity of the motion of the great 1882 comet, calculated from the displacement of the spectral rays.

THE action of very diluted nitromuriatic acid (aqua regia) on meat and other animal substances has been recently studied by Signor Pavesi (*Giorn. Farm. Chim.* xxxi. 529), and he finds the substance an excellent preserving agent; meat in pieces of about 1 kg. kept in the liquid in wooden vessels remains unaltered and savoury for years. The meat treated may also be dried at 15° to 20° without undergoing change, apart from a diminution of volume and the appearance of a brown colour. Put for a few hours in water, the meat recovers its original softness and natural colour. The proportions of the acids in the preserving liquid are not given. The method is also adapted to preservation of animal substances for scientific purposes.

TWO shocks of earthquake occurred at Agram, the first at 8.44 p.m. on the 4th and the other about 1 a.m. on the 5th. Both were of a violent character, accompanied, as the former disturbances were, by a rolling, thundering noise underground. The direction of the motion was from north-east and south-west, and each shock lasted about four seconds. No damage has been done hitherto. A telegram from New York, February 5, states that earthquakes have occurred at Bloomington, Illinois, and Wolfborough, New Hampshire, U.S., but no serious consequences are reported.

THE German Aëronautical Society held its general meeting at Berlin on January 13 last. During 1882 no less than 230 proposals, principally relating to the steerage of balloons, were submitted to the Society, none, however, furthering the question in any material way.

A MEMBER of the Paris "École pratique d'acclimatation" has discovered a species of spider on the African coast, the firm and long web of which resembles yellow silk very closely, and is said to be almost as good as the product of real silkworms. The syndicate of the Lyons silk-merchants has closely investigated the matter, and the result is reported as highly favourable. There seems to be no difficulty in the way of acclimatising the new silk-producer in France.

NEWS has been received in Bolivia regarding Dr. Crevaux's mission. It appears that several members of this expedition were not killed, as was formerly reported, but are kept prisoners by the Tabo Indians.

THE Museum of the Berlin Society for Commercial Geography will be opened on April 1 next. From time to time there will be in this museum special exhibitions arranged by foreign states. Several of these are already announced. The best part of the Brazilian exhibition will remain in the Museum.

LIEUT. WISSMANN, the intrepid and successful German traveller, arrived at Cairo on January 1. His route from Loanda, by way of Nyangwe, on the Lualaba River, to Zanzibar, which measures about 3600 kilometres, led him for at least one-third of the distance through unexplored country. He has thus solved some of the enigmas of equatorial Africa. It is the southern half of the Congo basin through which Wissmann passed, and he found this to be most densely populated. This fact is remarkable, as it was entirely unexpected. Wissmann also passed through the land of a tribe of dwarf negroes. On the long and dangerous route from Lake Tanganyika to Zanzibar the traveller met with a most hospitable reception at the hands of the renowned brigand chief Mirambo, who supported him in every respect.

IN our last number we stated that M. Tissandier's electro-magnetic machine had given a power of 4 horses per hour; it should have been 4 horse-power.

THE additions to the Zoological Society's Gardens during the past week include a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, presented by Mrs. Norman; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mrs. Sims; a Peregrine Falcon (*Falco peregrinus*) from North America, presented by Mr. C. H. Webster; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Mr. G. S. Northcote; four Ceylonese Terrapins (*Clemmys trijuga*) from Ceylon, four —— River Turtles (*Emyda* ——) from India, a Globose Curassow (*Crax globicera* ♀) from South America, deposited; a Blue-cheeked Amazon (*Chrysotis cœligena*) from Guiana, two Maximilian's Parrots (*Pionus maximiliani*) from Brazil, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), two Four-horned Antelopes *Tetraceros quadricornis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

DENNING'S COMET.—Mr. W. E. Plummer, of the University Observatory, Oxford, has made an interesting contribution to a branch of astronomical investigation, in which we have not shone greatly in this country, in the shape of a definitive determination of the elements of the orbit of the comet discovered by Mr. W. F. Denning, of Bristol, on the morning of October 4, 1881, which proved to be one of short period, though not previously observed. Accurate positions were obtained between October 5 and November 19 at Athens, Dun Echt, Harvard College, U.S., Marseilles, Odessa, Oxford, Palermo, Paris, Rome, and Strasburg. Starting with the second ellipse calculated by Dr. Hartwig, which assigned a revolution of 8·884 years, Mr. Plummer compares all the observations with an ephemeris computed therefrom. He then determines, by the method of variation of constants, for four day intervals, the effect of perturbations by each of the planets from Mercury to Saturn inclusive, during the period of visibility; the influence of the perturbations upon the observed right ascensions and declinations being inferred by calculating the differential coefficients for variation of elements for the particular epochs, and these coefficients were used in the formation of equations of condition. The tabular longitudes of the sun were corrected by the results of observations at the Royal Observatory, Greenwich, supplied by the Astronomer-Royal. Normal equations were then formed and solved in the usual manner by least squares, and a corrected set of elements was thus found. The positions of the comet computed from them, and the positions inferred from the substitution of the corrections to the elements in the original equations of condition, agreed generally, but owing to their considerable amount, and the neglect of terms of the second order in calculating the differential coefficients, the agreement was not exact. Since Mr. Plummer's principal object was the determination of the comet's mean motion, he preferred to obtain the values of the several unknown quantities in terms of the mean motion, so that by successive small variations of this element, accompanied by the corresponding alterations in the others, several sets of elements could be formed, and the preferable orbit selected by direct comparison with the observations. This additional labour adds much to the value of Mr. Plummer's work. He accepts as the most trustworthy guide the sums of the squares of the errors in right ascension and declination, though the two do not correspond to precisely the same value of the mean motion, and so obtains the following definitive orbit:—

Epoch 1881, September 28·5 Greenwich M.T.

	°	′	″	
Mean anomaly	1	40	35·39	
Longitude of perihelion ...	18	36	12·8	Mean Eq. 1881·0
,, ,, ascending node	65	52	2·0	
Inclination	6	50	22·6	
Angle of eccentricity	56	8	28·4	
Log. semi-axis major	0·6315148			
Period of revolution	3235 days.			

Hence we find (the unit of distance being the earth's mean distance from the sun)—

Semi-axis major ...	4·28070	Eccentricity	0·8304135
,, minor ...	2·38498	Period in years ...	8·8567
Aphelion distance..	7·83545	The perihelion passage Sept. 13·43493 M.T. at Greenwich.	
Perihelion distance	0·72595		

The orbit of this comet is remarkable for the near approach it makes to the orbits of Venus, the Earth, and Jupiter. By Mr.

Plummer's figures it appears that in longitude 223°·4 the comet is distant from the orbit of Jupiter only 0·145, a sufficient explanation of a probable cause of the short period of revolution.

THE GREAT COMET OF 1882.—The positions subjoined are extracted from an ephemeris published by Herr Stechert, of Berlin (*Astron. Nach.* No. 2486), and founded upon the elliptical elements of Dr. Kreutz:—

At Berlin Midnight

	Right Ascension. h	m.	s.		Declination. °	′		Distance from Earth.		Sun.
February 8 ...	6	1	57	...	−20	5·8	...	2·412	...	3·008
10 ..	6	0	17	...	19	34·5				
12 ...	5	58	46	...	19	3·7	...	2·506	...	3·063
14 ...	5	57	25	...	18	33·4				
16 ...	5	56	12	...	18	3·6	...	2·602	...	3·118
18 ...	5	55	8	...	17	34·4				
20 ...	5	54	13	...	−17	5·7	...	2·700	...	3·172

ASTRONOMICAL TELEGRAMS.—Mr. Spencer F. Baird, Secretary of the Smithsonian Institution, notifies that arrangements have been completed with the Director of the Harvard College Observatory for conducting the system of telegraphic announcements of astronomical discoveries, which was established by the Institution in 1873, and that henceforward the American centre of reception and distribution of telegrams will be "The Harvard College Observatory, Cambridge, Massachusetts," to which all astronomical telegrams should in future be sent.

THE MATTER OF SPACE

OF late years there has been a growing tendency towards the belief that matter is present everywhere throughout the universe, as well in interstitial space as in the bodies of the spheres. Yet an older hypothesis is still widely held. The phenomena of light seem to require some substantial medium in space, but this substance has been viewed as specifically distinct from matter, and named ether. Another class of thinkers has devised still another species of substance. This is required to meet the demands of the new gravitation hypothesis; and consists of excessively minute particles, moving with intense speed, and pressing vigorously on the larger and slower particles of matter. In the past still other species of substance were imagined; heat, electricity, etc., were each ascribed to a specifically distinct substance.

Now, however, the tide has turned, and the inclination is to believe in only a single form of substance. There are, of course, countless distinct conditions produced by the aggregations of substance, and variations from simplicity to complexity, but this may not necessarily require more than a single kind of basic particle, or whatever we may call it. If the substantial contents of space are similar in constitution to the matter of the spheres, their state of existence must be much more simplified. In the spheres we have matter ranging from the simple elementary gases of the atmosphere, through the complex mineral compounds of the solid surface, to the highly compounded organic molecules. In outer space the variation is probably in the opposite direction, and substance may exist there in a condition much more highly disintegrated than the atmospheric gases. This view is not held by all theorists. Dr. Siemens argues that space holds molecules of considerable intricacy, comprising certain terrestrial elements, and their simpler compounds; as to the contents of space we know that there are very numerous solid masses, some of considerable size, others minute, and possibly ranging through many degrees from the largest to the minutest. Yet these really occupy but an inconsiderable portion of space, and apparently originated in solar or planetary orbs.

Such is, briefly stated, the state of knowledge and of hypothesis concerning the substantial contents of space. We need but add the uncertain reasons for arguing the presence of a resisting medium in space, and the necessity of a highly elastic condition of the light-conducting substance, to exhaust the subject so far as yet pursued.

It is held by some that the gravitation energy of the suns and planets is sufficiently great to sweep space of all contiguous material particles, except those solid masses which are saved from this fate by the vigour of their orbital motions. The atmospheres of suns and planets are retained with an energy very greatly in excess of their reverse energy of molecular motion, and therefore it is quite impossible that any of this material should escape into space, or that any similarly-conditioned material should exist contiguous to the spheres without being forced to become atmospheric matter. The centrifugal energy of the earth's atmosphere at the equator is only $\frac{1}{17}$ of that necessary to overcome gravity. The molecules of the atmosphere have also a vigour of heat vibration about equal to their centrifugal energy. Hence the resisting energy of these molecules is far below the gravitative energy, and they are vigorously held.

The question of the possible existence of gravitating matter in interspheral space depends strictly upon that of its motor energy. If the momentum of any particle, or of the whole sum of particles, be insufficient to constitute a centrifugal energy equal or superior to the centripetal energy of gravitation, then the material contents of space must inevitably be drawn into the attracting spheres, as atmospheric substance, and space be denuded of matter. If, on the contrary, the centrifugal energy of these particles be sufficient to resist gravitation, they will remain free, and space continue peopled by matter.

Such gravitative particles, wherever existing in space, could not be for an instant free from the influence of spheral attraction whatever their energy of motion. If this energy be too small, they must be related to the spheres as falling bodies, and must become atmospheric matter. If the two opposite energies be equal, they must be related to the spheres as planetary bodies, and circle in fixed orbits around the centre of attraction. If the centrifugal energy be in excess they must assume the condition of independent cometary bodies, temporarily influenced but not permanently controlled by any sun, and wandering eternally through space.

Such are the three possible conditions of the material contents of space. If the first obtain, space must be denuded of matter; if the second obtain, it will permanently contain matter in a partially elastic state; if the third obtain, it will permanently contain matter in a highly elastic state, since the pressure upon each other of the vigorously centrifugal particles must be great, and may be extreme. Of course no single particle could long retain its direction of motion, as related to any sphere. Constant impacts must constantly vary the directions of molecular motion. But the motion of each particle is successively transferred to a long series of particles, and thus is virtually continued in force and direction. Each motion pursues its course independently, though not as affecting any fixed particle of matter, and each particle aids in the progression of a vast network of motions, proceeding in every direction throughout the universe. Thus each particle, though not actually changing its place, may have motor relations which extend in every direction to the utmost extremes of space. It is a node in an interminable network of motions, and its incessant leaps throughout the limits of its narrow space are each part of a long motor line, which affects successively myriads of particles. So far as the energy of gravitation is concerned the effect upon this incessantly transferred motion is precisely the same as if the motion was confined to a single particle. If it lack energy the motion will be a falling one; if it equal the gravitative energy it will form a closed orbit. If it exceed the gravitative energy it will form an open curve, and be only temporarily controlled by any sphere.

In this interchange of motor-energy certain particles may continually decrease in vigour of motion, and if near solar orbs may be drawn in as atmospheric matter. But they can only lose motion by transferring it to others, which would in consequence become more independent of gravity. The sum of motor energies in the universe must persist unchanged, and the aggregation of atmospheric substance around any planet must cause an outflow of motor energy which will increase the motor vigour of exterior particles. In such a case the height of atmosphere in any sphere will depend, partly on the attractive vigour of the sphere, and partly on the average motor vigour of the whole sum of matter. Every contraction and loss of motor energy by any portion of matter will increase the motor energy of remaining matter, and a fixed limit to the atmospheric control of every sphere must result, since in the outer layers of its atmosphere the centrifugal energy of molecular motion must increase until it equals the energy of gravitation.

Can we arrive at any conclusion as to which of the three possible conditions above considered really exists? If so we can answer the question as to the existence of matter as a constant tenant of space, and also reach some conclusions as to the character of its motor conditions.

There is one line of thought which seems to lead to a settlement of this question. If the nebular hypothesis of the formation of solar systems be accepted as true, either wholly or partly, there can be no doubt as to the interspheral status of matter. The conditions of nebular aggregation indisputably settle it.

This hypothesis holds that the matter now concentrated into suns and planets was once more widely disseminated, so that the substance of each sphere occupied a very considerable extent of space. It even declares that the matter of the solar system was a nebulous cloud, extending far beyond the present limits of that system. From this original condition the existing condition of the spheres has arisen, through a continued concentration of matter. But this concentration was constantly opposed by the heat energy of the particles, or, in other words, by their centrifugal momentum. This momentum could be only got rid of by a redistribution of motor energy. If, for illustration, the average momentum of the particles of the nebula was just equivalent to their gravitative energy, then a portion of this energy must radiate or be conducted outwards ere the internal particles could be held prisoners by gravitation. The loss of momentum inwardly must be correlated with an increase of momentum outwardly.

This is a necessary consequence of the heat relations of matter. As substance condenses its capacity for heat decreases, and its temperature rises, hence a difference of temperature must constantly have arisen between the denser and the rarer portions of the nebulous mass, and equality of temperature could be restored only by heat radiation. This radiation still continues, and must continue until condensation ceases, and the temperatures of the spheres and space become equalised, but this is equivalent to declaring that as the particles of the spheres decrease in heat momentum those of interspheral space increase, and if originally the centrifugal and centripetal energies of matter approached equality, they must become unequal, centripetal energy becoming in excess in spheral matter, centrifugal energy in the matter of space. Thus, as a portion of the originally widely distributed nebulous matter lost its heat, and became permanently fixed in place by gravitative attraction, another portion gained heat, became still more independent of gravity, and assumed a state of greater nebulous diffusion than originally. The condensing spheres only denuded space of a portion of the matter which it formerly held, and left the remainder more thinly distributed than before. The spheres, in their concentration, have emitted, and are emitting, a vast energy of motion. This motor energy yet exists in space as a motion of the particles of matter, which therefore press upon each other, or seek to extend their limits, with increasing vigour, so that the elasticity of iinterspheral matter is constantly increasing.

It might be hastily imagined that such an excess of heat vigour in the matter of space over that of the spheres should declare itself in temperature. But it must be remembered that temperature is no measure of the absolute heat contents of matter. Condensation increases, rarefaction decreases, temperature with no necessary change in absolute heat contents. The expression "fire mist," so often applied to the matter of uncondensed nebulæ, gives a very erroneous impression. The matter of the solar system nebula, though containing a high degree of absolute heat, was probably of low temperature. Its great rarity must certainly have greatly decreased its temperature. As a differentiation in this matter took place, one portion becoming condensed, another portion more rarefied, the former must have increased, the latter decreased, in temperature. Eventually the extreme condensation of one portion of this matter, and rarefaction of another, caused an extreme difference in temperature. An excessive radiation from the spheres to space has taken place in consequence, the absolute heat of the former constantly decreasing and that of the latter increasing. But the difference in temperature still continues great, the influence producing it acting much more rapidly than the influence tending to obliterate it. Eventually an equality of temperatures may be produced, but only by the production of a very considerable inequality of absolute heat. This must be the final result of spheral condensation and nebulous rarefaction of exterior matter; namely, equalization of temperature, with a change from the original homogeniety to a great heterogeniety of heat contents.

But we are again brought back to the question of the motor energies of matter. Are they sufficiently great to enable a portion of this matter, when reinforced in motor energy by radiations from the spheres, to defy gravitative attraction and, remain free in space? Undoubtedly so, and much greater than would be simply requisite for the purpose, since we find the matter of the planets, after their immense losses by radiation, still possessed of a considerable excess of motor energy. The earth, for instance, has an orbital motion sufficient to maintain it at a considerable distance from the sun. But the motion of the earth is but the combined motion of its molecules. This motion once existed as independent molecular motion, which in time, under the influence of gravity, became dependent molecular motion. We have already spoken of the fact that the particles of space, in consequence of their heat motions, tend to dart off in straight lines of motion, except in so far as the gravitative attraction of spheres causes these lines to become curved. These lines of motion, so far as individual particles are concerned, are checked by the particles coming into contact with others. The motion, however, proceeds onwards, though it is carried by successive, instead of by single particles. If, however, a number of particles move in company in the same direction, they may move much further as individuals, before transferring their energies. And if an immense mass of particles come to thus move in company their individual excursions may be indefinitely extended. The lines of motion, instead of being continued by successive particles, are continued by the same particles, and molecular motion becomes mass motion. The motion of terrestrial molecules, in their revolution around the sun, resemble those of the molecules in Prof. Brooks vacuum tubes, constituting his "fourth state of matter."

Now the degree of resistance of such a mass to centripetal energy will indicate the degree of resistance of the original uncombined molecules. In the earth the motion of the molecules, thus combined, yields a centrifugal energy sufficient to maintain the earth at its present distance from the sun. But this is only a portion of its molecular energies. Its molecules possess considerable independent motion, and form nodes in lines of radiation that extend in every direction. They have also lost a great vigour of motion by radiation to space. It follows that the original momentum of these molecules must have constituted a centrifugal vigour greatly in excess of their centripetal vigour. It secondarily follows that the momentum of those molecules of the nebula which still exist in space, augmented as it has been by radiations from the spheres, yields a very energetic excess of centrifugal vigour. Many of the comets have a centrifugal energy in excess of the centripetal energy of the sun, yet this represents only a fraction of the energy of their molecules, and a much smaller fraction of the energy of the material particles of space.

The combination of the centrifugal energies of terrestrial particles is due to the fact of a secondary centre of gravity having been formed. The heat velocity of its particles, in excess of that displayed in their revolution around the sun, has become partly a revolution around the earth's axis, and is partly retained as heat vibration. But the heat velocity of the material particles of space is not thus secondarily employed. It is affected by the attraction of the sun, or of the nearest sphere; but evidently, from the considerations above taken, this attraction cannot be sufficient to over-balance the centrifugal energy and cause atmospheric aggregation or even to cause orbital revolution. The particles must have energy sufficient to make them independent of spheral gravity. Their straight lines of motion must become to some degree curved in response to gravity, but cannot become closed curves. Instead of becoming planetary, they remain cometary lines, of very open orbit. For if we imagine the earth to be suddenly restored to its nebulous condition, or its particles to be set free in space, they would possess a velocity of motion much in excess of the earth's orbital velocity. Hence they could not be controlled by the sun. The existing particles of space possess a still much greater velocity, and are therefore much more free from gravitative control.

Certain necessary results of this condition have been considered. The lines of centrifugal motion in space are not confined to single particles as in the earth, but are transferred from particle to particle. The effect, however, is precisely the same; this motion of successive particles is in no respect different in effect from what we would have if a single particle were free to move in the same direction. Each particle moves a certain distance, and then transfers its motion in that direction to another. But it immediately pursues some other direction of motion in response to impact, and this aids in the progressive movement of innumerable lines of motor energy. The great centrifugal vigour of these motions must cause an energetic compressing influence upon interspheral matter, and thus produce an elasticity, sufficient perhaps for the requirements of light radiation.

The lines of motion thus transferred through space cannot be unvarying in their orbital directions. Nature knows no great or small in her processes, and each moving particle of the free matter of space is controlled by the same principles which control the motions of a planet. It is subject to perturbations from lateral attractions, similar to those which draw planets and comets out of their orbits, and completely change the orbit of the latter. And its impacts with other particles yield effects such as would arise in impacts between planets of oppositely moving systems. Action and reaction are equal, in this as in every case. The orbit and the speed of a line of motion may be changed through impact or attractive resistance, but only by its causing an opposite change in some other line. Thus the lines of motor energy referred to are not unvarying in speed and direction, but are unvarying in their sum of correlated speeds and directions. The variations which take place in the orbits of spheres and comets through attractive perturbation, and the greater variations which would take place did spheres come frequently into contact, are precisely similar to those which must occur in the case of interspheral particles, and any change in the direction of one orbit is balanced by an equal opposite change in the direction of another orbit,—the balance of motor direction and energy in nature being exactly preserved.

If such a line of motion pursues a cometary ellipse and enters the atmosphere of a globe, it must be affected by friction precisely as if the line of moving particles were a single particle, or a minute comet. It might be obliterated by friction or resistance, as the orbital motion of a falling body is obliterated. But this obliteration is really caused by the opposing energy of opposite lines of molecular motion. The single line of motion may be distributed into a thousand lines differing in direction, but the component of these thousand lines must agree with the original line.

The transfer of motion from particle to particle here indicated may take place through attractive resistance as well as through impact resistance. The original disintegration of the matter of space must have increased, as spheral condensation denuded space of much of its material, and as radiation from the spheres increased its motor energy. If matter thus divided up into smaller and smaller particles, these may have continued as closely contiguous in space as are the molecules of spheral atmospheres. In such a case they may present the conditions of excessive rarity so far as weight of matter is concerned; of close contiguity of particles, sufficient to permit the exercise of attractive energy; of great compression, through their vigour of centrifugal motion, and of intense elastic resistance to compression. These are the conditions necessary for the transfer of the radiations of light and heat. In these radiatures motion is conveyed through space by transfer of vibratory motions, not of impacts. The vibrating particle swings between lateral chains of attraction, and causes a like transverse swing in successive particles with which it is attractively connected. Greater energy here causes only greater width of vibration, not greater rapidity of transfer. The latter depends only on the elasticity of the matter concerned. Impact transfer of motion, on the contrary, must differ in speed with every difference in vigour. It is transferred by the motions of what we know as local heat, similar to the incessantly varied heat motions of gaseous matter. As the particles are unvarying in weight, increased momentum can be gained only by increased rapidity of motion, and the lines of motion thus transferred through space vary in speed with every variation in vigour. Every motion, of every particle of matter, is really a minute portion of an orbit, which represents that of a falling body, of a planet, or of a comet, according to its rapidity. Though the momentum affects successive particles of matter the orbit is continuous, except to the extent that it is varied by perturbations through attraction and impact.

Wherever any influence aids a translation of interspheral matter—causes a wind to blow through space—the lines of motion continue to be conveyed by the same particles. The orbital motions of the spheres are such winds through space; minor aggregations of moving matter may enter the atmosphere of the sun or other globes. But no atmosphere can become permanently increased in this manner; such masses, checked by friction, must yield motion, which flows outward. The centrifugal energy of the molecules of the external atmosphere is thereby increased, and the gain of matter must be balanced by an equal escape of matter at that critical atmospheric limit where centrifugal and centripetal energies are in balance. But any such fall of interspheral matter must aid the radiant emissions of the sun. Its loss of proper motion, its high degree of absolute heat, its increased temperature through condensation, and its consequent radiation, would make it a source of solar heat. Any such cometary matter must form part of "The Fuel of the Sun."

Philadelphia, U.S. CHARLES MORRIS

THE INSTITUTION OF MECHANICAL ENGINEERS

THE Annual General Meeting of this Institution was held on January 25 and 26, at the Institution of Civil Engineers, Great George Street; and the papers read were of unusual interest, from a scientific point of view, for a society whose aims are so distinctly practical. As it was pointed out by the president, Mr. Westmacott, three out of the five papers on the list were contributed by professors of science, and dealt with aspects more or less theoretical of the subjects treated upon. This forms, in fact, an additional instance of the way in which the old barriers between theory and practice are breaking down, and it is everywhere becoming recognised that neither can flourish without the aid of the other.

The first two papers, though quite independent, were both evolved, as it were, out of the same subject, namely, the research which the Institution has for some time been carrying on into the properties of hardened and unhardened steel. The first of these is an *interim* report by Prof. Abel, C.B., F.R.S., on the present stage of his experiments relating to the condition in which carbon exists in steel. Preliminary trials had shown that the treatment of steel and iron by a chromic acid solution (produced by mixing a solution of potassium bichromate, saturated in the cold, with one-twentieth of its volume of pure concentrated sulphuric acid) gave great promise of success in detecting the chemical differences existing in the same steel, according to the treatment to which it has been subjected. When cold—rolled, and annealed steel was thus treated, it yielded considerable amounts of an insoluble residue, consisting of black spangly particles, strongly attracted by the magnet, and presenting the characteristics of a true carbide, to which was assigned provisionally the formula Fe_6C_5. With hardened steel, on the other hand, but a small quantity of such particles were obtained, mixed with a lighter sediment; and the total residue contained only about one-sixth the carbon in the original steel, whereas in the annealed samples nearly all the original carbon was detected in the residue. The theory to which this points clearly is that in soft steel the carbon exists in a state of chemical combination, forming a carbide which is disseminated as a separate body through the mass of the iron; but that in hard steel this combination is dissolved, and the carbon exists in its pure form, either merely in mechanical admixture, as in the case of grey cast-iron, or in that peculiar and not very well understood form of association which metallurgists term an alloy. It would follow that the process of tempering, or rapid cooling, does not leave time for the complete formation of the carbide, and that in tempered steel all or some of the carbon still survives in its free or alloyed condition.

The fresh experiments described by Prof. Abel give, on the whole, great support to this theory. Four preparations were made of steel dissolved in chromic acid solution made as above, but of different degrees of strength. In the last only, where the strength was very high, were the results different, showing that the carbide had not been able to resist the oxidising effects of the solution. In the others, a considerable deposit was found, which, after being kept for several days, first in the original and afterwards in a fresh solution, was washed and dried, and then analysed. Another portion of the same was treated with chlorhydric acid, in order to ascertain what proportion would be converted into hydrocarbon. When this proportion was deducted from the whole, the remainder showed a most remarkable uniformity of composition, the percentages of carbon in three experiments being 5·93, 5·94, and 6·00, respectively. It seems evident that we have here a definite compound, to which Prof. Abel gives the formula Fe_3C. The deviations from this exact composition he accounts for by the presence of a certain amount of water, indicating that a carbo-hydrate had been formed, probably as a result of the action on the carbide first separated.

Prof. Hughes's paper, which was illustrated by a series of very striking and elegant experiments, performed with the simplest apparatus, may be considered, in its result, as the complement of Prof. Abel's. The latter goes to show that in soft steel the carbon is present as a chemical compound, which is dissolved by hardening; the former, that in hard steel the carbon is present as an alloy, varying with the temper. Between the two, we seem to reach the threshold of a complete theory. They approach the subject, however, from different sides, Prof. Hughes's work being purely electrical, Prof. Abel's purely chemical; and this makes their convergence the more important. Finding that the induction balance was equally sensitive to molecular and to chemical changes, in the metals tested, Prof. Hughes set himself to devise an instrument by which to examine the former class of phenomena by themselves. A wire forming the core of an ordinary magnetic coil, and capable of being shifted, twisted, &c., as desired, supplies what he requires. The coil is joined to a galvanometer, or, better still, to a telephone; the wire is joined to a battery, and currents are sent through it. So long as the wire is at right angles to the coil, no effect is produced; but if we set it at an angle to the coil, sounds are instantly heard, betokening the presence of induced currents in the coil. This is the ordinary effect of electro-magnetic induction, as discovered by Faraday. Now, instead of shifting the wire, let us give it a slight twist, say of 40°: the sounds are instantly heard as before, and we detect induced currents, which are positive for right-hand torsion, negative for left-hand torsion. Prof. Hughes's explanation of this is that the molecules of the wire, which he regards as so many separate magnets, have been given a twist round the axis, and thus set at an angle to the coil, just as the whole wire was by the shifting in the first case. Now let us twist the wire still further, even to several complete turns. No greater strength of current is observed, showing that the angle once given to the molecules is not exceeded, and that the subsequent torsion is of the wire as a whole. Approach to the wire, thus twisted, one pole of a natural magnet, laid parallel to the wire: the sounds cease, indicating that the magnetism has spun the molecules back again into their original directions. Approach the magnet at right angles to the wire: the current returns to zero while it is still two inches distant, and when it is in contact there is a reversed current, which is then at its maximum. Lastly, removing the magnet, untwist the wire by some 40°: the current returns to zero, showing that the molecular torsion has disappeared, while the molar torsion remains almost the same as before.

In all these effects the wire has been supposed to be of soft iron: a remarkable difference appears when we turn to tempered steel. For we now fail to detect more than slight traces of molecular disturbance or rotation, no matter how many turns we give to the wire. Thus, whereas in the iron we appear to have great molecular freedom, with steel we have almost complete molecular rigidity. But this molecular rigidity is found to obtain also in all alloys of steel which have been tested—*e.g.* magnetic oxide, iron and sulphur, iron and tungsten, &c. Hence we draw the conclusion that tempered steel is likewise an alloy, the associated elements in this case being, of course, iron and carbon.

The above is the essential part of this striking paper; but the same idea of molecular freedom and rigidity was illustrated by other examples. Thus, if a tube nearly filled with iron filings be magnetised, the magnetism, though permanent so long as the tube is still, is removed in an instant by shaking, or even by turning the tube gently, so that the filings roll over each other. If, however, we pour in any viscous liquid, the magnetism can indeed be imparted, but it cannot be mechanically removed: the filings are no longer able to revolve back into their former positions. Again, we may magnetise an iron and a steel wire to the same degree, and then give each a slight pull to set it in vibration; it will be found that almost the whole of the magnetism has disappeared in the iron, while it is scarcely affected in the steel. By such illustrations the remarkable physical differences thus shown to exist between iron and steel were brought very clearly home to the audience; and whether they accepted or not the theoretical explanation, they could not fail to recognise the suggestive and practical character of the facts thus put before them.

The third paper on the list, by Mr. Chas. Cochrane, was a sequel to one read by the same author at the Leeds meeting of the Institution, and dealt with Blast-Furnace working, with special reference to the analysis of the escaping gases. It was laid down at the outset that economy of fuel in blast-furnaces is governed by three conditions: (1) the temperature of the blast; (2) that of the escaping gases; (3) the quantity of carbon which can be maintained in the condition, once attained, of carbonic acid, instead of being re-transformed into carbonic oxide by absorption of carbon in the fuel. On the first two of these heads there is, of course, nothing new to be said; but they were illustrated by elaborate and valuable tables, giving, in units of fuel (C burnt to CO), (*a*) the heat carried *in* by blast of a given weight and temperature, (*b*) the heat carried *out* by escaping gases of given weight and temperature. The third is dwelt on at some length; and tables are given, showing, for any given consumption of C per ton of pig, the ratio of CO_2 to CO in the escaping gases, first when all the CO_2, once formed, is retained in that condition; and afterwards when ½ cwt., 1 cwt., 1½ cwt., &c., are afterwards reconverted into CO, or, as the author terms it, when a *transfer* of ½ cwt., 1 cwt., &c., of C has taken place. From this is deduced the conclusion that the mere knowledge of the ratio of CO_2 to CO in the escaping gases, as given by analysis, is useless to indicate what is really going on in the furnace; because the same ratio may appertain to any different conditions, according to the amount of the transfer which has taken place, from CO_2 back to CO. If, however, the consumption of carbon per ton of pig-iron has been at the same time ascertained, then we are at once able to refer the case to its proper position; and the knowledge of the ratio between the two cases enables us at once to see what amount of transfer has been going on, and what prospect there is of effecting an improvement. It was further pointed out that the main causes of this injurious re-conversion of CO_2 into CO were (1) the fact that the limestone, used as flux, contains a proportion of CO_2, which can only be evolved at a red heat, and therefore in contact with red-hot coke, which immediately gives up some of its C to the evolved gases; (2) the fact that the ore, especially in the larger pieces, does not get completely de-oxidised until it reaches the red-hot region, where the CO ascending in the furnace first unites with the oxygen in the ore to form CO_2, and then absorbs another equivalent of C from the coke, so returning again to the condition of CO. It is therefore suggested that both these sources of evil might be removed if (1) the limestone were *calcined* before entering the furnace, so as to have already parted with its oxygen, (2) the ironstone were broken up into pieces small enough to insure their decomposition in the higher parts of the furnace. Another means of accomplishing the latter result was to increase still further, if necessary, the height of furnaces. A sanguine estimate was made of the economy that would attend the application of these two devices, which it was expected might reach over 3 cwt. of coke per ton of pig-iron made.

The value to ironmasters of the elaborate tables annexed to the paper, and of the mode in which the problem of blast-furnace economy is presented, cannot but be very great; but grave doubts were expressed in the discussion, by Mr. I. Lowthian Bell, F.R.S., whether the practical results would answer the author's expectations. As regards the use of calcined limestone, in particular, it was stated that it had already been tried, without effecting any economy, at least in large furnaces; the suggested reason being that the calcined lime, as soon as charged, re-absorbed CO_2 from the escaping gases, and that although heat was no doubt disengaged in the process, yet this was too near the throat of the furnace to have any serious effect. Moreover it is to be remembered that the previous calcining of the limestone must itself require fuel, the amount of which must be deducted from any apparent gain due to the absence of CO_2 in the lime within the furnace.

The fourth and last paper which was read (one on Screw Shafts, by Prof. Greenhill, of Woolwich, being postponed for want of time) was by a Swiss engineer, Herr Wendelstein of Lucerne, and gave a good and clear account of the mechanical arrangements connected with the construction of the great tunnel under the St. Gothard, the longest in the world. These are beyond our scope; and the important questions of temperature and ventilation, though just touched upon, were reserved for a future communication, which will also deal with the railway approaches. It may be mentioned that the observations of Dr. Stapff, the official geologist at the St. Gothard, were stated to give as the rate of increase of heat in that locality, 2° C. per 100 metres depth (1·1 Fahr. per 100 feet); and that, if this figure be applied to the Simplon tunnel, as at present proposed to be made just above Brieg, the heat to be dealt with would reach the very high figure of 47° C., or 116° Fahr.

THE QUARTERLY JOURNAL OF MICROSCOPICAL SCIENCE

THE January number of this well-known scientific periodical appears in so new a form as to call for special notice. Under the editorship of Prof. R. Lankester it has long since attained a very high standpoint among the high-class journals of Europe, but it wanted a little in its general get-up to bring it to the very highest level of these, in such details as size, paper, and illustrations. No doubt such details are not to be taken for more than they are worth, and of late years it will be conceded by all those whose opinion is worth having that the value of the contents of the quarterly numbers of the journal left it in a great measure independent of mere typographical superfluities. Still it is very pleasant to find this eighty-ninth number of the New Series so splendidly got up—its paper and type are such as we might expect to find associated with some special monograph; while the increased size (royal octavo) enables the illustrations to be given on a scale quite up to anything we have been accustomed to in the very first of the German and French journals. Let us hope that the enterprise of both Editor and Publisher will meet with sufficient reward to enable them to continue to show what can be done in the way of a scientific journal in these countries.

That the contents are worthy of such a shrine is beyond dispute. Never has Prof. Lankester issued a more important number of his journal, as a mere enumeration of the contents as follows will show. Dr. E. Klein, On the relation of Pathogenic to Septic Bacteria, as illustrated by Anthrax cultivations. This paper relates to a most serious question: it is a model of fair and judicious criticism of the labours of others, and of skill in experimental details. Our space forbids an allusion to its conclusions; but every medical man of any culture should read and re-read this memoir. Somewhere Claude Bernard has said, "Nowadays every medical man *thinks* himself a physiologist." Such would profit by a perusal of this paper if they are able to understand its full significance.—E. B. Poulton, M.A., On the tongue of *Perameles nasuta*, with some suggestions as to the origin of taste-bulbs (Plate 1).—Dr. L. Elsberg, Plant-cells and living matter.—F. O. Bower, M.A., Plasmolysis and its bearing upon the relations of cell-wall and protoplasm (Plate 8).—Prof. A. P. Thomas, The life-history of the Liver Fluke (*Fasciola hepatica*), (Plates 2 and 3); a most elaborate, complete, and beautifully illustrated monograph.—W. F. R. Weldon, B.A., Note on the early development of *Lacerta muralis* (Plates 4-6).—R. V. Willemoes-Suhm (the late), On a crustaceous larva, at one time supposed to be the larva of Limulus (Plate 7).—A. G. Bourne, B.Sc., On Haplobranchus, a new genus of Capitobranchiate annelids (Plate 9).—E. Ray Lankester, M.A., and A. G. Bourne, B.Sc., The minute structure of the lateral and the central eyes of Scorpio and of Limulus (Plates 10–12). The authors find, in the essential agreement of the central eyes of Limulus with those of Scorpions, another important detail, which confirms the opinion of Prof. Lankester, that the Scorpions and King Crabs are closely-allied representatives of one class, the Arachnida.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Dr. E. B. Tylor's first lecture on Anthropology will take place on Thursday, February 15, at 2.30 p.m., at the large lecture room at the University Museum, not on Wednesday, the 14th, as previously announced. The second lecture will be given at the same hour and place on Wednesday, the 21st.

The voting for "elected members" of the new Boards of Faculties took place last Saturday. The electors were the Members of Convocation, authorised by the Colleges to teach in the subjects of the various faculties. Mathematics and Natural Science are included in one faculty—that of Natural Science. As there are more college lecturers in Mathematics than in Natural Science, it was resolved at a preliminary meeting of the electors to choose five mathematicians and five teachers of Natural Science to occupy the ten places which were to be filled up. Ten names were then agreed upon, but at the formal meeting another mathematician and another scientist were proposed so that it became necessary to vote. The ten names before agreed upon were those chosen, the mathematicians naturally heading the list. They are Messrs. W. Esson, F.R.S., Merton, C. J. Faulkner, University, C. Leudesdorf, Pembroke, E. B. Elliott, Queen's, and J. W. Russell, Balliol, as representatives of mathematics; Messrs. R. E. Baynes, Christ Church, as a representative of Physics; Messrs. J. Watts, Merton, and A. G. Vernon Harcourt, F.R.S., Christ Church, as representatives of Chemistry; and Messrs. E. B. Poulton, Keble, and W. H. Jackson, New, as representatives of Biology.

The Professors of Mathematics and Natural Science are *ex officio* members of the Board.

The Examiners for the Radcliffe Travelling Fellowship give notice that the examination will commence at the Museum on February 13.

The Examiners for the Burdett-Coutts Geological Scholarship give notice that the examination will commence on February 12.

The serious illness of Prof. Henry J. S. Smith is causing much anxiety in the University.

CAMBRIDGE.—The syndicate appointed to frame regulations on the subject of the degree of Doctor in Science or Letters report that they think it important that precautions should be taken to secure that whenever a degree in Science or Letters is granted, the provisions of the statute requiring that the candidate shall have given proof of distinction by some original contribution to the advancement of science or learning have been *bonâ fide* complied with; but they think it undesirable to require from candidates any additional examination or special act or exercise. Considering that it is desirable to encourage the more distinguished graduates to turn their thoughts towards original work at a comparatively early age, and that it is not uncommon for able men to be elected Fellows of the Royal Society at the age of thirty or thereabouts, the Syndicate are of opinion that five years will be a sufficient interval between the degrees of M.A. and D.S. or D.L. The Syndicate think that it is to be wished that some of the older graduates in Arts should proceed to their new degrees. They think that the probability of this would be increased if the seniority of all those who so proceed within a limited time were reserved to them. The Syndicate have carefully considered the difficulty which may arise from the ambiguity of the term "Science." They are of opinion that no regulations can be laid down drawing a clear line between claims for a degree in Science and claims for a degree in Letters.

The Syndicate have drawn up a code of regulations to the effect of the above. The fee to be payable for the degree of either Doctor of Science or Letters to be 20*l*.

Candidates' applications are to be sent to the Chairman of the Special Board of Studies with which their original contribution is most closely-connected, specifying the printed contribution or contributions for which the degree is sought. The application is to be considered by a committee, and the contributions reported on by at least two persons, who may be members of the committee or not. If the Special Board report in favour of the candidate, the General Board of Studies is to have a voice in the matter, and if they approve, the Vice-Chancellor is to publish the name as approved.

The following are nominated Electors to the Professorships named:—

Chemistry.—Professors A. W. Williamson, Lord Rayleigh, Dewar, Frankland; Doctors Phear (Emmanuel College), and Ferrers (Caius), Prof. Fuller (of Aberdeen), and Mr. Coutts Trotter.

Jacksonian of Natural Philosophy.—Professors A. W. Williamson, G. G. Stokes, G. D. Liveing, Dr. Hugo Muller, F.R.S., Dr. M. Foster, Mr. P. T. Main, Prof. Fuller (Aberdeen), and Mr. Coutts Trotter.

Cavendish of Experimental Physics.—Sir W. R. Grove, Prof. G. G. Stokes, G. H. Darwin, Sir W. Thomson, R. B. Clifton, G. D. Liveing, James Stuart, and Mr. W. D. Niven.

The Smith's Mathematical Prizes are awarded to Messrs. Welsh, Jesus College (1), and Turner, Trinity College (2).

The Balfour Fund now amounts to about 4130*l*., in addition to the 4000*l*. contributed by his relatives and by Dr. Foster.

DURING last year there entered at the University of Upsala 330 students. In 1881, the number was 312; in 1880, 263; in 1879, 259; in 1878, 243.

SCIENTIFIC SERIALS

Journal de Physique, January, 1883.—On the metallic gratings of Mr. Rowland, by M. Mascart.—*Résumé* of experiments made at the Exhibition of Electricity, on magneto- and dynamo-electric machines, and on electric lights, by M. Potier.—On electric shadows and on various connected phenomena, by M. Righi.—On the surface of the wave, by M. Doyen.—Demonstration of the principle of Archimedes for bodies immersed in various gases, by M. Terquem.

Atti della R. Accademia dei Lincei. Transunti, Vol. xvii., fasc. I.—On attenuation of the carbuncular virus, by S. Perroncito.—On the tenacity of the carbuncular virus in its forms of spores, or of *Bacillus anthracis*, by the same —On the presence of yttrium in the sphene of syenite of Biellese, by S. Cossa.—New Sicilian fungi, by Srs. Passerini and Beltiami.—On some unpublished propositions of Fermat, by M. Henry.—On the action of chloride of cyanogen on the potassic compound of pyrrol, by Srs. Ciamician, and Dennstedt.

Vol. xvii. Fasc. 2.—On a class of triple systems of orthogonal surface, by S. Bianchi.—Observations of the Venus transit at the Observatory of Campidoglio, by S. Respighi.—Reports on prize competitions.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xv. Fasc. xvii.—On compensatory hypertrophy of the kidneys, by S. Golgi.—On drunkenness in Milan (continued), by S. Verga.

Fasc. xix.—Prof. Giacci's "Fundamental theorem in the theory of the canonical equations of motion," by S. Morera.—On drunkenness in Milan (continued), by S. Verga.—On olivil and some of its transformations, by Srs. Korner and Carnelutti.—Congenital pachydactylia from a psychical impression in the mother, by S. Scarenzio.—Observation of the transit of Venus at the Royal Observatory of Brera, on December 6, 1881, by S. Schiaparelli.—Bacteria of anthrax in the fœtus of a heifer that died of the disease, by S. Sangalli. Discussion with S. Golgi.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 1.—"On the Affinities of Thylacoleo," by Prof. Owen, C.B., F.R.S., &c.—Since the appearance of Part IV. of the "Fossil Mammals of Australia" in the *Philosophical Transactions* for 1871, the author has omitted no opportunity of promoting the acquisition of additional evidences. The application of a grant by the Legislature of New South Wales, in aid of further exploration of the Limestone Caverns in Wellington Valley, having been confided to Ed. P. Ramsay, F.L.S., the results have furnished the author with additional evidences, including those which form the subject of the present communication. After a brief exposition of the state of the question at the date of the previous paper, a description is given of the complete dentition of the upper and lower jaws of a mature marsupial lion. This is followed by descriptions of the antibrachial bones and ungual phalanges of the same extinct animal, the characters of those parts of the skeleton being compared with the same parts in feline mammals and in the existing kinds of diprotodont marsupials. The paper concludes with a description of an entire mandible; and the conclusions to be drawn from the shape and position of the articular condyles, which harmonise with those deducible from fragmentary fossils previously described, go nearly to complete the reconstruction of what the author deems to be the most extraordinary of the extinct pouched quadrupeds of Australia.

The paper was accompanied by drawings of the natural size of the fossils described.

In the subsequent discussion the author remarked on the correspondence of spælean phenomena, the proportion of the remains of the old British lion in bone caves of this country being paralleled by that of the Australian carnivore in the antipodean caves. They were the retreat of the destroyer in both localities; and the fragmentary, gnawed condition of the remains of the prey, with usual immaturity of the captured kangaroos of great size, the *Diprotodon australis*, *e.g.*, afforded an instructive analogy.

"Preliminary Note on a Theory of Magnetism based upon New Experimental Researches." By Prof. D. E. Hughes, F.R.S.

In the year 1879 (*Proc. Roy. Soc.*, vol. xxix. p. 56, 1879) I communicated to the Royal Society a paper "On an Induction Currents Balance and Experimental Researches made therewith." I continued my researches into the molecular construction of metallic bodies, and communicated the results then obtained in three separate papers (*Proc. Roy. Soc.*, vol. xxxi. p. 525; vol. xxxii. pp. 25, 213, 1881) bearing upon molecular magnetism.

To investigate the molecular construction of magnets, required again special forms of apparatus, and I have since been engaged upon these, and the researches which they have enabled me to follow.

From numerous researches I have gradually formed a theory of magnetism entirely based upon experimental results, and these have led me to the following conclusions:—

1. That each molecule of a piece of iron, steel, or other magnetic metal is a separate and independent magnet, having its two poles and distribution of magnetic polarity exactly the same as its total evident magnetism when noticed upon a steel bar-magnet.

2. That each molecule, or its polarity, can be rotated in either direction upon its axis by torsion, stress, or by physical forces, such as magnetism and electricity.

3. That the inherent polarity or magnetism of each molecule is a constant quantity like gravity; that it can neither be augmented nor destroyed.

4. That when we have external neutrality, or no apparent magnetism, the molecules, or their polarities, arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction.

5. That when magnetism becomes evident, the molecules or their polarities have all rotated symmetrically in a given direction, producing a north pole if rotated in this direction as regards the piece of steel, or a south pole if rotated in the opposite direction. Also, that in evident magnetism, we have still a symmetrical arrangement, but one whose circles of attraction are not completed except through an external armature joining both poles.

The experimental evidences of the above theory are extremely numerous, and appear so conclusive, that I have ventured upon formulating the results in the above theory.

I hope in a few weeks to bring before the Royal Society the experimental evidence which has led me to the conclusions I have named; conclusions which have not been arrived at hastily, but from a long series of research upon the molecular construction of magnetism now extending over several years.

Linnean Society, January 18.—Sir John Lubbock, Bart., F.R.S., president, in the chair.—E. A. L. Batters, A. J Burrows, E. F. Cooper, Prof. J. A. Harker, and G. Lewis, were elected Fellows of the Society.—Mr. H. Grooves called attention to a specimen of *Ranunculus ophioglossifolius* obtained in Hampshire, and therefore new to Britain.—There was exhibited, on behalf of Mr. Jas. Romanis, a live specimen of *Pieris Rapæ*, which had been found fluttering on the window of his house a few days previously.—A paper was read on the fall of branchlets in the aspen (*Populus tremula*) by Samuel G. Shattock. He shows that in this tree and some few others—in contradiction to the majority of exogenous trees—a process takes place termed "cladoptopsis" by the Rev. M. J. Berkeley many years ago. In the small branchlets only disarticulation is effected by a swollen ring of corky tissue at the base, somewhat as in the ordinary fall of leaves.—Mr. A. G. Bourne gave a contribution on the anatomy of Polynoina, pointing out that the *Polynoë grubiana*, very common in the Mediterranean, is only a variety of the *P. clava*, Montague, of our own coasts. The latter itself has certain constant characteristics, and others much more variable.—Prof. P. Martin Duncan read his observations on the Madreporaria, fam. Fungidæ, with special reference to the hard structures. Edwards and Haime described the synapticula as constituting an essential family structure, and also the absence of endothecal dissepiments. Dr. Duncan describes that the ridges of the continuous synapticula with canals between them is limited by solid and also perforate septa, and he delineates the structures. The synapticula are shown to have no relation to the ornamentation on the ridges of the septa. The basal wall is shown to be of synapticular origin, and the foramina in it to relate to the growth of these binding structures.

Physical Society, January 27.—Prof. Clifton, president, in the chair.—New Member, Mr. Hugh E. Harrison.—Prof. G. Carey Foster read a paper on the determination of the ohm, in which he described the various methods which have been used and proposed in determining the B.A. unit of resistance. He also described a method of his own, proposed in 1874, and recently tried with good results. The method consists in balancing the E.M.F. set up in a coil of wire by spinning it in the earth's magnetic field, against the E.M.F. of a battery or other electromotor, in a wire whose resistance is to be determined. The two opposing circuits through this wire, R, are composed, the first of the spinning coil and a zero-galvanoscope, and the second of a battery and an absolute galvanometer; these two circuits meeting at the ends of the wire R. The late Mr. Hockin and Prof. Foster find that the best conditions obtain when the resistance of the absolute galvanometer r is equal to R; the resistance of the zero galvanoscope r_2 equal to $\frac{R}{2} + r_3$, and the resistance of the spinning cord, r_3, many times the battery-resistance, which should be so low as to be practically negligible. The E.M.F. of the battery should be double that of the spinning coil. Many other conditions had to be attended to, as explained by Prof. Foster. With this method, and using a thermo-electric battery giving an E.M.F. of 2·2 volts, the coil was spun at about 1800 revolutions per minute; r was 63 ohms, r_2 was 135, r_3 was 50, and R was 73 in one, and of 80 in another experiment. R was made up by coils on a resistance box. The ohm was determined by two trials to be 1·003 and ·999. This general result is so satisfactory that the experiments will be continued with extra precautions. Mr. Glazebrook called attention to the remarkable agreement between the results of Lord Rayleigh's determinations and his own independent ones. Lord Rayleigh's figures are for the unit, ·9893, ·9865, ·9868, and Mr. Glazebrook's is ·9866, or the mean of Lord Rayleigh's results. He also announced that the Clarendon Laboratory, Cambridge, would soon be in a position to test and certify any resistance coils sent there.—Mr. Walter Baily then read a paper on the spectra formed by curved diffraction gratings. In a diffraction grating ruled on a portion of a cylinder, if r is the distance of a point from the centre of the grating, and θ the angle which a line to the point makes with the perpendicular from the centre of the grating, c the radius of curvature of the grating, and d an arbitrary constant, a series of curves may be drawn in the plane perpendicular to the lines of the grating having as the general equation

$$r^{-1} \cos^2 \theta = c^{-1} \cos \theta + d^{-1}.$$

If a source of light is placed on a point on one of these, curves the foci of the diffracted light lie on the same curve. The curve consists of two loops, one of which gives the spectra of transmitted and the other those of refracted light. When d is infinite, these curves coincide in a circle, the properties of which have been so used by Prof. Rowland in the construction of his diffraction spectroscope. The paper also describes how the position of the spectra on the curves can be determined for any position of the source of light.

Geological Society, January 24.—J. Gwyn Jeffreys, vice-president, in the chair.—Walter Raleigh Browne, Thomas Charles Maggs, Lieut.-Col. William Alexander Ross, and Cecil Carus Wilson, were elected Fellows of the Society.—The following communications were read:—On *Streptelasma Ræmeri*, sp. nov., from the Wenlock shale, by Prof. P. Martin Duncan, F.R.S., V.P.G.S.—On *Cyathophyllum Fletcheri*, Edw. and H., sp., by Prof. P. Martin Duncan, F.R.S., V.P.G S.—On the fossil Madreporia of the Great Oolite of the counties of Gloucester and Oxford, by Robert F. Tomes, F.G.S.

Institution of Civil Engineers, January 30.—Mr. Brunlees, president, in the chair.—The paper read was on "Mild Steel for the Fire-boxes of Locomotive Engines in the United States of America," by Mr. John Fernie, M.Inst.C.E.

SYDNEY

Linnean Society of New South Wales, November 29, 1882.—Dr. James C. Cox, president in the chair.—The following papers were read:—"Description of two new birds of Queensland," by Charles W. De Vis, B.A.—One of these birds —*Prionodura Newtoniana* constitutes a new genus and species of the Family Paradiseidæ. It is described from a unique specimen taken in Tully River scrubs, Rockingham Bay. The other bird described—*Cracticus rufescens* came from the same locality.—"Fungi aliquot Australiæ Orientalis," by the Rev. Carl Kalchbrenner.—The following new species were described *Agaricus megalotheles*, *Agaricus Kirtoni*, *A. peltastes*, and *Scleroderma pileolatum*.—The Rev. J. E. Tenison-Woods, vice-president, read the fifth part of his "Botanical Notes on Queensland."—This paper consisted of a description of the "Brigalow" scrubs, which consist mainly of *Acacia harpophylla* (F.v.M.) instead of *A. excelsa* as usually stated. The brigalow forms thickets of from thirty to eighty feet in height, amongst which a peculiar flora occurs. A list of those collected by the author was given at the end of the paper.—"Contribution to a knowledge of the Fishes of New Guinea" No. 3, by William Macleay, F.L.S., &c.—In this paper Mr. Macleay completes the list of the Fishes sent by Mr. Goldie from Port Moresby, bringing the number up to of species 274. The new species described in the present paper are:—*Platyglossus guttulatus*, *Coris cyanea*, *Pseudoscarus Goldiei*, *Pseudoscarus frontalis*, *Pseudoscarus papuensis*, *Pseudoscarus zonatus*, *Pseudoscarus labiosus*, *Pseudoscarus Moresbyensis*, *Monacanthus nigricans*, *Monacanthus fuliginosus*, *Trygon granulata*, and *Tænura atra*.—"Notes on the Geology of the Western Coal Fields, No. 2, by Prof. Stephens, M.A.—In this paper Prof. Stephens proceeds to an examination of the Wallerawang, Marangeroo and Capertee conglomerates which leads him directly to the conclusion that the continent off whose shores the upper marine carboniferous beds were deposited, was a system of high mountain ranges, snow-capped, and under erosion by glaciers which descended to near the level of the sea. It was shown further that all the subsequent formations were of shore or river formation, in plains skirting the mountains, or in valleys penetrating their recesses, and that these were all fresh water deposits, excepting the coal seams themselves, which were subaerial; and that the most recent sedimentary formations in that district was the Hawkesbury Sandstone, also lacustrine in origin, and due like the underlying strata to a continued rise of the lake waters upon the land.—"Note on an Australian species of Phoronis," by William A. Haswell, M.A., B.Sc.—"Note on a curious instance of Symbiosis," by William A. Haswell, M.A., B.Sc.—"Note on the segmental organs of *Aphrodita*," by William A. Haswell, M.A., B.Sc.

BERLIN

Physiological Society, December 29, 1882.—Prof. du Bois Reymond in the chair.—Dr. Pohl-Pincus spoke about the effect of weak local stimulations of the heart, and about the effect of vagus-stimulation upon the heart.—Prof. Quincke of Kiel, who was present as a visitor, spoke upon the physiological part of the results of experiments and observations which he had made upon the life-history of the red blood corpuscles. It is a well-known fact that large cells with numerous pigment-granules occur in the spleen and in the marrow of bones. These cells, as the micro-chemical reaction teaches us, contain a great deal of iron in combination with albumen. The iron-reaction of the spleen and bone-marrow is more pronounced than the number of pigment-cells can explain; and hence Prof. Quincke hypothesizes the presence in both structures of a colourless iron-albumen, which is, on the one hand, the product of the destruction of red corpuscles, and on the other hand forms the material out of which the new red corpuscles are developed. Both these circumstances were verified experimentally; when by frequent transfusions of equal quantities of blood into an animal, the number of the red blood corpuscles was considerably augmented, and, by this means, the destruction of red corpuscles likewise increased, the number of the pigment-cells and the amount of iron-albumen in the spleen and marrow of the bones was also very much increased, and there was present in the capillaries of the liver a considerable quantity of white blood-corpuscles with iron-albumen, which, under normal circumstances are only found in this organ in very small numbers. When, on the other hand, the number of an animal's red blood-corpuscles had been diminished by repeated bleedings, both the number of pigment cells and the amount of iron-albumen in the spleen and marrow was found after a few days to be considerably diminished and reduced to a minimum. While a part of the iron from the disintegrating red blood corpuscles describes a circle forming a reserve for the newly-forming blood-corpuscles, another portion is eliminated from the blood through the urine and bile. In the experiments, in which the destruction of red corpuscles was increased as a consequence of transfusions, it was possible to demonstrate the intermediate stages in the process of the abnormally-large elimination of the iron, as both the liver-cells and the kidney-epithelium gave a quite distinct iron-reaction.—Dr. Schiffer then read two preliminary communications. One of these was upon the poisonous properties of the mammalian urine. When the urine of either carnivorous or herbivorous mammals was injected under the skin of a frog, symptoms of poisoning manifested themselves, to which the frog soon succumbed. Rabbits also exhibited symptoms of poisoning after subcutaneous injection of evaporated urine, which had been deprived of the poisonous potash-salts. Dr. Schiffer is still engaged in the investigation of the isolation of the poison.—The second communication was upon his experiments with curare. The striking inoperativeness of this violent poison, when introduced into the stomach cannot be due, as has up to the present been almost universally accepted, to the absorbed poison being quickly eliminated by the kidney, because Dr. Schiffer's experiments showed that the elimination of this substance through the urine is complete, although very slow, so that the animal, if it would absorb the poison, would have had to succumb long before. When Dr. Schiffer introduced a very large dose—about 2 grms.—of curare, into a stomach which he had ligatured at the pyloric orifice, the animal died in about twenty-two hours, which was far too late for curare poisoning, and far too soon as a consequence of the ligature of the pylorus. When introduced into the small intestine, the continuity of which was interrupted above and below by a ligature, the curare was very quickly absorbed; when the small intestine was only occluded above, only a very little curare was absorbed, this absorption taking place slowly. The large intestine behaved like the small one. From the rectum out, curare was very quickly absorbed. Outside the body, curare diffused very well through a stomach wall. To sum up, the inoperativeness of curare when introduced into the stomach is as yet unexplained.

PARIS

Academy of Sciences, January 29.—M. Blanchard in the chair.—The death of M. Sedillot, Member in the Section of Medicine and Surgery, was announced.—Note on the observation of the transit of Venus, by M. Janssen. The conditions were very favourable at the fort du Chateau-Neuf, Oran. Special attention was given to the question as to presence of aqueous vapour in the atmosphere of Venus. This was not demonstrated. Afterwards M. Janssen spent a month at Mecheria, a military station on the high desert plateaux, with the same purpose. The air was so dry and clear that, *e.g.*, Jupiter's satellites could be seen with the naked eye. With very perfect spectroscopic apparatus applied under extremely good conditions, he is yet constrained to great reserve as to the presence of aqueous vapour in Venus's atmosphere. He studied mirage, photographed it several times, and finds its causes, in most cases, to be quite different from those commonly supposed.—On the mechanical and physical composition of the sun (continued), by M. Faye. This relates to spots.—Contributions to the history of the reactions between sulphur, carbon, their oxides, and their salts, by M. Berthelot. The results have a bearing on the reactions produced during explosion of powder.—On the morbid phenomena produced in rabbits by introduction of hydrate of chloral into the ear, by M. Vulpian. The most salient phenomenon is impetuous rotation of the animal on its longitudinal axis; which the author attributes to the inflammation produced in the cavities of the internal ear; to this inflammation, along with more or less broncho-pneumonia, the animals often succumbed. The brain and meninges were not affected, as in the experiment of M. Brown-Séquard, who poured chloroform into the ear. Though the disorders of motility are weakened in time, they are found still to persist in some degree, a month after operation.—Observations on the occasion of a Report of M. Leon Colm, on the mortality produced by typhoid fever in the French army, by M. Vulpian. This Report, by a committee of the Academy of Medicine, throws doubt on the value of M. Glenard's recent statistics (to show the efficacy of cold baths).—Note on the state of natural sciences and on anthropology in Brazil, by M. de Quatrefages. *Inter alia*, Brazil now devotes, on an average, 16 per cent. of her whole revenue to public education; in one of the twenty-one provinces (Goyaz), the proportion reaches 30 per cent. The National Museum in Rio, dating from 1817, has been wholly reorganised by Dom Pedro, and it is of great value. The Emperor is often present at the lectures there. The Museum has its *Archives*, and M. de Quatrefages indicates the contents of the first four volumes sent him; they reveal great scientific activity. The successful Brazilian Anthropological Exhibition held last year is to be followed by one for the entire Continent.—Note on the determination of the phosphoric acid in arable land, by M. de Gasparin. He describes an easy and rapid method. Arrangements were made in connection with a new annual prize of 1000 francs, provided by a widow, Mme. Francœur; it is to be given "to the author of discoveries or works useful to the progress of the mathematical sciences, pure or applied."—On wounds from fire-arms, called seton-wounds, by M. Guérin. These wounds always contain foreign bodies, from crushing of the tissues, and perhaps particles of cloth, &c.; and the conditions are adverse to immediate cicatrisation. The author adopts (with success)—(1) antiseptic washings with continuous currents, (2) pneumatic occlusion; simultaneously, alternately, or successively, as the case may be.—On a class of functions of two independent variables, by M. Picard.—On the algebraic integration of a class of linear equations, by M. Goursat.—On a theorem of M. Tchébychef, by M. Korkine.—Application of a method given by Legendre, by M. Lipschitz.—Observation of a magnetic storm at Cape Horn, by M. Mascart. This was on November 17 and 18 last. The principal perturbation was simultaneous with that at the Parc St. Maur.—Reply to a note by M. Marcel Deprez, by M. Lévy.—M. Deprez presented a translation of the official Report at the Munich Exhibition, on transport of force by dynamo-electric machines.—Reply to M. Lévy, by MM. Mercadier and Vaschy.—New experiment in electrolysis, by M. Semmola. In proof of the law that the quantity of liquid decomposed in a given time is proportional to the quantity of electricity which passes in that time, he uses a voltameter with three platina electrodes inserted equally apart at the bottom. The current coming by one is caused, by a commutator, to pass either by one of the others or by both.—Researches on the passages of alcoholic liquors through porous bodies (second note), by M. Gal. He investigates the influence of temperature, and nature of membrane, and the case in which the membrane is exclusively in contact with the liquid or with its vapour. An alcoholic liquid in contact with a membrane tends to diminish in degree, instead of concentrating, as Sœmmering affirmed, and as is everywhere taught; and it is the same with its vapour.—On the vapour of carbamide, by M. Isambert.—On sulphite of manganese, by M. Gorgeu.—On new ammonio-cobaltic combinations, by M. Maquenne.—On the crystalline form, specific heat, and atomicity of thorium, by M. Nilson. The crystals form a regular combination between the octahedron and the tetrahedron: the specific heat is 0·02757; the substance is quadrivalent.—On the mutual displacements of bases, &c. (continued), by M. Menschutkine.—Importance of zoological characters furnished by the upper lip in the *Syrphides* (Diptera), by M. Gazagnaire.—On the effects of respiration of air charged with petroleum vapour, by M. Poincaré. Dogs, rabbits, and guinea-pigs were experimented with. Respiration was increased in frequency and amplitude, heart-beats were retarded, (the shock was intensified); there was itchiness, sleepiness, and inappetence. Guinea-pigs alone succumbed, after one to two years.

CONTENTS

PAGE

Zoological Sketches. By George J. Romanes 333
The Gold Coast 335

Our Book Shelf:—

Newell's "Handbook of Vertebrate Dissection" 335
Williamson's "Ferns of Kentucky" 336

Letters to the Editor:—

Hovering of Birds.—Hubert Airy; David Cunningham; William Galloway; Dr. J. Rae, F.R.S.; C. S. Middlemiss; W. Larden 337
Science and Theology.—C. 357
Intelligence in Animals.—J. Birmingham 337
Electric Railways.—V. 338
The Channel Tunnel.—Prof. W. Boyd Dawkins, F.R.S. . . . 338
The Great Comet of 1882.—Thos. Wm. Backhouse 338
Meteor of November 17.—Rev. Stephen H. Saxby 338
The Sea Serpent.—William Barfoot; Prof. W. Steadman Aldis 338
Natural Enemies of Butterflies.—Henry Higgins 338
On the Graduation of Galvanometers for the Measurement of Currents and Potentials in Absolute Measure, IV. By Andrew Gray (*With Illustration*) 339
Norwegian Geodetical Operations 341
Scientific Heresies in China 342
Notes of Travel in Sardinia. By G. F. Rodwell 342
Mathematics in Scandinavia 343
The French Mission to Cape Horn (*With Illustration*) 344
Heating by Acetate of Soda (*With Illustrations*) 344
Notes 346

Our Astronomical Column:—

Denning's Comet 348
The Great Comet of 1882 349
Astronomical Telegrams 349
The Matter of Space. By Charles Morris 349
The Institution of Mechanical Engineers 351
The Quarterly Journal of Microscopical Science 353
University and Educational Intelligence 353
Scientific Serials 353
Societies and Academies 354

THURSDAY, FEBRUARY 15, 1883

THE TERTIARY HISTORY OF THE GRAND CAÑON DISTRICT

A Monograph by Capt. C. E. Dutton. Being Vol. II. of the Monographs of the United States Geological Survey. With Atlas. 4to. [Vol. I. is not yet published.] (Washington, 1882.)

In a handsome quarto volume, with a large atlas of maps and coloured views, the recently-constituted United States Geological Survey begins its series of memoirs descriptive of the geological structure and history of the country. Most appropriately the subject selected for illustration is at once the grandest and most unique feature in the geology of the United States, and to which indeed there is no parallel elsewhere in the world. Ever since the early Report by Ives and Newberry, in which the marvels of the Rio Colorado of the West were first made known, there has been a strong desire among geologists to learn more of that region, to have accurate measurements and careful drawings, and to be told authoritatively the details and the history of what they could not but admit to be the most stupendous example of river-erosion on the face of the globe.

Major Powell's bold descent of the river and the charming volume in which he described it threw much fresh light on the wonders of the cañons. But he had no opportunity of properly exploring the surrounding regions, though we looked forward to his return to the scene of his exploits and the consequent elaboration of another memoir discussing the whole problem of the origin and history of the geological features of that remarkable area. Pressure of other duties has prevented him from realising this hope. But, though unable himself to resume this task, he deserves our best thanks for having induced the late Director of the Survey, Mr. Clarence King, to intrust the detailed survey of the Grand Cañon to Capt. C. E. Dutton, who had already done excellent service among the high volcanic plateaux farther north. Capt. Dutton unites some of the highest qualities of a geological explorer. He is an excellent stratigrapher, a good petrographer, an enthusiast in the study of rock-sculpture, writes clearly and pleasantly, has a physical frame capable of carrying him triumphantly through any amount of physical fatigue, and is the happy possessor of a bright, cheerful nature, that must lighten the hardships of camp-life in the remote West both for himself and for his companions. We can well imagine how such a man, wandering among the lofty plateaux of Utah that had been assigned to him for exploration, should have cast many a longing gaze southward to that strange wild desert region of rocky platforms and winding *mesas*, through which the gorges of the Colorado and its tributaries have been sunk; how he should have been unable to resist the temptation to stray into that wonderland; and how he must in some measure have almost welcomed the blasts of early winter that drove him down from the survey of the plateaux, and allowed him to journey through the cañon country on his way back to the Mormon settlements and the nearest railroad.

When at last the task of actually exploring and describing that region was intrusted to him, he already possessed a general acquaintance with its character and with many of its details. A stranger who first finds the cañon scenery before him is so excited by its novelty and grandeur, that for a time he feels utterly bewildered. Only after his eye has in some measure recovered its power of grasping the broad effects, without being lost in the details, does he begin to realise what are the elements of this stupendous grandeur. But Capt. Dutton had gone through this preliminary training. He had been led to scrutinise the scenery in detail, to discover the relations of part to part, and to speculate upon the evolution of the whole. Yet no one can read his pages without feeling that this analytic process has in no way dulled his sense of the beauty and majesty of the scenery. His words glow with the light that floods those flaming precipices. The blue aërial perspective of chasm and cliff receding into the dim distance in the central gorge seems to rise before our eyes as we read. With no irreverent hand does he tear the mask off the face of Nature. Rather does he make us feel how deeply the mystery of the scene has entered into his soul, as he gently lifts the veil that we may see a little way within, even as far as he has himself been enabled to penetrate.

And this is the true spirit in which such scenery should be described and discussed. The man who could sit down and dissect these cañons in cold blood, and with as little emotion as he would show in cutting up a joint of beef, would be a creature not to be envied. Nowhere in this world does the scenery appeal so powerfully to the imagination. Among the Alps the rocks have been so stupendously crumpled that we may be pardonably at a loss to tell how far the outlines of a mountain are due to subterranean movements or to subsequent erosion. But among the western cañons there is no room for any such doubt. The rocks lie for thousands of square miles as flat as when they were laid down upon the floors of ancient seas and lakes, and their horizontal undisturbed beds may be followed by the eye, winding in and out from cliff to cliff, preserving the same breadth, colour, features, and serving as so many datum-lines from which to measure the amount of solid rock that has been removed from the gorges. In tracing back the origin of these landscapes, and seeking out the causes of their infinite variety of detail yet marvellous harmony of effect, the mind naturally compares them with the feeble illustrations of erosion with which alone we are usually personally familiar. Such a comparison, however, will almost suggest a doubt as to whether we ever before could have had any proper conception of what the power of running water actually is, so utterly beyond description is the impressiveness with which this power is now realised. Nor is one disposed to deny that nowhere else is the dominant influence of geological structure upon the ultimate contours developed by erosion so significantly displayed. On every part of the scenery the story of its origin is impressed in characters that cannot be mistaken. Yet these characters are on so colossal a scale that the dry prosaic language of ordinary geological description seems utterly incongruous when applied to them. It must be a difficult task to preserve the sober decorum of scientific treatment, and to convey at the same time an adequate impression of the infinite majesty of the subject.

Capt. Dutton may be congratulated on having accomplished this task with as large a measure of success as probably was achievable. Without entering into stratigraphical details he addresses himself to the problem of the origin and history of the erosion that has converted the level rock-platforms of the Colorado River into their present profoundly trenched condition. Sketching briefly but clearly the general geographical features of the region and their relation to the underlying geological structure, he presents the reader with a series of pictures of the various types of scenery. He shows how everywhere the evidence arises of vast denudation. Not only have the wide valleys and deep gorges been excavated, but an enormous amount of material has been worn away from the broad rocky terraces. From the high plateaux of Utah the Mesozoic and Tertiary formations descend by a succession of broad terraces like a giant staircase to the platform of Palæozoic rocks. Capt. Dutton gives reasons for his belief that the strata which end at the cliffs of these successive terraces once extended over the whole of the Grand Cañon district, and he estimates the amount of rock thus removed to have averaged probably 10,000 feet in thickness over an area 13,000 to 15,000 square miles in extent. He bases this estimate partly upon the obvious continuity of the strata, and the improbability that they could have ended off upon the Carboniferous platform; partly upon the evidence of displacements whereby Palæozoic rocks, formerly buried at least 10,000 feet below the sea-level, under an accumulation of sediment of that depth, have been again uplifted into the lofty plateaux of the Colorado; partly upon an argument from the history of the drainage-lines of the district. In this last argument, developing the views so forcibly expressed by Jukes many years ago for the rivers of the south of Ireland, and more recently applied by Powell to the stupendous illustrations in the Colorado basin, he shows that the present courses of the rivers are so entirely independent of structural features, that their position is inexplicable save on the interpretation that when the streams began to flow these features had not revealed themselves. He thus smoothes over the faulted Carboniferous platform, piles over it a covering somewhere about two miles thick of Mesozoic and Tertiary strata, and makes the rivers begin their first erosion on the surface of this covering. The faulting, plication, and uplifting have taken place subsequently; but meanwhile the rivers have kept their courses, incessantly sawing their way downward into lower layers of rock, and across the dislocations and folds that subterranean disturbance might throw across their path. No thoughtful student of this subject can refuse his assent to the solution of the problem so well worked out.

In tracing the geological history of the cañon region, we find at the bottom of all the visible strata, a foundation of ancient crystalline Archæan rocks, and also crumpled and broken masses of stratified formations, referred with more or less confidence to the Silurian and Devonian periods. The disturbance and extensive denudation of the older Palæozoic masses had been effected before the lowest of the vast conformable series of formations in this region began to be deposited, for the latter lie upon the upturned edges of the former, as on a platform—an impressive feature in the scenery. Continuous sedimentation began some time in the Carboniferous period, and appears to have been carried on with no sensible break up to the close of the Eocene period, until a total depth of at least 15,000 feet of sediment had accumulated. The Carboniferous portion is estimated at a thickness of 4500 feet, the various Mesozoic formations at 9000 or 10,000, and the portion of Eocene lacustrine beds deposited were 1000 or 1200 feet.

Capt. Dutton calls attention to the remarkable uniformity and persistency of the lithological characters of each formation, while at the same time there is great diversity in those respects between the strata of different platforms. By far the larger proportion of the whole mass of conformable strata consists of sandstone, presenting on successive horizons the most extreme contrasts of structure and colour, for they consist along certain platforms of adamantine quartzite, in others of massive cross-bedded sand-rocks, while they graduate also into shales and these into marls. It is this alternation of strata, showing very different degrees of permanence, yet each retaining its normal characters over vast areas, that affords the key to much that is most characteristic in the scenery of the region. The limestones are almost wholly confined to the Carboniferous system, where they occur both in the lower and upper divisions.

Another significant feature brought out by the survey is the evidence that sedimentation went on nearly at sea-level during the whole of Mesozoic time throughout the Cañon province. As the Mesozoic strata are 9000 or 10,000 feet thick, it is obvious that the sediments which were at or near the sea-level at the beginning had sunk to that distance below it by the end of the period. We have here, therefore, a consecutive series of shallow-water deposits not much less than two miles in vertical thickness. The Cretaceous rocks which form the uppermost division of this series are from base to summit banded with seams of lignite or coal, and layers containing marine mollusca. They vary in different parts of the province from 3500 to 8000 feet in thickness. At the close of their deposition, those movements appear to have begun which have culminated in the elevation of the sea-floor into the elevated plateaux that now form so prominent a feature on either side of the watershed of the continent.

With the advent of Eocene time the shallow sea-floor, in which sedimentation had been so continuous during the whole of the Mesozoic ages, began to be converted into wide fresh-water lakes. The Tertiary history of Western America is in large measure a record of the formation, duration, and effacement of these lakes, as the land gradually increased in elevation. In the plateau country the Eocene lacustrine deposits range from 1000 to 5000 feet in thickness. Great as this accumulation is, it unquestionably took place in comparatively shallow water over an area that was generally rising, yet was locally sinking, so that the lake persisted, and remained shallow; for its depth was reduced by the deposit of sediment as fast as it was increased by subsidence. The waters appear to have dried up from south to north, and finally disappeared somewhere in the area of the Uinta mountains.

It was on the floor of this desiccated lake that the drainage system of the Colorado river began, somewhere about the close of the Eocene period. During the vast succession of ages that have since elapsed, erosion has been continuously in progress, and the result is the scenery of the Cañon region. Capt. Dutton gives what appear to be good reasons for believing that the larger rivers flow along the same channels which they took at the beginning, but that the minor tributaries, where any exist (and they are conspicuously absent in some wide districts), are comparatively recent in origin, and have been determined by modern surface conditions. The excavation of the Grand Cañon of the Colorado has thus been going on ever since the Eocene period. During that enormous interval the climate of the region appears to have passed through successive oscillations. There is no more skilful feature of the volume before us than the way in which the scattered facts that bear on this question are marshalled to their places and made to tell their story. Ancient river-beds, which for ages have been dry and are partly filled up with debris, open on the edge of the great chasm. They doubtless discharged their waters into the main river at a time when rains were abundant and watercourses numerous. But their fountains have long since been dried up, and their fading channels are almost gone. But all the while the Colorado and its larger feeders, drawing their supplies from far well-watered uplands, have continued their task of erosion until they have sunk their channels in some places more than a mile below the level of the plateau across which they flow.

The process of the excavation of the Grand Cañon is treated at length, and much new information is given as to its varying conditions. The details of the erosion are described with great clearness. The two final chapters, wherein these subjects are discussed, contain much that is suggestive, and deserve careful perusal by all who take interest in questions of denudation. They are condensed pieces of reasoning which cannot be intelligibly summarised here, and which indeed one is hardly prepared to find in an official report. Like his colleague, Mr. G. K. Gilbert, Capt. Dutton properly lays great stress upon the influence of an arid climate as one of the chief factors in cañon excavation. He points out how the absence of vegetation exposes the surface of bare rock to the action of rain. But it may be doubted if the scanty rains of the region can do more than remove material already disintegrated. We have to account for the continuous lowering of the level of the plateaux, and the removal of so vast a depth of stratified rock from their surface. Capt. Dutton himself admits that most of the rain which falls upon the country is absorbed by the rocks, and gushes out in copious springs at the base of the cañon-walls, thereby notably increasing the volume of the river. But there is everywhere a perceptible disintegration of the rock at the surface. This decay cannot be attributed to frost, which in so dry a climate can have but small effect. It seems to be due in large measure to the superficial strain induced by a great daily range of temperature. And it is no doubt aided by the action of wind, which removes the loosened particles, and exposes a new surface to the same kind of disintegration.

In conclusion, reference must be made to the truly magnificent series of illustrations by which this monograph is accompanied. The maps of the atlas give the reader a clear mental picture of the general topography and geological structure of the region. But it is by the pictorial illustrations that he will be chiefly fascinated. These are scattered profusely through the text, and form an important feature in the atlas. Mr. Holmes, whose reputation for the accurate and artistic rendering of geological details is so well established, has here far surpassed all his previous efforts, and has produced the most impressive and instructive geological pictures that have ever been made. His large coloured views of the Grand Cañon are in themselves a series of lessons in geology far more interesting and effective than can be supplied in words. The United States may be heartily congratulated on this first of the monographs of their Geological Survey. Let us hope that Congress will continue in the same liberal spirit the annual appropriations that have enabled the Director of the Survey and his associates to produce such splendid results.

ARCH. GEIKIE

CENTRAL ASIA

Travels and Adventures East of the Caspian during the Years 1879-81, *including Five Months' Residence among the Tekkés of Merv.* By Edmond O'Donovan. Two Vols. (London: Smith Elder, 1882.)

Wanderings in Baluchistan. By Major-General Sir C. M. MacGregor, K.C.B. (London: Allen and Co., 1882.)

MR. O'DONOVAN'S venturesome excursion to the Merv Oasis stands out conspicuously as perhaps the most romantic episode in the recent annals of Central Asiatic travel. Yet in proceeding eastwards his original goal was not the Mervli Turkomans, but their western kindred, the Akhal Tekkés of the Daman-i-Koh. Sent out as the Special Correspondent of the *Daily News* with the Russian expedition against those nomads in 1879, he was at first well received, and spent some profitable time during the progress of military operations on the Caspian seaboard. But after the death of General Lazareff, having been suddenly banished from Chikislar, his ramblings lay henceforth mainly within the North Persian frontier. Here he again went over the ground, with which we have been made tolerably familiar by V. Baker, Macgregor, Stewart, and other recent explorers. Nevertheless even of this region Mr. O'Donovan has much to tell us, which is both new and interesting. There is a freshness and a fulness of detail in his account of Meshed, Tehrán, Kuchan, Resht, Shahrúd, as well as of the people and scenery of Khorasán and Mazandarán, which lend a peculiar charm to the first of these brilliantly written volumes.

But the chief interest of the work naturally centres in the section devoted to the Merv Oasis and its Tekke Turkoman inhabitants, with whom the traveller passed a forced residence for over five months during the year 1881. How he eluded his Persian escort, crossed the border above Sarakhs, traversed the Tejend river valley, plunged boldly into the heart of the desert, safely reached the Murghab Oasis, allayed the suspicions of the Tekkés, who took him for a Russian spy, gradually gained their confidence, became in fact a "Tekke of the Tekkés" and head of a Turkoman triumvirate, finally, by a rare combination of tact, patience, and courage, again escaping from his too importunate friends, all reads far more like a wild piece of fiction than so much sober history.

Although Colonel Stewart had recently brought home some accurate information regarding the present state of the Oasis, this region had been actually visited by no European traveller since Abbot's expedition in 1840. Hence Mr. O'Donovan is here on comparatively new ground, and his graphic account of the place and its inhabitants will be read with deep interest, especially by those who have not seen the portions already published in the *Daily News*. Since the Russian occupation of the Akhal country, the Oasis has doubtless lost whatever strategical significance it may have hitherto possessed. Nevertheless its position in the desert midway between the Oasis and Caspian, its great fertility and dense population—estimated at about 500,000—its numerous antiquities and grand historic memories, must always ensure for the "Queen of the World" an exceptional importance in the eye of the statesman and historian. The student will here find ample details of its present social and economic condition, of its government and administration, of the organisation of the Toktamish and Otamish Tekke tribes,[1] their local institutions, the water system of the Murgháb, the remains of Bairam Ali, and other cities which successively bore the name of Merv, the home life of the Mervlis, their actual commercial and political relations and future prospects.

A very full description is given of the many ruins scattered over the Oasis, all of which were visited and sketched by the explorer. Of these the most extensive are Giaur Kala, the original site of Merv, destroyed about the end of the seventh century by the Arabs, and Bairam Ali, its successor, destroyed in 1784 by the Amir of Bokhara. A general plan is given of all these crumbling citadels, palaces, tombs, baths, and earthworks, "where now no living creature is to be met with, save an occasional Ersari robber or treasure seeker. For here, as in almost every other part of the East, the popular imagination enriches these ruined vaults and foundations with secret treasures stowed away beneath them" (ii. 241).

From the frequent recurrence of the term *Kalassi*, supposed to be a corruption of *ecclesia*, some archæologists have scattered the remains of ancient Christian churches with a liberal hand over Western Turkestan. But Mr. O'Donovan suggests that there is here a confusion between *Killissi*, which really represents *ecclesia*, and *Kalassi*, a Turki form of the Arabic *Ḳal'a*, a fort or castle.[2] Hence Kara Kalassi, for instance, would mean, not the "Black Church," but the "Black Castle." In Armenia, a Christian country, *Killissi* certainly occurs; but in the Oasis Mr. O'Donovan "never came upon any structure which could possibly have been a Christian Church" (ii. 177).

Amongst the remains are some earthworks bearing the title of Iskander Kala, or "Alexander's Castle," the local tradition being that the Macedonian army encamped here on its way to India. But here again he pertinently remarks that "in these countries Alexander comes into every story connected with ruins of remote antiquity."

Some points of resemblance are discovered between the Turkoman and Kelt, which are probably not intended to be taken seriously. But the description of the Turkoman type, coming from a shrewd and original observer, possesses sufficient ethnological value to deserve quoting:

"The usual Turkoman physical type, both male and female, is rough, rude, and vigorous, and quite in contrast with that of the frontier Persian, which is sleek, cat-like, feeble, and mean. The worst part of the Turkoman is his head, which is decidedly conical, the point being thrown somewhat to the rear. A phrenologist would say that firmness was very pronounced, conscientiousness wanting, and benevolence small. The features are not of that Tartar cast that one would be apt to suppose in denizens of East Caspian districts, and though here and there may be seen a suspicion of peeping eye, a tendency towards flattening of the point of the nose, and occasionally high cheek bones, on the whole the faces are more European than otherwise. In fact I have seen some physiognomies at Gumush Tepé which, if accompanied by an orthodox European dress, would pass muster anywhere as belonging to natives of the West. It is among the women that the absence of European features is most conspicuous. There are many of them who could fairly be reckoned pretty, though it is quite a different order of beauty from that to which we are accustomed. . . . It is among the men that the handsome individuals must be sought for, especially when there has been an admixture of Persian blood. The scanty beard of the pure Turkoman is then replaced by one of much more luxurious proportions, and of a darker tint; the nose assumes a more or less aquiline form, and the eye loses the cold grey expression so characteristic of the pure-blooded dweller on the Steppes" (i. 231-3).

The accompanying portrait of the author in oriental garb might be taken as an apt illustration of this description. There is also an excellent map of North Persia and the Trans-Caspian region, based on that of Colonel Stewart, but with numerous fresh details embodying the results of the explorer's observations in the Tejend Valley and Merv Oasis. But the spelling is as usual at variance with that of the text, and there is unfortunately no index. The appendix contains facsimiles of a number of letters from Turkoman Khans, one or two of which are fine specimens of the beautiful ta'lík penmanship.

The reputation of an intelligent and enterprising explorer secured to General Macgregor by his "Journey through Khorasán (noticed in NATURE, vol. xx. p. 453), will be considerably increased by his "Wanderings through Baluchistan." The trip was made in company with the ill-fated Capt. R. B. Lockwood, of the 3rd Bengal Cavalry, on their return to duty in India, between the months of September, 1876, and March, 1877. During this period the western section of Makrán was thoroughly explored, and the problems connected with the drainage of the Mashkíd and Mashkel rivers at last cleared up. The Mashkíd was supposed by many geographers to flow through the Dasht to the Arabian Sea, while the Mashkel was sent northwards to the Zirreh or Sistán Hamun, that is, to the Helmand basin. But by actual survey the explorers have shown that (1) both of these rivers belong to the same hydrographic system; (2) this system is unconnected either with the Helmand or Arabian Sea; (3) the two streams, after their confluence above the romantic Tank Zorati pass in the Sianeh-kuh range, flow mainly north-west to the Mashkel Hamun in 28° 20′ N., 60° E.; (4) this swamp has no outlet, and is actually separated by another depression, the Kindi Hamun, and by a range of hills, the Koh-Amir, from the Sistán Hamun. A ride performed under great difficulties across the Kharan desert to the neighbourhood of the Sistán swamp placed all these points beyond doubt, so that the drainage system of the hitherto almost unknown region along the Perso-Baluch frontier, from the Lower Helmand to the Arabian Sea, has now been satisfactorily determined. From Sistán the travellers made their way by two new and parallel routes right across North Baluchistan to Jacobabad in Sind. The numerous typographical points recorded both here and throughout West Makrán are embodied in the accompanying map, which is on a large scale, and which forms an important contribution to our knowledge of the south-eastern section of the Iranian plateau. In the appendix are given the directions, distances, and other useful details of no less than twenty-two routes in the same region. The relief of the land and its salient physical features are also further illustrated by numerous sketches made on the spot by General Macgregor. Much valuable matter regarding the Baluchi and Brahui tribes, and the present political situation of Baluchistan, is scattered over the pages of this pleasantly written volume.

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[1] The subdivisions of these two main branches of the Merv Tekkés do not correspond with those given by Mr. Marvin in his "Merv, the Queen of the World." Some of the discrepancies however may be reconciled by restoring to their proper form the names disguised in Mr. O'Donovan's peculiar orthographic system. Thus his *Karatchmet* appears by reference to Marvin's tables to stand for the *Kara-Ahmed* subdivision of the Otamish branch.

[2] At the same time this *Kalassi* would appear in many cases to be simply the Persian *Kalása*, a well, and especially the watering-places maintained at intervals in the desert for the convenience of caravans and pilgrims to Mecca.

A. H. KEANE

PHYSICAL OPTICS

Physical Optics. By R. T. Glazebrook, M.A., F.R.S. (London: Longmans, 1883.)

THIS is the most recent volume of the well-known series of Text-books of Science published by Messrs. Longman. Mr. Glazebrook is already favourably known as an accurate experimenter and an able theorist in the subject of which this volume treats, and it is therefore unnecessary to say that the treatise under notice contains a large amount of authentic and interesting information on all branches of the subject. We must confess, however, to a certain feeling of disappointment after going through the book, arising chiefly from the fact that the author does not appear clearly to have made up his mind as to the class of readers to whom the book is to be useful. Those who have had any experience in real personal teaching of the artisans and students in science schools for whom the volumes of this series are stated to be intended, will soon perceive that Mr. Glazebrook has assumed an amount of mathematical knowledge and ability which very few of them possess. On the other hand results are occasionally assumed, the investigation of which would be quite within the reach of those university students who will probably form the larger part of the readers of the treatise. For instance, the investigation of the focal lines of a pencil refracted in a principal plane through a prism, and the condition of their coincidence in the position of minimum deviation, is settled by an "it may be shown," although the analysis required is certainly not more difficult than much that is given in the book, and the point to be elucidated is of considerable importance.

The author has intentionally introduced a large quantity of matter which is usually considered to belong to the kindred subject of Geometrical Optics, and although there will probably be a difference of opinion as to the advantage of this proceeding, there will be none as to the clearness of the explanations and the excellence of the diagrams employed. There does not seem to be quite so much "matter new to the text-books" as is hinted at in the preface, but on the whole the book furnishes a good account of the subject, comparable with Lloyd's well-known treatise on the Wave Theory of Light, and dealing with many points which have been investigated since the date of the latter work.

Where there is so much of good, it is a pity that it should not be made better, and there are a few points in which perfection has not been reached. In places there is a tendency to a slipshod and "high-falutin" method of expression which may be forgiven in University Extension lectures delivered extempore to popular audiences, but which is hardly suitable for a scientific treatise. On p. 2 we have a graphic representation of the author raising his arm and of the effect thereby produced on his own body. Later on in the book, quitting the solitary first person, he becomes more friendly to his readers, and speaks of "our apertures, our lens, our prism, our eye," and so on. He even presently hands the apparatus entirely over to the reader and directs him to perform the operations for himself. A more serious matter is the want of care in revising the proofs. For instance, on p. 202 an effect is spoken of as "that due to a single aperture multiplied by a number of apertures," which is nonsense, the author's meaning being "multiplied by *the* number of the apertures." Again, on p. 141, the sentence—"They are distinct from the coloured rings of thick plates discovered by Newton, and were described by him as follows," gives an almost opposite meaning to that which the author intended. It ought to read "which were described."

The proper names are not treated with the accuracy and uniformity which are desirable. We have Fraunhofer usually, but on p. 316 Frauonhofer. Huygens appears on p. 15, but more frequently the name is met with as Huyghens, while the possessive case assumes the different forms of Huyghen's, Huyghens', and on p. 226 Huyghens's. Defects of this kind mar the pleasure with which the book would otherwise be read, and seem to indicate that more care might have been advantageously bestowed on the original composition as well as on the revision of the proofs. Possibly the author wishes to leave something to be looked for in the second edition, for the speedy arrival of which he has our best wishes.

OUR BOOK SHELF

The Year-Book of Pharmacy, 1882. 8vo. Pp. 607. (London: Churchill, 1883.)

THIS volume contains a number of exceedingly interesting papers and extracts. The most interesting are those which relate to the artificial production of organic alkaloids, for when we obtain such a knowledge of the constitution of these bodies as will enable us to make them artificially, we may hope that a new era will commence in medicine, and that the results of the treatment of disease will be more definite and satisfactory than heretofore.

Prof. Ladenburg, who has been engaged for some time on researches into those alkaloids which dilate the pupil, is still continuing his researches, and has obtained very interesting results indeed. Atropia when heated with strong hydrochloric acid splits up into a base, tropine, and an acid, tropic acid. While pursuing his investigations upon tropine, the author came to the conclusion that this base contained an alcoholic hydroxyl group which possessed the exceptional property of forming fresh alkaloids, when treated with certain acids in hydrochloric solution as in the preparation from it of atropine and homatropine. By acting upon secondary amines by chlorhydrines, he has succeeded in obtaining a series of bases analogous to tropine, and yielding, like it, other basic compounds, which resemble natural alkaloids in their properties and composition. For these bases, which perform the function of alcohols and amines, the author proposes the name of *alcamines*, and for the basic ethers derived from them, that of *alcameines*. That resulting from the action of phenylacetic acid, for instance, has a composition represented by the formula $C_{15}H_{21}NO_2$, and forms crystallisable salts. It is a powerful poison, acting on the respiration and the heart.

A curious relation has been discovered between theobromine, the active principle of cocoa, caffeine, the active principle of tea and coffee, and xanthine, a substance found in muscle, and largely contained in beef tea and Liebig's extract.

Dr. Fisher shows that xanthine may be converted into theobromine, and theobromine into caffein. The relation between these bodies seems to be that theobromine is dimethyl- and caffein is trimethyl-xanthine.

From the great importance of the cinchona alkaloids and their extensive use in medicine, it is exceedingly desirable that we should be able to make quinine artificially. This has not yet been done, but, with a view towards it, extensive researches are being made into the constitution of the cinchona alkaloids; and Skraup finds that all the four cinchona alkaloids—quinine, quinidine, cinchonine, and cinchonidine—when oxidised with potassium permanganate yield formic acid, and a base apparently related to phenol or carbolic acid. Other modes of oxidation exhibit a relation between quinine and cinchonine.

Cinchonine has been prepared artificially by treating a mixture of nitrobenzol, aniline, and glycerol with sulphuric acid. In its physiological properties it exhibits a certain relation to quinine, and, like it, reduces the temperature in fever and lessens or prevents putrefaction. It is said to differ from quinine in not producing giddiness, or ringing in the ears, and to have very little action on alcoholic fermentation.

An important paper by Plugge relates to the various strengths of aconitine. He finds that Petit's nitrate of aconitine has a poisonous action at least eight times greater than Meret's and 170 times greater than Friedlander's. Such differences as these between preparations bearing the same name have already led to fatal cases of poisoning—when aconitine has been prescribed medicinally; and Mr. Holmes considers that the only way to secure uniformity in the ordinary preparations of aconite is to prepare them only from plants grown in this country, and gathered while the plant is in flower.

In addition to many other interesting papers this volume contains a bibliography of chemistry, pharmacy, botany, and allied subjects.

Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux. 2^{e} Série, Tome v. 1^{ie} Cahier. (Paris, 1882.)

IN this number there are several interesting papers. We note "La Route d'Australie par le Thermomètre," with tables, by M. Hautreux; "Sur les Unités de Gauss," by M. Abria; "Le Téléphone à Bordeaux," by M. Auguste Bonel; "Notice sur les Communications Télégraphiques sous-marines," by the same; "Modification aux Machines à Force centrifuge," by M. O. de Lacolonge; "Températures et Densités de l'Eau dans l'Estuaire de la Gironde," by M. Hautreux; "Vérification expérimentale des Lois de Dalton relatives à l'Évaporation des Liquides," by M. E. Laval. M. P. Tannery contributes one of his useful critical notes, this time "Sur une Critique ancienne d'une Démonstration d'Archimède." The criticism is contained in § 36 of the fourth book of the "Collections" of Pappus (Hultsch's edition), and it charges Archimedes (in Prop. 18 of his "Spirals") with solving as a *solid* problem (*i.e.* with the aid of the conic sections) the following proposition:—*O M A* is a spiral, of which *O* is the pole, *O A* the axis, and *A C* the tangent at *A*, meets the perpendicular to the axis through *O* in *C*, then *O C* is equal in length to the circumference described with *O A* as radius. M. Tannery supplies the gist of Archimedes' proof, and shows that Archimedes "fait appel simplement à l'intuition et au principe de continuité" Other points of interest turn up in the communication. Dr. Sigismond Günther is engaged upon an extended inquiry into the processes employed by the ancient mathematicians in the extraction of square roots, and in the course of his work has met with some interesting results. Some of these he puts forth in his paper "Sur la dépendance entre certaines méthodes d'extraction de la racine carrée et l'algorithme des fractions continués." The methods examined are those of Mollweide ("Commentationes mathematico-philologicæ tres," Lipsiæ, 1813), and of Alexeieff ("Sur l'extraction de la racine carrée d'un nombre," *Bulletin de la Soc. Math. de France*, t. vii. p. 167).

We have left to the last an article by Dr. Adolf Dux (translated from the *Pester Lloyd* for February 4, 1880), entitled "La Tombe du Savant." Bolyai was professor of mathematics and physics in the "Collège Réformée" at Marös-Vásárhely. No statue, nor marble mausoleum with sides covered with laudatory inscriptions, marks the spot where this *savant* lies; but the tomb, by its occupant's strict direction, is overshadowed by the boughs of an apple-tree, "*en souvenir des trois pommes* qui ont joué un rôle si important dans l'histoire de l'humanité, et il désignait ainsi la pomme d'Ève et celle de Pâris qui réduisirent la terre à l'esclavage, et la pomme de Newton, qui la replaça au rang des astres." Strangely enough, when Dr. Dux visited the tomb there hung on the tree just three apples, "ni plus ni moins."

Bolyai was not only a mathematician, he was also a poet: hence he had not only in his room a portrait of Gauss (with whom he had been associated at Göttingen from 1797 to 1802), but also the portraits of Shakspeare and Schiller.

In 1855 he wrote his own *Necrologe*, and survived its completion about one year. In this "Adieu" occur passages, some grave, and some humorous; of himself he writes, "S'il a été mauvais, la terre est délivrée de lui; s'il a été bon, il est délivré de la terre." He burned his poetical writings, and collected the ashes in a wooden cup, on which he wrote the following lines from Horace:—

> "Poesis
> Si paulum a summo discessit, vergit ad imum."

This is now preserved as a relic in the library of the College. Here too is shown a photograph taken after Bolyai's death. "Une noble figure, au front et au nez puissants, illuminée par la majesté de l'esprit et du sublime repos, avec de longs cheveux lisses, descendant jusqu'aux épaules. Quand on a vu ces traits, on comprend mieux le sens du nécrologe que Bolyai a lui-même écrit et suivant le texte duquel un noble pommier a été planté sur sa tombe.'

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Natural Selection and Natural Theology

I HAVE just received last week's issue of NATURE from England, and find in it some remarks by Prof. Asa Gray on an article of mine which appeared in the *Contemporary Review*. As he appears to solicit a further statement of my views, I shall supply a brief explanation of those passages in my article to which he refers.

This article, I must begin by observing, was written in reply to a criticism on my essay in the "NATURE Series" ("Scientific Evidences of Organic Evolution"), and, being intentionally limited to the ground covered by that criticism, it did not require to discuss all the points raised by Prof. Gray. But without reference to the original article, I shall now consider these points *seriatim*.

First, I am requested to state what I mean by urging that in my opinion there is no logical point of contact between natural science and natural theology. My answer is that natural science, *as such*, can only be legitimately concerned with the investigation of natural or physical causes, and that in whatever degree it presumes to pass beyond the territory of such investigation, it ceases to be natural science, and becomes ontological speculation. In other words, there is no point of logical contact between the methods and aims of natural science, as such, and the super-scientific conclusions which constitute the aim of natural theology. For it is the aim of natural theology to establish certain very definite conclusions with reference to the existence and the character of a "*causa causarum*," which is acknowledged to be supernatural at least to the extent of being inscrutable by any of the methods possible to science. But from this sufficiently obvious position it does not follow, as my critic seems to insist, "that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum*." Whether or not any such legitimate or rational inference can be made, from the data mentioned, *by the human mind* is not here the question, and therefore I decline to enter upon it. The only question before us is as to whether any such inference can be drawn by the human mind from the province of *natural science*, and I say that this question must be answered in the negative, seeing that there is, as I have just explained, an absence of logical contact between the sphere of natural science and the sphere of supernatural theology. Any inferences of the kind in question, be they legitimate or not, are drawn from the *general* order of Nature—*i.e.* from the *universal* prevalence of "laws and sequences of cause and effect"; therefore they are not really or logically strengthened by a mere enumeration of *particular* instances of such laws and sequences all similar in kind. The so-called law of causation as a whole being known, and its universality recognised, its true argumentative value to the theory of theism is not influenced by the explicit formulation of any number of its specific cases which the progress of science has been able, or may be able, to supply.

I allow, of course, that *the human mind* cannot avoid occupying itself with the most momentous of all questions—the nature of the First Cause and of its relations to the universe—or, in the words of Prof. Gray, that "such questions are inevitable"; I am only concerned with explaining why I conceive that such questions are not affected by any of the methods or results of natural science, which do not stand in any logical relation to them. And this introduces me to the next point in Prof. Gray's criticism. He says that I am inconsistent in first alleging that there is no point of logical contact between natural science and natural theology, and then proceeding to affirm that the theory of natural selection has proved destructive of the evidence of special design in organic nature. But I think this charge must have been made without due reflection; for, if a man believes that there is no logical connection between one thing and another, I do not understand why he should be deemed inconsistent merely because he endeavours to show the fictitious character of the logical connection which has been erroneously supposed to exist. Assuredly I think that "Darwin's theory need not, and legitimately should not, concern itself with natural theology"; but I also think that natural theology should not seek to obtain unreal support from natural science, and it is because natural theology has sought to do this in one conspicuous instance, and in that one instance has been as conspicuously met by Darwin's theory, that, as I explained in my article, it seemed to me desirable, both in the interests of science and of theology, henceforth clearly to recognise the logical gulf which is fixed between these two departments of human thought.

Next Prof. Gray observes, and quite correctly, that my view of the matter as a whole is fairly presented by the following sentences, which he quotes:—

"The facts of organic nature furnish no *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly*."

Prof. Gray then asks: "Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually*?" My answer is but too easy. I must leave to others the happy position of being "quite sure" about anything relating to the possibilities of supernatural causation. For aught that I know, or for aught that any living man can ever know, not only all existing organisms, but all existing atoms, may have depended from all time, and for all their changes, sudden or gradual, upon "intelligence," without which they may not have been able either to have lived or to have moved, or even to have had their being. But how does this necessary ignorance on my part affect my statement that "the facts of organic nature present no *evidence* of design of a quality other or better than any of the facts of inorganic nature"? I confess I do not see how this failure in the evidence of design is made good by telling me that, for anything to the contrary of which I can be "quite sure," there *may* have been a designer. For I cannot follow my critic where he argues that the element of a supposed sudden introduction of an organism to its environment makes no difference in the evidence of its adaptations to its environment having been designed. He asks: "How is this presumption [*i.e.* that of special design] negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations with their environment?" The answer is, that if we suppose the sudden or special creation of organisms in manifold adaptation to their several environments, we can conceive of no cause other than intelligence as competent to produce the adaptations, whereas, if the adaptations have been effected gradually, and *by the successive elimination of the more favourable variations by a process of natural causation*, we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design. Assuredly "*gradualness* is in nowise incompatible with design," and I do not suppose that there has ever been any one so foolish as to imagine that it is; but all the same, the progressive adaptations of structures to functions by such a purely physical cause as natural selection when once clearly revealed must destroy all special or particular *evidence* of design, even supposing such design to exist. For under this point of view it was *only* those variations which were "in compatible relations with the environment" which were able to survive. Only if it could be shown that the variations always took place exclusively in the directions required for a development of the adaptations, so as to leave no room for the operation of the physical cause in question—only then would the evidence of design as deduced from the theory of evolution be comparable with that evidence as deduced from the theory of special creation.

Towards the close of his letter, Prof. Gray seems to have anticipated this obvious rejoinder, for he says that, in order to make the purely physical explanation tenable, "it must be shown that natural selection scientifically accounts for the adaptation,"—*i.e.* as I understand, it must be shown that there is not some influence of an intelligent kind guiding the occurrence of the variations in the requisite lines, which, having been thus intelligently caused to arise, are then seized upon by natural selection. If this is Prof. Gray's meaning, he is certainly wrong in attributing it to Mr. Darwin, and I cannot see that it is a meaning of any argumentative use. For the burden of proof lies with the natural theologian to show that there *has* been some such intelligent guidance of the variations, not with the evolutionist to show cause why there *may not* have been such guidance. The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may sometimes have occurred along favourable lines as "responses of the organisms to their physical surroundings."[1] But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this—that they are of a physical or natural kind.

And this leads me to the core of the whole subject. Prof. Gray says:—

"What is probably meant is, that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world on physical principles, and so renders . . . the evidence of design from these adaptations of no other or better value than that from anything else in Nature." He then proceeds to object to this view, and says:—"If means and ends are practicable in inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*."

This, I say, is the core of the whole subject. If once it is fully admitted and understood that organic nature is one with all the rest of the universe in the matter of physical causation, so that all the wonderful adaptations which we there encounter are the results of natural causes—survival of the fittest *plus* any number of other natural causes—then it appears to me, as I have said in the essay already alluded to, that all such cases of adaptation must fall into the same logical category, with reference to the question of design, as all or any other series of facts in the physical universe. For the only element of difference arises from the greater intricacy of the physical causation in the cases contemplated, rendering it more difficult to perceive the operation of the causes, at work, and therefore, as Prof. Gray truly asserts, rendering their operation more suggestive of design. But this element of difference does not really affect the question. For, *ex hypothesi*, the law of causation is everywhere and equally uniform, and for this reason the evidence of design in organic nature is certainly *not* other than it is in inorganic nature, nor, in view of the same reason, is it, to our thinking, better.

Florence, February 3 GEORGE J. ROMANES

THE letter of Prof. Asa Gray (NATURE, vol. xxvii. p. 291) contains a sentence which seems to me to contain the essence of the difference between the views of organic life, as held by the supporters of Natural Selection and Natural Theology. He says: "How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, *yet so as always to be in compatible relations to the environment?*" The italicised portion is just such a statement as "Design" would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species? With "Design" there ought to be a perfecting of all species; whereas we know of so many which have been ruthlessly swept aside, owing to their having "differed (or owing to their not having sufficiently differed) from time to time in small particulars, yet" *not* "so as to be in compatible relations to the environment." Change is the evolutionist's view of life—change sometimes caused by the environment, sometimes beneficial, sometimes eventually detrimental: where beneficial, the species increases; where detrimental, other changes or extinction must ensue. Design would never have supplied us with a "Nature red in tooth and claw with ravine," nor would it have built up a system by the expensive and cruel mode of trial and error.

Cove Castle, Loch Long, N.B. J. B. HANNAY

Two Kinds of Stamens with Different Functions in the Same Flower

To the Melastomaceæ and Commelynaceæ mentioned in NATURE (vol. xxiv. p. 307, vol. xxvi. p. 386, and vol. xxvii. p. 30), may be added the genera *Mollia* (Tiliaceæ), *Lagerstrœmia* (Lythraceæ), and *Heteranthera* (Pontederaceæ), for having differently coloured anthers. In several species of *Mollia*, according to Darwin ("Forms of Flowers," p. 168, footnote), the longer stamens of the five outer cohorts have green pollen, whilst the shorter stamens of the five inner cohorts have yellow pollen; the stigma stands close beneath the uppermost anthers. In a *Lagerstrœmia* in my garden the six outer stamens have green pollen, and are much longer than the numerous inner ones, which have bright yellow pollen; the stigma stands on a level with the outer anthers. I have repeatedly seen bees alighting on, and gathering the pollen of, the inner anthers without noticing the outer ones.

In *Heteranthera reniformis* there is one long stamen (belonging to the outer whorl) having pale bluish pollen, and two short stamens (of the inner whorl) with bright yellow pollen. The stigma stands generally on a level with the anther of the long stamen. When the white flower opens, pistil and long stamen diverge, the pistil bending (almost without exception) to the right, and the stamen to the left; at the withering of the flower, they again approach each other, so that the stigma may be fertilised by the pollen of the long stamen. Visiting insects are attracted yet more to the yellow anthers of the two short stamens by their being placed close to a yellow spot, surrounded by a violet border, at the base of the upper petal.

Thus it may be safely assumed that in all these flowers, as well as in the above-mentioned Melastomaceæ and Commelynaceæ, fertilisation is almost exclusively effected by the pollen of the longer stamens, whilst the shorter stamens serve only to attract pollen-gathering or pollen-eating insects. It is far from surprising that the pollen of these latter stamens, though often produced in large quantity, should tend to degeneration. Darwin long ago came to this conclusion with respect to some Melastomaceæ with differently-coloured anthers, of which he had raised seedlings from pollen both of the longer and shorter stamens ("There is reason to believe that the shorter stamens are tending to abortion."—"Cross- and Self-Fertilisation," p. 298, footnote). The *Lagerstrœmia* in my garden being self-sterile, I fertilised some flowers with green, and others with yellow pollen of a different variety (or species?) growing in other gardens; both produced fruits with apparently good seeds, but only some of those from the green pollen have germinated.

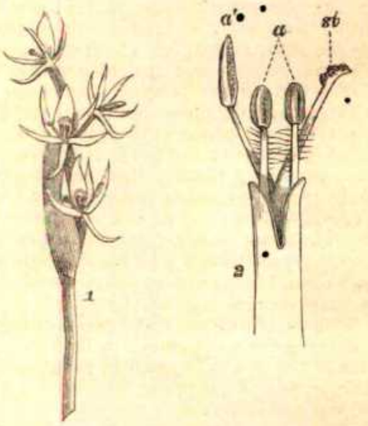

FIG. 1.—Flower-spike of *Heteranthera reniformis* (natural size). FIG. 2.—Upper end of the flower-tube, seen from behind. *a'*, the one anther of the outer whorl, with pale bluish pollen; *a*, the two anthers of the inner whorl, with bright yellow pollen; *st*, stigma.

As in all the flowers above-named, with differently-coloured anthers, the dull colour of those of the longer stamens evidently serves to make them less visible to insects, may not the green colour of the anthers of the long stamens of the mid-styled and short-styled flowers of *Lythrum salicaria* also protect them against the attacks of pollinivorous insects, to which, from protruding far from the corolla, they would be more exposed than those of the shorter stamens?

Even without being differently coloured, the stamens of the same flower may be divided into different sets with different functions. Thus in a species of *Cassia* the visiting humble-bees gather the pollen of the four intermediate stamens (the three upper ones being pollenless), which are short and straight, whilst the three lower ones are very long and curved in such a way that their pollen is deposited on the back of the humble-bees. The pistil is of the same length and curved in the same way as the longer stamens. Another very striking instance has been carefully described by Prof. J. E. Todd of Tabor (Iowa) in a plant of a very different family, viz. *Solanum rostratum* (*American Naturalist*, April, 1882, p. 281): one stamen and the pistil are very long and strangely curved; four stamens are short and straight, and serve only to furnish pollen to the visiting insects; all the anthers, as I am informed by Prof. Todd, are of the same dull yellow colour. FRITZ MÜLLER

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[1] In my "NATURE Series" essay I expressly stated that natural selection is probably not the only cause of organic evolution, and therefore I think it might have been well if my critic had taken the trouble to refer to this essay before indulging in the general proposition at the close of his letter with reference to exactitude.

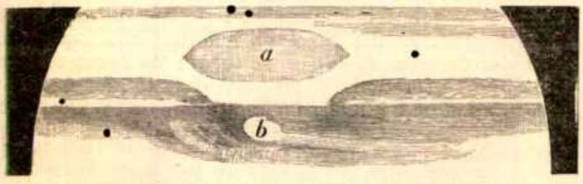
Blumenau, Santa Cattarina, Brazil, December 27, 1882

The Markings on Jupiter

AFTER heavy storms of hail on January 30, the sky cleared and the night was exceptionally fine. I observed Jupiter with my 10-inch reflector about 11h. 30m., and watched the chief markings pass the central meridian of the planet. The well-known equatorial white spot came to transit at 11h. 44m., and it was followed 5 minutes later—at 11h. 49m.—by the great red spot. These objects, therefore, must have been in conjunction on January 30, at 2h. 47m., as the greater velocity of the white spot enables it to gain 13m. 24s. on the red spot daily.

In NATURE, vol. xxv. p. 225, I stated that during the 400d. 0h. 20m. elapsed between 1880, November 19, 9h. 23m. and 1881, December 24, 9h. 43m., the white spot had completed 9 revolutions of Jupiter relatively to the red spot; the number of rotations performed by the former being 976, and by the latter 967. Since 1881, December 24, I have continued to watch the anomalous velocity of these curious markings, and find that between that date and 1883, January 30, the white spot has completed 9 further revolutions of Jupiter. From 1881, December 24, 9h. 43m., to 1883, January 30, 2h. 47m., is 401d. 17h. 4m., during which the white spot has rotated 980 times, while the number for the red spot is 971. In fact my observations since 1880, November 19, show that up to 1883, January 30, the white spot had performed 1956 rotations, as against 1938 by the red spot in the interval of 801d. 17h. 24m.

On January 30, when I last saw these markings, the red spot was remarkable on account of its great faintness. On the other hand, the equatorial white spot was extremely brilliant and conspicuous, and formed one of the most noticeable features on the planet. Observers should now keep a close watch on the red spot, as it seems likely to be on the point of disappearance, though this disappearance need not necessarily be of final character. It fortunately happens that a curious irregularity in the formation of the great southern belt will probably enable the exact position of the spot to be watched for a considerable time. This particular region of the planet, as I drew it on January 30 at midnight, was as follows:—



1883, January 30, 12h. The region south of the equator of Jupiter. *a*, The red spot; *b*, white spot.

The sketch shows that the great south belt is now double, and a very conspicuous object on Jupiter. The south half of this belt is bent abruptly to north, and runs into the other half exactly north of the preceding and following ends of the red spot. There is some explanation to this interesting feature, though it is at present involved in mystery; in any case it may possibly serve as a very accurate indication of the place of the red spot long after that object has become obliterated altogether. W. F. DENNING

Bristol, January 31

Meteor of November 17

I THINK now that more observations of the remarkable phenomenon of November 17 have been brought forward, that we cannot but candidly acknowledge that the evidence is extremely contradictory and impossible to reconcile, that is as applying to one and the same object. Altogether there is something mysterious about it. It is evident that since it appeared to reach the greatest apparent length of about 30° at York, then from all places further south it ought to have attained a length exceeding this, the more so the further south they are. The ends of the beam appeared very well defined from here, and there was very little room for estimates varying according to the observer's sensitiveness to light. If we take the observations made from Clifton, Cirencester, East Clevedon, Woodbridge, and Windsor, as they nearly all agree in estimating the length as *over* 30°, some considerably over, then these may all relate to the same object. But its appearance from York is flatly contradicted by Mr. Batson's observation from Hungerford, that from Halstead, Essex (which seems to agree with Mr. Batson's), also those from Lincoln's Inn Fields, Greenwich, and Cambridge. All these agree in contradicting the others named above, by assigning a much *smaller* angular length. Mr. Batson describes a sudden foreshortening which the meteoroid underwent when passing the moon, and since I saw it pass below the moon at practically the same time, then (on the supposition that we beheld the same object) the same shortening ought to have been visible to me; but there was not the slightest trace of any such thing. I noticed that it very gradually shortened in length (after allowing for perspective) in its journey towards the west, which is significant, and explainable if we suppose the body to have been encountering resistance to its momentum. It is impossible to reconcile all the observations, and yet most extraordinary that no single observer is known to have witnessed more than one such phenomenon at about that time except Mr. Worthington, who says he saw two *at once*. I have reason to believe that a rather similar thing was seen below the moon at about 5.30 on that night from here. I see that from Ziericksee, in Holland, a similar phenomenon was seen to transit α Pegasi (which would be at about 50° altitude, and on the magnetic meridian from there). If this was the one that I saw, then at the time that it was seen to transit α Pegasi, from Holland, it would appear to me to be just forming in the south-east where it appeared to be about 10° above the horizon, in which case it would have to be under seventy miles high when over Belgium. But it is almost certain that it attained a height of over 150 miles during the latter part of its course. As yet (figuratively speaking) the spectra of these auroral phenomena have not thrown as much light on these things as that which enters the narrow slip of the spectroscope to print its uncertain record on the retina. I only hope that some one with a clear head and much patience will succeed in unravelling the tangled skeins of evidence which surround the mysterious meteoroid of November 17, 1882. H. DENNIS TAYLOR

Heworth Green, York, February 11

Aino Ethnology

IN an article on "Aino Ethnology" which appeared in NATURE, vol. xxvi. p. 524, and which I happened to read only a few days ago, Mr. A. H. Keane makes the following statement:—"Until the appearance of Herr Rein's large work on Japan, one of the most universally-accepted of these conclusions was that, whatever be their affinities, the Ainos must certainly be separated from the Mongolic connection. No little surprise was accordingly produced by Rein's attempt to affiliate them to the surrounding members of the yellow race. But it was soon seen that his arguments, apparently inspired by a love of paradox, were sufficiently refuted by the very illustrations of the Aino type introduced into his work."

I submit that one who has read my work upon Japan will decide, with me, that the spirit of the matter quoted is unfair, in so far as it charges me with "attempts" at affiliation, and with being "inspired" otherwise than by a love of truth—this motive being, as stated in my preface, that which induced me to write.

It is repeatedly mentioned in my book that I had never been in the island of Yezo, and those who have carefully read the whole work—including Mr. Keane, if he has done so—cannot reasonably fail to observe that I speak of the Aino tribe as one who had never visited them in their proper home, nor made them a special subject of study in any respect. My remarks upon their probable racial affinities were based upon good, and the then latest, authorities, whose names I was careful to mention. Thus, on p. 444, occurs a passage of which the following is a rendering:—"Dönitz and Hilgendorf have made thorough investigations of their (the Ainos) physical peculiarities, and have published the results thereof in the *Mittheilungen der Deutschen Gesellschaft Ostasiens*. It appeared as an undoubted fact 'that the Ainos are Mongolians, who are separated from the Japanese in a perhaps less degree than the Germans from the Romans.'"

In the comments which I added to the views of these eminent observers, wherein I mentioned those circumstances that seemed to me to tend in the direction of their support, I was, of course, unable to include, as I would if possible have done, a statement of the opinions of such noted authorities as Dr. Steube and Herr von Siebold, the results of whose investigations have so recently been given to the press, and which are cited in the article that elicits this letter. J. J. REIN

Marburg, Germany, February 8

Hovering of Birds

I REGRET that I did not notice until to-day that Mr. Airy, in his letter published in NATURE, vol. xxvii. p. 294, specially referred to "hovering with perfectly motionless wings" as being that for which an upward slant of wind is, as he believes, absolutely requisite to enable the bird to do so.

Is the term "hovering" applicable to the examples given by Mr. Airy of gulls and hawks floating as it were with motionless wing along hillsides and cliffs?

I have always associated "hovering" with the flapping or fluttering of the wings, as is invariably noticed when terns or hawks are looking for their prey either over land or water.

February 10 J. R.

Intelligence in Animals

IN his letter in NATURE, vol. xxvii. p. 337, Mr. J. Birmingham does not mention what kind of bear it is that throws down pieces of rock "in order to catch the bareins," as told by the Kamtschadales; but the Eskimos have a somewhat similar story of the white bear, when attacking the walrus, the largest of which, with their formidable tusks, Bruin generally avoids.

The circumstance was told me by an eye-witness, a very truthful and honest Eskimo of Repulse Bay. He said: "I and two or three other Innuit were attempting to approach some walrus in winter, lying on the ice close to the water, kept open by the strong current, in Fox's Channel. As we were getting near we saw that a large white bear was before us. He had reached in the most stealthy manner a high ridge of ice, immediately above where the walrus were lying; he then seized a mass of ice[1] in his paws, reared himself on his hind legs, and threw the ice with great force on the head of a half-grown walrus, and then sprang down upon it."

The Eskimos then ran up, speared the bear, and found the walrus all but dead, thus securing both animals. I should add that the bear threw the ice as if he was "left-pawed."

Kensington, February 10 J. RAE

WHILE spending the late winter months at Paignton, in Devon, I frequently watched, through a telescope, shore birds of various kinds stalking game on the low-tide sands. These abound with sand-eels, which lie, perfectly concealed, about an inch below the surface, and are caught in the following way by the gulls.

Standing close to the water's edge, the birds tread the wet sand into soft puddles by rapid alternate movements of their feet, and when a sand-eel, thus disturbed, makes a dart for the sea, he is instantly taken by a skilful but leisurely-looking snap of the beak.

Sand-eels bury themselves without leaving any marks on wet sand, and the gulls were always seen steadily and tentatively beating over the ground in the way I have described. They took, each, a fish a minute, perhaps, and impressed me with the idea that some thoughtful ancestral gull had deserved well of his race for the invention of such an easy logical way of picking up a living. D. PIDGEON

Holmwood, Putney Hill, February 7

The Sea-Serpent

ON reading the letter of W. Steadman Aldis in NATURE (vol. xxvii. p. 338) yesterday, I was reminded by a person present that some years ago, when in Orkney, I pointed out an appearance that most people unaccustomed to witness it might have taken for a great sea-monster. This was nothing more or less than some hundreds of cormorants or "scarps" flying in a continuous line close to the water, the deception being increased by the resemblance of a head caused by several "scarps" in a cluster *heading* the column, and by the "*lumpy*" seas of a swift tideway frequently intervening and hiding for an instant part of the black lines, causing the observer to—not unnaturally—imagine that the portions so hidden had gone under water. The speed of the cormorant on the wing may be fairly estimated at thirty miles an hour or more. J. RAE

Kensington, February 10

The "Zoological Record"

I SHOULD like to point out a slight error in the last impression of NATURE (p. 311). In your notice of the *Zoological Record*, 1881, it is stated that no separate paper seems to have appeared in 1881 exclusively devoted to the group *Octactiniæ*. I should mention that Prof. Nicholson's book on "Monticulipora," his paper on the skeleton of "Tubipora," and Mr. Wilson's paper on the development of "Renilla," all appeared in 1881, and were duly recorded by me. SYDNEY J. HICKSON

Anatomical Department, Museum, Oxford, February 5

SIEVE-TUBES

A CAREFUL examination by E. Russow (*Ann. Sci. Nat.* xiv. 1882, Nos. 3 and 4) of the structure and development of sieve-tubes leads him to the following general conclusions.

In all vascular plants examined, the sieve-tubes exhibit a remarkable agreement in structure, always expressed by the presence of callus. The sieve-punctation appears to be wanting in *Isoetes*, and possibly also in the Marattiaceæ. It is not, when present, confined to the sieve-tubes, but occurs also in the parenchyma of the secondary liber. It is often difficult to decide whether these punctations are actually perforated; but this is clearly the case wherever the sieve is traversed by callose cushions or striæ, or by connecting filaments; the presence of callus is not of itself sufficient to indicate perforation, for its formation certainly precedes the perforation of the membrane. In conifers the punctations between the sieve-tubes and the cells of the medullary rays are provided with callose cushions only on the side of the sieve-tubes, and the punctations remain closed.

The development, accumulation, and final disappearance of the callus indicate that it is not a product of transformation of the cellulose, but that it is separated from the contents of the sieve-tubes; its accumulation round the perforations is proportionate to the freedom and duration of the intercommunication that takes place through them; this communication probably continues as long as the striæ of the callus remain clearly developed, and ceases when these disappear, close up the sieve-pores, and end the function of the sieve-tubes.

In gymnosperms and vascular cryptogams, mucilaginous filaments are never to be seen traversing the callose cushions, although there is always a certain amount of communication between the elements of the sieve-tubes. The special function of the sieve-tubes is probably always maintained wherever striæ cross the callose cushions. The large number of plants in which the sieves are traversed, both in summer and winter, by mucilaginous filaments, and the large number in which no such filaments are at any time observable, contradicts the idea that the function of the callus is to close the sieve-pores during the dormant season.

Much less callus is deposited in the sieve-tubes of closed fibrovascular bundles, especially in permanent organs, than in those of open bundles which increase in thickness from the activity of their cambium. This difference corresponds to a difference in the nature of the contents, and in the duration of the activity of the sieve-tubes. While in gymnosperms and dicotyledons the active period of the sieve-tubes rarely exceeds two years, in monocotyledons and vascular cryptogams it lasts as long as the organ itself. A stem of *Alsophila*, at least twenty years old, had all the sieve-tubes at its base still in a state of full activity. In a stem of *Yucca aloifolia*, about fifteen years old, the sieve-tubes of all the fibro-vascular bundles, even the innermost, were active, and had their sieves covered with callus; but this was no thicker in the oldest than in the youngest tubes. In a stem of *Dracæna draco*, at least twenty years old, the callus had nearly or entirely disappeared from many of the sieve-tubes; but the plant was otherwise in bad health.

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[1] It may be questioned how the bear could find a lump of detached ice. The strong current mentioned is constantly breaking up the ice into small pieces.

The callus is not a reserve-substance; for in gymnosperms and dicotyledons it often remains unchanged for years in the dead sieve-tubes, and even in leaves which have fallen in the autumn, and in aërial branches which die in the winter. It behaves rather like a secretory product; and this view is confirmed by the study of its development. The organised structure which the callus sometimes exhibits is not a sufficient objection to this view.

Under which class of organic compounds the callus should be placed cannot at present be determined with certainty. Its behaviour to iodine-reagents and to aniline blue appears to indicate an alliance with proteinaceous substances, and especially with nuclein; in this respect it differs altogether from the solid carbohydrates, such as cellulose and starch.

All sieve-tubes resemble one another in their contents, at least as far as relates to the parietal protoplasm and water. The mucilage, which is undoubtedly a non-granular protoplasm, only exists in large quantities in dicotyledons; no mucilaginous threads can be detected in monocotyledons or vascular cryptogams; in some monocotyledons there is simply an accumulation of mucilage in the sieve-tubes. The sieve-tubes of these two classes contain, on the other hand, a large quantity of smaller or larger refringent globules, which are also proteinaceous. Similar globules have been observed in the closed vascular bundles of *Hippuris vulgaris*.

Although starch is almost always present in the sieve-tubes of open vascular bundles, it is seldom to be met with in those of closed bundles. The diameter of the starch-grains is always greater than that of the canals which are clothed with callus, which renders it impossible for them to pass from cell to cell as long as the sieve-tubes are in an active state. The reddish-violet or brick-red colour which these starch-grains take with iodine reagents indicates the presence of a diastase among the contents of the sieve-tubes.

A series of observations on the same organs by E. Janczewski (*Ann. Sci. Nat.* xiv. 1882, Parts 1 and 2) was directed mainly to a comparison of their structure in the different primary groups of the vegetable kingdom.

In vascular cryptogams the elements of the sieve-tubes are not much larger than those of the parenchymatous tissue. They have no nucleus, and contain proteinaceous globules, adhering to the parietal protoplasm, and collected below the pores. Both the lateral and terminal walls have a larger or smaller number of pores. The membrane of these pores is never perforated, and prevents the intercommunication of the contents of adjoining elements; it is sometimes (as in *Pteris aquilina*) pierced by callose cylinders. The time of year exercises no influence on the sieve-tubes, which remain in the same condition through the whole of their existence.

In gymnosperms the life of the sieve-tubes may be divided into two periods, *evolutive* and *passive*. During the first period the pores in the walls of the young tube produce callose substance, and are transformed into sieves covered and closed by the callus; the elements of the tubes contain, at this period, parietal protoplasm. During the second period the tubes entirely lose their protoplasm, and become inert; but at its very commencement the sieves also lose their callus, and free communication is established between adjacent elements.

In dicotyledons the structure of the tubes is still more complicated; their life may be divided into four periods: *evolutive*, *active*, *transitional*, and *passive*. During the first period the cambial cell is not transformed immediately into an element of the tube, as in gymnosperms; it divides longitudinally, and produces on one side an element of the tubes, on the other side one or two cells of the liber-parenchyma. In the elements thus separated, the pores of the walls, or the entire horizontal septa, become covered with callus, and perforated into true sieves composed of a delicate network of cellulose and a callose envelope. The tubes now enter the second or active period, characterised by the sieve-structure and the free intercommunication of the protoplasmic contents of adjacent elements. It may last for months or years. In some cases the sieves are closed before winter by a fresh formation of callus, and open again in the spring. During this period the tubes contain protoplasm, a larger or smaller quantity of a mucilaginous proteinaceous substance, and sometimes starch. During the transitional period the tubes gradually lose their contents; the sieves are closed by callus, and reopen again by the complete absorption of the callose substance. They have now entered the passive period; they are completely inert, and contain no organic matter; the sieves are reduced to a delicate network of cellulose.

The development and behaviour of the sieve-tubes of monocotyledons resemble that of dicotyledons, and their life may be divided into the same four periods. But from the fact of the vascular bundles being closed, and having no cambial zone capable of forming fresh tubes, the active period of the tubes may last as long as the life of the organ which contains them requires it. The passive period is, in fact, rarely manifested. In our climate the sieve-tubes have the power of closing their sieves in autumn, and reopening them in spring. The elements of the tubes contain no starch or mucilaginous substance; and their parietal protoplasm only contains proteinaceous particles which seem to disappear in the spring, and to add to the density and refrangibility of the protoplasm.

CASSELL'S NATURAL HISTORY[1]

WITH the sixth volume, this well-illustrated account of the natural history of the animal kingdom is brought to a close, and the six handsome volumes leave nothing to be desired, so far as good covers inclosing excellent paper and beautiful typography are concerned. Indeed, the general get-up of the series is quite unexceptional, and as to the average value of the scientific contents we feel fully justified, on the strength of such contributors as Parker, Sharpe, Carpenter, Dallas, Sollas, &c., in strongly recommending the series to the majority of our readers.

From a purely scientific point of view, we regret the title selected by the Editor. He should not have launched so important a book in these days upon the sea of science under an obviously wrong title. The "Historia naturalis" embraces, as the Professor of Geology in King's College, London, well knows, something more than an account of the members of one of nature's kingdoms, and of their distribution in space and time. It is therefore certainly not scientific, and we take it as against modern culture to adhere to such a style. If, indeed, the eminent firm of publishers were to extend this natural history so that in another half-dozen volumes we should have an account of the equally interesting, and even more important vegetable kingdom, the title of the series would the more approach exactness.

Although in the title of his work the Editor has followed in the footsteps of the mere compiler, he has by no means followed this example in the direction of writing on all the groups of the animal kingdom with his own pen, but has been fortunate in getting together a number of contributors, whose very names command respect for their contributions. The table of contents of the just published volume shows that the subjects of the Insects, Myriopods, and Arachnids have been written by Mr. W. S. Dallas, with the exception of the Lepidopterous Insects written about by Mr. W. F. Kirby, the Crustacea are described by Mr. Henry Woodward, the Echinoderms by Mr. Herbert Carpenter, the Sponges by Prof. Sollas, the Rhizopods by Prof. Rupert Jones, and the Worms, Zoophytes, and Infusoria by the Editor.

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[1] "Cassell's Natural History." Edited by P. Martin Duncan, M.D. Lond., F.R.S., Professor of Geology, King's College, London. Volumes 1 to 6, illustrated. Volume 6. (London, Paris, and New York: Cassell, Petter, Galpin, and Co., 1883.)

We have been greatly struck by the immense amount of information given to us by Mr. Kirby in Chapter IX., which treats of the characteristics of the order of Lepidoptera, gives an account of the evolution of these insects from the egg to the perfect state; describes the imago condition; gives a condensed but very clear account of their anatomy, food, and geographical distribution, and concludes with a few hints on collecting, killing, and setting. Among the statistics of lepidopterous life, we note that the present census gives about 10,000 species of butterflies, and 40,000 moths; but then Mr. Kirby adds: "Hundreds of new species are being added to our lists every year." The abundance of species in a district would seem to be in proportion to the variety of the vegetation, which latter is intimately connected with variety of elevation, and so it is "that Lepidoptera are far more numerous in Switzerland than in the peninsulas of Italy and Spain:" but is it not possible that the mountainous regions of Spain will still yield many as yet unknown forms? The illustrations in this portion of the volume are often very beautiful, and comparatively new. Of the next order, Diptera, says Mr. Dallas, "it is not easy to arrive at any trustworthy estimate of the total number of species, yet allowing Dr. Schiner's estimate of 9000 species as European, it has been calculated that the total fly population of the world would be from 150,000 to 160,000. Only a very few of this great army could be of course alluded to, but the information given about the gnats, midgets and crane flies is very full and interesting. Many of these forms are injurious to our crops, as well as irritating to ourselves. The Gall Midges (Cecidomyidæ) are among the most delicate species of all these gnat-like Diptera. The larvæ of these elegant little insects feed upon various species of plants. The number of species is very considerable, about 100 being recorded as European. Many of them by attacking useful plants, frequently do much mischief. Among these may be mentioned the Hessian Fly (*Cecidomyia destructor*), which has done so much damage to the grain crops of the United States, and which received its name from a belief that it was introduced into the States with the baggage brought by the Hessian troops in the pay of the British Government about the year 1776. The Wheat Midge (*C. tritici*) is an enemy of the wheat crops in this country, sometimes doing much damage; several other species form the flower-like galls oftentimes found on willows.

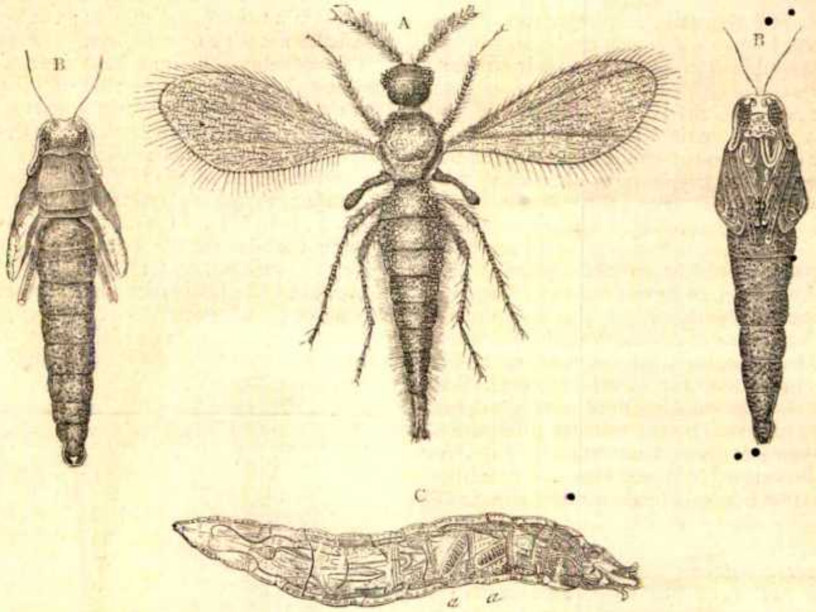

FIG. 1.—Cecidomyid with viviparous larva. A, adult insect; B, B, pupæ; C, larva, showing young larvæ at *a a*.

In 1860 Dr. Nicolas Wagner, of Kasan, made the startling discovery that in certain of these Cecidomyids the larval stages could give rise by a kind of budding, to several small larval-like forms, and that when these latter got free, they in their turn produced still other larval forms in the same curious fashion, and so one generation succeeds another throughout the autumn, winter, and spring. In the summer the last generation undergoes a change to the pupa state, and from these pupæ the perfect winged males and females emerge. The latter lay eggs in the bark of trees, and the larvæ produced from these commence once more a fresh series of organic broods. This strange circle of development is in part represented in the accompanying illustration, which will serve as a fair example of those which abound in this volume. All the families of the flies, ending with that of the flea, which, however, is placed in an order by itself, are well and judiciously treated.

The chapter on the Rynchota is also, despite its subject, a very interesting one, and a great deal of useful information is crowded into a small space. Mentioning the noise produced by the male Cicadæ, the author says: "During the heat of the day they sit concealed amongst the foliage of the trees and shrubs, and sing incessantly;" but is it not rather their wont to select the end of some dead twig, or the extremity of some vine pole, and there out in the full glare vibrate violently. A little space might have been spared for an account or figure of the vine phylloxera.

The chapter on the Orthoptera begins with the crickets and ends with the springtails. Among the Myriopods we find the orders Pauropoda and Onychophora, the latter for Lansdowne Guilding genus Peripatus, which, by the way, he referred in his original description to the mollusca, and not, as here stated, to the worms.

The chapter on the Arachnida includes the Scorpions and their allies the Spiders, Mites, Tardigrades, and Pantopods; in the sketch of the latter a half page might usefully have been devoted to recent researches on the distribution of this extraordinary group of marine forms in the depths of the sea.

The class of Crustacea is well illustrated, and in the introductory chapter we have an excellent account of the general anatomy and strange development of the group. Even Fritz Müller's account of the metamorphosis of Penæus is given, with figures of the Nauplius, Zoea, and Mysis stages. The typographical arrangements of the headings of the orders of the Crustacea seem faulty. The Editor's eye has failed him here, and though the sense is in no ways altered by the want of uniformity in the type used for the headings Brachyura (p. 197), Anomoura (p. 202), Macroura (204), yet there is a utility in the case of a classification of appealing to the eye. The King Crabs are placed as usual among the Crustacea, but the joint authors (Messrs. Dallas and Woodward), in their concluding remarks on the Arthropods write: "the structural relations of these to the scorpions would seem to be very close, and certainly raise a difficult problem, one which is rendered still more interesting by the fact that, according to the researches of Dr. Jules Barrois, a Limuloid, or King Crab-like stage occurs in the development within the egg of certain true spiders. For the present, this and many other such questions must, however, remain open. In all biological problems relating to the past developmental history of the organic world, we must for a long time yet expect to come continually upon obscure and puzzling points which only a more extended knowledge of minute details can clear up."

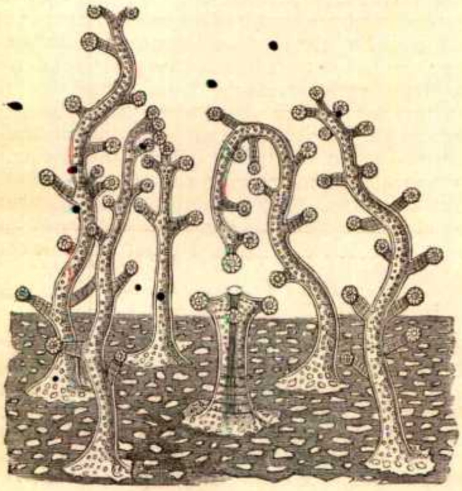

FIG. 2.—Millepora, showing expanded zooids.

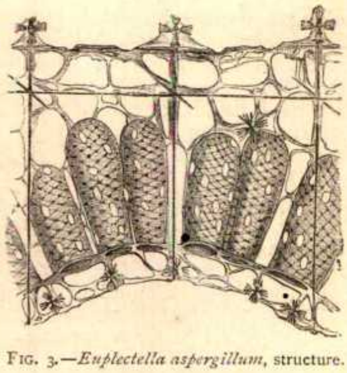

FIG. 3.—*Euplectella aspergillum*, structure.

The various classes of the "grand division" of the worms are treated rather unevenly. This grand division is, no doubt, a somewhat heterogeneous one. "Thus it is found that an animal does not exactly correspond with one of the articulate groups, and another resembles in certain points, but not in all, an Infusorian. They are then placed with the Vermes [worms] because of the existence of certain fundamental structures." There is a good deal of minute anatomical detail given about the Leeches and Rotifers, while the Land Planarians are dismissed with the following:—"They have eyes, no tentacles, a proboscis, and a narrow body. They are found in the United Kingdom and generally in Western and Central Europe. They have been found in America and on continental as well as on oceanic islands. Moseley says that they are nocturnal in their habits, when in the light getting under leaves. Some contain chlorophyll and seek the light, but die in the sunshine. They eat small snails, worms, and flies. An American kind secretes a mucous thread, and suspends itself in the water, and another lets itself down from the leaves by one."

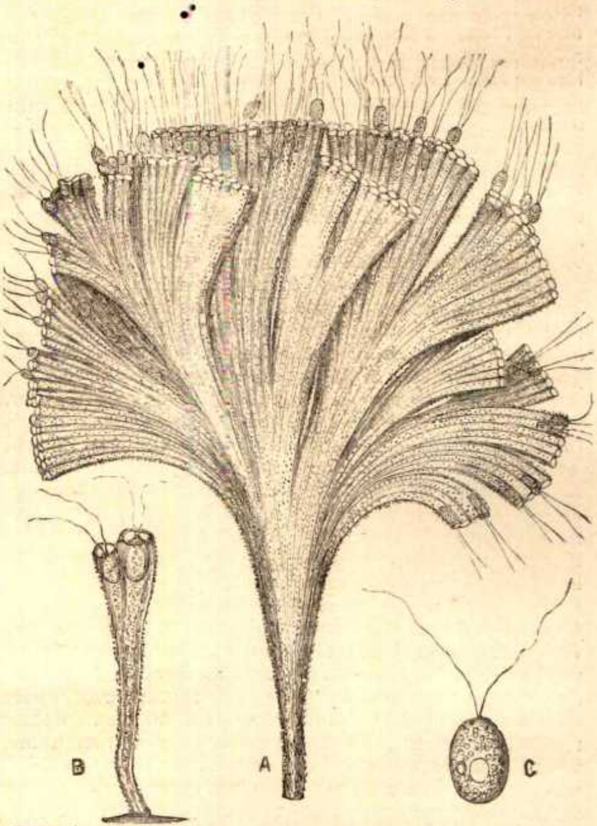

FIG. 4.—*Rhipidodendron splendidum.* A, colony; B, two monads; C, free monad.

If we were introduced to the Worms as a Grand Division, we are told that the Echinoderms form a Sub-Kingdom of the Animal Kingdom, but there is nothing to guide us to this in the heading of the portion of this volume in which Mr. Herbert Carpenter so well, though succinctly describes this important group, an account that we would have wished to have been much more detailed. The classes of this group getting about three pages of text to each, and several of the pages are devoted to new and excellent woodcuts.

The group of the Zoophyta embraces the Hydrozoa and

Actinozoa. These "are distinct from the Spongida, although some synthetic-minded morphologists classify all together as Cœlenterata." In treating of the freshwater Hydra we notice the "old story" repeated that "if the body be turned inside out, the old ectoderm [why the adjective?] takes on the digestive power and the former endoderm that [takes on the function] of the skin."

The order Hydrocorallina is placed as the last of the Hydrozoa, with the families Milleporidæ and Stylasteridæ, as indicated by Moseley, to whose researches and those of Agassiz we are indebted for all we know about the order. *Millepora alcicornis* was obtained by Moseley at Bermuda. The calcareous tissue of the coral is very hard and compact, and the polyps are extremely small. It is very difficult to prevail on the polyps to protrude themselves from their cells, but Mr. J. Murray, of the *Challenger* Expedition, succeeded in procuring them in this state on two occasions, and the accompanying drawing (Fig. 2) of one of the expanded polyps, and of five of its surrounding zooids, is from Mr. Moseley's memoir on the structure of this genus. In the centre is seen the short polyp form provided with a mouth and with only four short knobby tentacles, while grouped around are the five polyps without mouths, and for the sake of letting the central zooid be clearly seen a sixth mouthless zooid is omitted from the sketch; these latter zooids have from five to twenty tentacles; they are much more active than the mouth-bearer, and do the work of catching food for it. When alarmed all disappear within the framework.

The article on the group of the Sponges is excellent. The author now regards the sponges as forming a separate class independent of the Cœlenterata, and situated at the very bottom of the Metazoic sub-kingdom, and gives a brief sketch of the orders and sub-orders.

The figures of sponge structure are refreshingly new, many of them being from quite recent sources—such as the memoirs of Haeckel, Schulze, and Prof. Sollas himself. The beautiful sponge belonging to the genus Euplectella, now known to live anchored in the mud in deep seas, or attached to the hard bottoms of shallower waters, has had its structure ably described by Prof. Schulze, from whose memoir the annexed woodcut (Fig. 3) is taken. The membranous wall—very delicate and thin—which surrounds the skeleton is furnished with smooth-edged roundish *pores* of different sizes, irregularly arranged, and varying very much in number. These form an open communication between the cavities of the chambers and the duct-like spaces surrounding them, which penetrate everywhere between the ciliated chambers and extend even to their mouths, where they terminate on a tougher membrane, which binds together and connects laterally the chamber walls. The figure shows the outer portion of a thin section taken perpendicularly to the outer surface through the side wall of a ridge, and is magnified × 150. Several of the ciliated chambers are seen.

Although the Rhizopods are described as standing "first in the scale of animal organisation," we find them treated of in a chapter before that relating to the Infusoria, and we are told in the same paragraph that "they have in a great degree the same simple constitution as several other kinds of animalcules which are grouped by naturalists as Protozoa." We venture to think that such a description will be apt to lead the general reader astray; nor was it quite fair of the Editor to allow the writer of the article on the Rhizopods to go somewhat out of his way in his forty-ninth paragraph to give a view of the organisation of the Sponges which will be apt to puzzle the reader who has perused the more accurate account of the sponge structure given by Prof. Sollas.

As an example of the beautiful illustrations of the Infusoria, which are for the most part taken from Saville Kent's excellently illustrated Manual of Infusoria, we give the woodcut of *Rhipidodendron splendidum*. There are few workers with the microscope who devote themselves to the study of the Infusoria but must be familiar with the stems of that group of animalcules, which gravitate about the well known *Anthophysa vegetans* of Müller; the attached colony stocks putting one in mind of some minute fucoid stem. Of this group the species figured after Stein is one of the most remarkable, originally described and most beautifully illustrated in Prof. Stein's great work. This freshwater form has apparently not yet been found in this country, but a nearly allied species, *R. Huxleyi*, has been met with in South Devon. The figure shows the compound colony stock at A, the quite young colony stock at B, which latter was built up by a single monad, which divided by longitudinal fission, producing two parallel, or nearly so, tubes, and one of these monads is seen at C free, without a tube.

In congratulating the Editor on the successful termination of his labours, we are not unmindful of the difficulties he has had to encounter in trying to secure a more or less uniform style of treatment of subjects so varied as the different classes and sub-divisions of the animal kingdom.

THE CONDENSATION OF LIQUID FILMS ON WETTED SOLIDS

IN *Poggendorff's Annalen* for 1877, and in the *Philosophical Magazine* for 1880, I have recorded some facts which are satisfactorily explicable only on the supposition that the liquid in contact with the glass undergoes condensation upon the surface of the latter. In the latter paper I was able to show that this condensed film visibly altered the resistance experienced by the liquid in flowing through the tube. In the paper in the *Poggendorff's Annalen* it was shown that a difference of potential was set up between the two ends of a capillary tube through which water was forced, and that the effect of leaving the water in contact with the tube was that this difference of potential rapidly diminished. No doubt this finds its explanation in the effect of the condensation of the liquid on the sides of the capillary tube, causing the friction of the water against the tube to become less and less, whilst the friction of the water upon the condensed water-film becomes progressively greater, as the latter adheres more strongly to the glass. Probably simple drying would suffice to restore to the tube the originally observed difference of potential between its ends.

Whilst working upon this subject I noticed the large E.M.F. produced by a small air-bubble slowly ascending through the vertical capillary tube which was full of water (see Dr. Dorn, *Ann. d. Phys. u. Chem.* 1880, S. 73). At the time I could not account for this, but not long ago I constructed an apparatus which allowed of alternate drops of water and bubbles of air being driven through the capillary tube. This produced a very large E.M.F. Probably this increase in the E.M.F. is dependent upon (a) the increased electrical resistance consequent upon breaking up the water in the tube into drops separated by air-bubbles, and (β) upon an increased disturbance of the liquid film adhering to the glass. Experimentally these effects, (a) and (β), might be separated by substituting for water a (practically) perfectly insulating liquid.

Another and very interesting illustration of a liquid condensed on the surface of a solid is probably to be seen in the familiar fact that water will not clean a greasy sheet of glass.

As is well known to all workers on surface tension, almost the only way of getting a *physically* clean surface of glass is by heating the glass in concentrated sulphuric acid, to which a little nitric acid has been added, and then heating, after washing in pure water to remove the acid. Such a glass surface exposed to the air for a short time is generally imperfectly wetted by water. This no doubt arises, partly at least, from the condensation of *gases* on the surface, which Quincke has shown will produce this effect to a remarkable extent under certain conditions described by him. To this, also, Barrett and Stoney have referred certain modifications of Leidenfrost's phenomena; and the floating shells, &c., of Hennessey are due to the same general cause. But a very minute trace of oil on a physically clean surface produces the familiar greasy surface. Why is this? Oil is not insoluble in water, and when the quantity of water used is sufficient to dissolve the quantity of oil placed on the glass, it ought to wash off. Every one knows, however, how difficult it is to wash oil off glass. *Is this then due to a diminution in the solubility of the oil in the water owing to its* CONDENSATION *on the glass surface?* I believe it to be very probable that this is the case, and think that the experimental proof would be possible by placing estimated quantities of oil on a physically clean glass surface, and subsequently washing in quantities of water, such as under ordinary circumstances would readily suffice to dissolve it. By dissolving the oil in a volatile medium, its quantity might be readily estimated. No doubt other liquids of somewhat greater and better known solubility might be advantageously substituted for the oil, and perhaps, as Dr. Japp has suggested to me, by employing a coloured liquid the result might be rendered evident to the eye.

My inability to complete these experiments at the present time, and the great interest attaching to a determination as to whether the condensation experienced by the liquid-film alters the physical or chemical properties of the liquid must be the excuse for the publication of incomplete results, which I much hope may be taken up by others.

J. W. CLARK

THE STOCKHOLM ETHNOGRAPHICAL EXHIBITION[1]

DR. STOLPE was asked to arrange and describe the Ethnographical Exhibition of Stockholm in the year 1878. This exhibition was brought together from all, or at least nearly all, Swedish public and private collections; no less than 217 exhibitors with about 10,000 objects participated, the King himself opened the galleries, and general interest was raised by an ethnographical exhibition as indeed no other country has realised till now. We took occasion to visit the exhibition, and were astonished to see so rich a material, as well as a thoroughly scientific arrangement.

Both works named below are a result of the meritorious undertaking. The second was partly a guide through some parts of the exhibition, especially China and Japan, with a general introduction, and many valuable and interesting special remarks, partly, in its second volume, a determination of all, about 6200 numbers of the exhibition, arranged after the exhibitors. The first-named work illustrates in geographical order the more important objects of the exhibition, partly in groups, but chiefly in single representations. There may be represented in all about 1500 objects, and we hear that a fourth supplementary volume is in the press.

The first volume of this album contains, on 84 plates, Australia, Oceania, Malaysia, Madagascar, Malayo-Chinese, and Tibet; the second, on 116 plates, China, Japan, Samoyedes, and Turks; the third, on 78 plates, America, Africa, Circassia, Persia, and India. Japan and China, as well as Oceania, are relatively best represented; among the last-named division figures the fine collection from the Savage Islands, which the expedition of the *Eugénie* brought home in the year 1853.

This photographic album must be regarded as the best existing ethnographical atlas; it gives, notwithstanding the inequality in the representation of the single countries, a good idea of a high-class ethnographical museum. The editor has had a full appreciation of the problem which was to be solved, and no ethnologist who works scientifically can do well without this album. It was therefore right that the International Geographical Congress of Venice in the year 1381 should bestow a prize on this beautiful work. Copies of the album are, we believe, only printed to order, and may be obtained direct from Herr Lindberg, R. Archæological Museum, Stockholm.

A. B. MEYER

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[1] H. Stolpe, "Exposition ethnographique de Stockholm, 1878–1879." Photographies par L. F. Lindberg 3 vols. 4to. 36 pp. 278 plates. (Stockholm, 1881).—"Den allmänna etnografiska utstallingen, 1878–1879" (The General Ethnographical Exhibition). 2 vols. 8vo. 80 pp. 1878-1879, and viii. 112 pp 1880.

BARON MIKLOUHO-MACLAY

LETTERS have been received from Baron N. de Miklouho-Maclay from the Suez Canal, the distinguished traveller being now on his way back to Australia. During his prolonged and arduous experience of eleven years' life amongst Melanesian and other savages of the Pacific his health has, we are sorry to say, suffered very seriously, and he returns to Sydney mainly on this account, since he finds that the climate of New South Wales suits him best. He intends to call at Batavia on the way out, where he left a part of his collections in 1878, in order to convey these to Sydney, where the main bulk of the gatherings of his many journeys is already stored. The Emperor of Russia, with enlightened liberality, has promised to defray the cost of the publication of the scientific account of Baron de Maclay's results, and the collections have been brought together at Sydney in order that they may be available for the preparation of the work for the press there.

Baron de Maclay hopes to be able to get ready the whole of his numerous diaries, notes, and papers for publication in about two years' time. The complete work to be issued by him will, if his present plan be carried out, consist of an anthropological and ethnographical section, a section treating of comparative anatomy, and a general narrative of his travels, together with appendices containing meteorological observations and information on physical geography.

The work will be published first in Russian, but translations in other languages will probably soon follow.

He intends to do a good deal of the anatomical work needed to complete his researches on animals collected by him in Australia and New Guinea at the Zoological Station at Watson's Bay, of which he is the founder. This Zoological Laboratory at the very first received most important support from the Linnean Society of New South Wales, and by the influence of this Society a grant of land was obtained from the New South Wales Government for the erection of the building. Scientific men in other colonies, and notably in Victoria, recognising the great importance of the establishment to the progress of biological research, have come forward nobly to support the enterprise, and the Australian Biological Association has been formed, a Society including men of science of all the Australian colonies and some distinguished European naturalists, the object being to support biological stations in Australia. It is very gratifying to find so enlightened a sympathy with scientific progress developed, and that the different colonies are able to work together in so excellent a cause. We hope to refer shortly again to the constitution and aims of the Australian Biological Association.

NOTES

WE can only for the present express the deep regret with which we learn of the death, on the 9th inst., of Prof. H. J. S. Smith, of the Savilian Chair of Geometry at Oxford, at the comparatively early age of fifty-six years. We hope next week to refer to his career and work in detail.

THE death is announced at Basle of Prof. Peter Merian, the Nestor of Swiss geologists. He was born on December 20, 1795. His first important work, on the Jura of the Canton of Basle, was published in 1821, followed a few years later by a geological account of the Southern Schwarzwald. In 1821 he was appointed Professor of Physics and Chemistry in his native University, and at a later period he accepted the Chair of Mineralogy and Geology, which he held for half a century. He was more than once chosen rector of his University, and throughout his life not only continued his geological activity, but took an active interest in scientific work of all kinds as well as in public affairs.

WE learn that Dr. Oscar Dickson arrived in Christiana on the 9th inst. to confer with King Oscar, who is sojourning there, as to an Arctic expedition to be despatched this year, under the command of Baron Nordenskjöld, to North Greenland.

M. JANSSEN, as leader of the French Eclipse Expedition, will embark on March 6 for Panama. He will cross the isthmus by rail, and the *Éclaireur* will be ready at Colon to take him to Sable Island, near Caroline Island in the Marquesas group.

MR. DEANS COWAN, well known for his explorations in Madagascar (see *Proc. R. Geogr. Soc.*, vol. iv. p. 521), has now fully settled to return there, in order to explore the southern part of the island. Of this district little is known, and it may be fully expected that Mr. Cowan will get valuable additions to our knowledge of the natural history of the country. Mr. Cowan calculates that his journey will occupy about two years. His plan is to begin at Ambahy on the south-east coast, and to proceed inland to the most southern point reached when he made his survey of the Bara-land, working southward amongst the Tausay and Taudroy people, thence westward towards the district of the Mahafaly tribe, and on to the River Onylahy. This will occupy one year. From the Onylahy the route will be nearly north through western Bara-land and the Sakalava country, ending at Mojanga. As these journeys will be amongst the aborigines, and even in different geological formations to that from which nearly all our Madagascar specimens are obtained, Mr. Cowan expects that the results will be of a most valuable character, and help to a solution of many interesting questions in regard to Madagascar.

COLONEL ÉMILE GAUTIER has been appointed Director of the Observatory of Geneva, in succession to the late Prof. E. Plantamour, who had filled the position from 1839.

ON Tuesday, February 6, a large number of scientific men and social and political notabilities assembled at the Northern Station, Paris, to witness the transmission of energy by an iron wire used like an ordinary telegraphic line, and extending to Sevran, near Le Bourget, and returning to the station, thus completing a distance of 20 kilometres. The primary engine was moved by a force of about 5 horse-power, and the force of the secondary was said to be 2½. No precise measure was taken. The experiment was in continuation of the much spoken of Munich transmission of energy from Mierbach, according to the Marcel-Deprez system. It is not believed that this new experiment will put an end to the controversy. Many papers have reported enthusiastically on the proceedings, and letters have been written by electricians claiming to have executed more successful trials. It is stated that new experiments on the Marcel-Deprez system of transmission of energy will take place under the superintendence of M. Tresca, with the same machines as on February 6. The *Lumière Électrique* states that the percentage of force is about 37½ per cent., and that the dynamometer for measuring the motive power of the primary machine was not in order at the time of the first experiment.

PROF. FLOWER will commence, at the Royal College of Surgeons, on Monday, the 26th inst., a course of nine lectures upon the Anatomy of the Horse and its allies. In the first three lectures the general position of the horse in the animal kingdom, and its relations to other existing and extinct species will be treated of; the remainder of the course being devoted to a more detailed account of the osseous, dental, muscular, nervous, and other systems, as compared with those of the generalised Mammalian type, the allied forms of Ungulates, and Man. The lectures will be given on Mondays, Wednesdays, and Fridays, and are free to all who take an interest in the subject.

IT seems that the season of 1882 has, on account of the state of the ice in the Arctic seas, undoubtedly been one of the most adverse on record. Thus while the Norwegian walrus and white fish hunters were unable to get to the north of Spitzbergen and the Swedish Meteorological Expedition to Mossel Bay, no vessel succeeded in reaching the Siberian rivers. It appears from information just to hand that the summer along the coast of Siberia has been unusually cold, while incessant north-east winds have accumulated drift-ice on the shores to such an extent that the estuaries of the Yenissei and the Obi were not once navigable in the season. Thus the small steamer *Dallmann*, of Yenisseisk, belonging to Baron Knop, was quite unable to get from the Yenissei into the Obi, and all she accomplished during the year was to transfer a few thousand poods of grain from Mr. Sibiriakoff's depôt, where it had been lying for some years, to Baron Knop's, to be eventually forwarded to Europe. When it is remembered that this was the state of the ice in the eastern and southern parts of the Arctic seas, and we remember the reports of Mr. Leigh Smith and Sir Henry Gore Booth of open water north and east of Novaya Zemblya, it becomes apparent that some other part of the Polar basin must have been very free from ice during the summer. It seems to be the opinion of several authorities, as for instance Baron Nordenskjold, that any vessel which had attempted to penetrate by way of Behring Strait would, no doubt, have demonstrated the practicability of navigating the Siberian seas every summer from one end or the other. This year fresh attempts will be made by Mr. Sibiriakoff, Baron Knop, and Dr. Oscar Dickson to open up a trade route with Siberia from Europe; those however acquainted with the Arctic seasons would not be surprised to see the ice in the summer of 1883 as adverse to Arctic voyaging as it was in 1882.

THE first news has been received at St. Petersburg from the Russian Lena Expedition. Lieut. Harder, who was searching for the remains of the victims of the *Jeannette* disaster, met Dr. Bunge and Jürgens on October 3. He found the members of the Lena Expedition in excellent health, and already comfortably settled in their winter quarters.

M. WOLF, chief of the Physical Department in the Paris Observatory, delivered last Saturday evening a lecture at the Sorbonne, on the Methods employed in Astronomical Physics, before a very large and enthusiastic audience. M. Wolf insisted upon the three methods employed by astronomers, viz. ocular inspection with telescopes, spectroscopic analysis, and photography. He dwelt upon the difficulties of vision with instruments possessed of a great magnifying power, and he tried to oppose the popular delusion that any description of celestial phenomena could be photographed with advantage. He explained that this method should be almost exclusively confined to the sun and moon. The lecture was illustrated by many experiments and projections.

AN important advance is in course of realisation in the use of telegraphy for French newspapers. The *Reforme* has hired a direct cable from London to Paris. The instruments are in the London and in the Paris office of the paper, so that the transmission is instantaneous. According to circumstances, the *Réforme* telegraphists use the Calais, Boulogne, or Dieppe cables. None of these gives a sensible retardation through crossing the sea; but it is remarked that, contrary to expectation, the Dieppe cable is the best of the three. The transmission is made with an ordinary Hughes apparatus.

THE following are the subjects of the lectures to be given at the Royal Institution by Prof. Robert S. Ball, the Royal Astronomer of Ireland, on the Supreme Discoveries in Astronomy:—"The Scale on which the Universe is Built," "The Sun no more than a Star, the Stars no less than Suns," "The Law of Gravitation," and "The Astronomical Significance of Heat." The first lecture will be given on Tuesday, February 20.

PROF. JOHNSTRUP, Rector of the University of Copenhagen, in a paper on "The Glacial Phenomena manifested in Denmark," has shown that the Cyprina-mud deposits overlying the gravel in many parts of the Danish territories afford evidence that an interval of lesser cold must have followed the great glacial period. He moreover regards the presence of the shells of *Cyprina islandia*, and other boreal forms of similar habit, as a proof that the climate in this intermediate period must have been similar to that of the North Sea and the Cattegate in the present day. His views of the connection between these Cyprina deposits and the varied manifestations of glacial action are based on the hypothesis that the ice, which advanced from the interior of Scandinavia and covered Denmark and Northern Germany, must have been driven back, and that on its disappearance, the Cyprina mud was deposited in horizontal layers. On the recurrence of another glacial period these deposits were crushed, dislocated, and often thrust into a vertical position by drifting ice-fields, which had ploughed and broken up the sea-bottom in their advance. This view is in opposition to the opinion more generally held by geologists, that Denmark was twice completely buried under one connected ice-pall, which owed its origin to the continental ice of the Scandinavian peninsula. The direction of the striations and scouring lines in the island of Bornholm, and in some parts of Iceland, which are now being carefully investigated, are, however, admitted to be favourable to the views advanced by Prof. Johnstrup.

MR. GEORGE STALLARD, B.A., of Keble College, Oxford, at present Science Master of St. Paul's School, has been appointed Science Master at Rugby, in place of the Rev. C. M. Hutchinson.

THE Council of the Meteorological Society have determined upon holding at the Institution of Civil Engineers, 25, Great George Street, S.W., on the evening of March 21 next, an Exhibition of Meteorological Instruments which have been designed for, or used by, travellers and explorers. The Exhibition Committee invite co-operation, as they are anxious to obtain as large a collection as possible of such instruments. The Committee will also be glad to show any new meteorological apparatus invented or first constructed since last March, as well as photographs and drawings possessing meteorological interest.

WITH reference to the recent explosion of a zinc-plate oxygen gasometer, described by Herr Pfaundler in *Wiedemann's Annalen*, Dr. Loewe states (*Wied. Ann.* No. 1) that to protect oxygen or atmospheric air from admixture of carbonic acid or acid vapour from the air of the laboratory, he has for many years placed them over lime-water. Some 20 to 30 gr. freshly slacked lime, in a powdered state, is placed in a strong linen bag, which is tied with cord just above the contents, and hung near the outflow tube of the water vessel of the gasometer. This ensures that all carbonic acid and acid vapours which the water of the gasometer may in time absorb from the air, are neutralised by lime-hydrate, and rendered innocuous. There is the further advantage, for elementary analysis, that the potash or soda lye, which is preferred for washing the gases, remains long quite caustic, and thus serves—as it ought to do—less for purification of the gas than as an indicator of the gas current. After long use the linen bag of lime-powder is renewed.

THE February part of Hartleben's *Geographische Rundschen* contains an interesting account of Potanin's journey through Mongolia in the years 1876–77; also some well-written articles on the Samoa Islands, on Eastern Africa, and the European census of 1881, together with a memoir of Count Hans Wilczek.

SOME time ago we announced the commencement of the publication of an "Elektro-technische Bibliothek," by Hartleben, of Vienna. The second volume of this series has just appeared, entitled "Die elektrische Kraftubertragung und ihre Anwendung in der Praxis," by Eduard Japing.

WE are requested by the Council of the Society of Telegraph Engineers and of Electricians to state that an International Electrical Exhibition will be held in Vienna under the patronage of the Austro-Hungarian Government, in the months of August, September, and October next. At the request of the Austrian Minister of Commerce, and of the Managing Committee of the Exhibition, the Council of the Society have appointed a Committee for the purpose of receiving applications for space from intending British exhibitors, and for promoting generally the formation of a British section. Application should be made as early as possible, and should be addressed to the Secretary of the Society of Telegraph Engineers and of Electricians, 4, The Sanctuary, Westminster, S.W.

THE Municipal Council of Paris is proposing to the administration to organise a medical service for the inspection of the eyes, ears, and teeth of the pupils of the public schools, in order to see how to cure constitutional or chronic diseases by which they may be affected.

A LOCAL committee has been established in Annonay for the erection of a statue to commemorate the invention of the *Montgolfier* balloon.

UPWARDS of one hundred Palæolithic implements from the collection of Mr. Worthington G. Smith have been transferred to the collection of Mr. John Evans at Nash Mills, Hemel Hempstead.

A "SECOND London edition" has been issued by Macmillan and Co. of Prof. Newcomb's "Popular Astronomy." The principal additions relate to Dr. Draper's investigation on the existence of oxygen in the sun; Janssen's conclusions from his solar photographs; Prof. Langley's investigation on the solar spectrum on Mount Whitney, California; the satellites of Mars; the supposed intra-Mercurial planet; preliminary results from the late (1874) transit of Venus, and other recent methods of determining the solar parallax; the transit of Venus on Dec. 6, 1882; the great telescopes completed within the last four years; and recent developments in cometary astronomy. The preface is dated Washington, July, 1882.

A DEPOSIT of remains of mammals from the diluvial period has been laid bare by the waves of the Wolga on the bank of that river between Zarizyn and Sarepta. M. Al. Knobloch, of Sarepta, has made a valuable collection of the bones found, which belonged to *Elephas primigenius*, *Bos priscus*, *Elasmotherium*, *Camelus Knoblochi*, besides several antelopes, stags, &c.

On the evening of January 24 an aurora was observed at Geestemunde, which was remarkable both for its duration as well as for the intensity of its light. The sky was quite clear and the moon shining brightly, when about 7.30 p.m. a semi-circle of light appeared in the north-east. Soon afterwards long rays shot out from this across the sky, forming an immense fan of light; the middle one of these rays crossed the sky right down to the south-west, and remained visible in the same brightness for two hours. The size and brightness of the other rays changed constantly. The light was perfectly white.

A violent earthquake is reported from Freiburg-im-Breisgau January 24, at 5.30 a.m., accompanied by loud subterranean noise. At the same time two strong shocks were felt at Bischoffingen. On the same date, at 7.58 a.m., an earthquake was observed in Herzegowina. It lasted for four seconds, and its direction was from north to south.

During the coming summer a Fine Art and Industrial Exhibition will be held at Huddersfield in connection with the opening of the New Technical School.

The additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Mr. T. W. Davidson and Miss M. Sutton; two Common Marmosets (*Hapale jacchus*) from Brazil, presented by Mr. A. Pariss; an Oak Dormouse (*Myoxus dryas*) from Russia, presented by M. A. Wrzesniowski; a Common Marmoset (*Hapale jacchus*) from Brazil, presented by Mrs. Lynch; two Common Gulls (*Larus canus*), British, presented by Mr. W. K. Stanley; two Herring Gulls (*Larus argentatus*), British, presented by Capt. C. R. Suckley; a Brant Goose (*Bernicla brenta*), European, presented by Mr. J. C. Robinson; a Black Lemur (*Lemur macaco*) from Madagascar, four Impeyan Pheasants (*Lophophorus impeyanus* ♂ ♀ ♀ ♀) from the Himalayas, a Black-necked Swan (*Cygnus nigricollis*) from Antarctic America, deposited; two Philantomba Antelopes (*Cephalophus maxwelli*), a Crowned Hawk Eagle (*Spizaetus coronatus*) from West Africa, four Snow Buntings (*Plectrophanes nivalis*), two Brant Geese (*Bernicla brenta*), European, a Red-throated Diver (*Colymbus septentrionalis*), British, purchased; a Schomburgk's Deer (*Cervus schomburgki*), from Siam, received in exchange; two Hybrid Peccaries (between *Dicotyles labiatus* ♂ and *D. tajaçu* ♀), five Ring-hals Snakes (*Sepedon hæmachates*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Comet of 1771.—The comet discovered by Messier at Paris on April 1, 1771, and last observed by St. Jacques de Silvabelle at Marseilles on July 17, has long been mentioned in our treatises on Astronomy as undoubtedly moving in a hyperbolic orbit. This inference was first drawn by Burckhardt, who considered that of all the comets calculated up to the time he wrote (*Mémoires présentés par Savans étrangers*, 1805) that of 1771 was the only one of which it could be stated with some degree of certainty that the orbit was hyperbolic. Encke considered the case worthy of further investigation; remarking that from the nature of the conditions it might be demonstrated that a comet could not rigorously describe a parabola, and that experience so far rather gave the preference to the ellipse over the hyperbola; he insisted that a comet, whose track could not be represented completely except by hyperbolic motion, merited the greatest attention. He accordingly reduced anew the six observations employed by Burckhardt; and after their careful discussion found that the most probable elements were hyperbolic with eccentricity = 1·00937, which is almost identical with Burckhardt's value (1·00944). Nevertheless he did not regard the decided superiority of the hyperbola in the representation of the six places as an indubitable proof of the necessity of admitting motion in that curve; the positions used were not normal positions, but the results of single and isolated observations, and as such, the errors exhibited by a parabolic orbit had not so great a preponderance in his opinion as to enforce such necessity. He concluded that the subject still required examination by a combination of all the observations, and especially if the originals of those at Marseilles could be found. On this point Zach stated, in a note to Encke's communication (*Correspondance Astronomique*, t. v.), that during a recent visit to Marseilles he had searched in vain amongst the papers of St. Jacques de Silvabelle for these originals.

Lately, the orbit of the comet of 1771 has formed the subject of two memoirs, containing very rigorous discussions of the observations, the first by Mr. W. Beebe, in the *Transactions* of the Connecticut Academy of Arts and Sciences, vol. v.; the second by Dr. H. Kreutz, published in the *Proceedings* of the Vienna Academy. Mr. Beebe gives also a hyperbolic orbit, accompanied by the most probable parabola for comparison. The investigation by Dr. Kreutz, a very complete one, gives perhaps a more definite result. He is led to a parabolic orbit for the closest representation of the comet's path, and though the original observations at Marseilles had again been sought for unsuccessfully, he does not think their recovery would affect the conclusion at which he had arrived. The elements of the definitive parabola are as follow:—

Perihelion passage, 1771, April 19·14144 M.T. at Paris.

	°	′	″	
Longitude of perihelion ...	104	1	21·7	M. Eq. 1771·0
,, ,, ascending node	27	53	11·7	
Inclination	11	15	53·1	

Logarithm of perihelion distance, 9·955127

Motion—direct.

The Cassini Division of Saturn's Ring.—At the January meeting of the Royal Astronomical Society, Prof. J. C. Adams made a very interesting communication on William Ball's observations of Saturn, upon which much confusion and misapprehension have existed. Attention has been directed to the subject lately by several astronomical contemporaries, mainly with the view to show that William Ball was not, as he has been considered, the discoverer of the chief division of Saturn's ring. Prof. Adams has carefully examined letters from Ball preserved in the *Archives* of the Royal Society, Huyghen's *Opera Varia*, &c., and remarks: "I find no evidence that Ball, any more than Huyghens, had noticed any indication of a division in the ring." This statement may be accepted as conclusive that the impression of several English writers as to Ball's claim to the discovery of a double ring is a mistaken one, and the credit of the discovery rests with Cassini. The announcement of it made by the French astronomer to the Academy of Sciences is in the following terms:—"Après la sortie de Saturne hors des rayons du soleil l'an 1675 dans le crépuscule du matin, le globe de cette planète parut avec une bande obscure semblable à celles de Jupiter, étendue selon la longueur de l'anneau d'orient en occident, comme elle se voit presque toujours par la lunette de 34 pieds, et la largeur de l'anneau étoit divisée par une ligne obscure en deux parties égales, dont l'intérieure et plus proche du globe étoit fort claire, et l'intérieure un peu obscure. Il y avoit entre les couleurs de ces deux parties, à-peu-près la même différence qui est entre l'argent mat et l'argent bruni (ce qui n'avoit jamais été observé auparavant), et ce qui s'est depuis vû toujours par la même lunette, mais plus clairement dans la crépuscule et à la clarte de la lune que dans une nuit plus obscure. Cette apparence donna une idée comme d'un anneau double, dont l'inférieur plus large et plus obscur fût chargé d'un plus étroit et plus clair." In two figures attached to this announcement the ring is shown with the outer half shaded and the inner half white, and there is a central band across the globe.

ON THE CHEMICAL CORROSION OF CATHODES[1]

This paper contains a description of the influence of various circumstances upon the chemical corrosion of metallic cathodes in different liquids.

Several preliminary experiments are described by means of which it was found that in some cases the chemical corrosion of a metal is increased, and in others decreased, by making the metal a cathode. Also, that the loss of weight of a cathode in an electrolyte is dependent upon several conditions, such as difference of metal, of liquid, or of strength of liquid, some of which tend to increase, and others to decrease the corrosion. In a solution of potassic cyanide pure silver is always protected by being made a cathode. The influence of variations of strength of acid was tried in several cases.

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[1] By G. Gore, LL.D., F.R.S. Abstract of paper read before the Birmingham Philosophical Society, December 14, 1882.

The results, which at first were apparently contradictory, were found to depend upon a number of conditions, and it would require an extensive research to determine the limits of those conditions, and what the proportions are, in which all those separate influences participate in producing the effect. Unequal capillary action is one of them, and its effect is described in a separate paper entitled, "The Electrolytic Balance of Chemical Corrosion." Another is unequal corrodibility of the metal itself. This was investigated, but how it arose was not clearly ascertained. Traces of certain kinds of soluble impurity in the liquid was also a disturbing circumstance. The altered chemical composition of the liquid around the cathode, caused by substances set free or formed by electrolysis, was another influence; this was investigated in the case of a silver cathode in a solution of potassic cyanide, and the protective influence of the current upon the cathode was found to be partly due to the formation of potassic hydrate; the current, however, operates also in some other manner. The effect of temperature was also examined, and it was found that the current exercised a greater protective power when the liquid was hot than when it was cold; the corrosive effect without a current was also greatest (as might have been anticipated) in the hottest liquid. The effects were further influenced by the degree of strength of the current; the greatest strength of current exercised the most protective power, and a large number of experiments were made expressly to test the question whether difference of electro-motive force alone, independently of difference of strength of current, affected the rate of corrosion, but the difficulty of insuring perfect uniformity in all the other conditions which affected the corrosion was so great that sufficiently decisive results were not obtained.

THE MOVEMENTS OF AIR IN FISSURES AND THE BAROMETER

FROM time to time attention has been called to the property exhibited by certain wells in different parts of this country of maintaining an active and permanent circulation of air. It was observed that currents alternately entered or issued from fissures in the sides of the wells, and though in some cases the first emission on sinking the well consisted of choke-damp, the gas subsequently passing consisted of no more than atmospheric air. While it was clear that the currents were not due to the evolution of any gas by chemical action in the rock or the water, an explanation of the phenomenon was found in the fact that the changes in the direction of the circulation coincided precisely with the changes of movement of the barometer, the current being outwards with a falling glass, inwards when the barometer was rising, and ceasing altogether when no change in the atmospheric pressure was taking place. The strength of the currents moreover was found to be proportionate to the rapidity of the barometric movements.

The name of Blowing Wells has come to be applied to such wells in consequence of these properties. From their extreme sensitiveness to changes in the atmospheric pressure, they have been found to give useful indications of the approach of bad weather. Their warnings are rendered audible by fixing horns or whistles in an air-tight covering, in such a way as to sound readily to the outward current, or to give a different note for an outward or inward movement of the air.

The first blowing well of which we have an account appears to have been of an entirely artificial origin. A well was sunk at Whittingham, near Preston, to a depth of eighty feet, and being afterwards abandoned, was covered with a large flagstone pierced by a small hole. Currents of air were observed to enter or issue from this hole, according as the barometer was rising or falling, and a tin horn fixed in it became audible at a considerable distance. Similar phenomena were exhibited by a cess-pool, intended to receive offensive residue from some chemical works. The pool was arched over, a small hole being left for the passage of the refuse; a fall in the barometer was made unpleasantly evident by the issue of offensive vapours.[1]

Subsequently it was noticed that three wells in the New Red Sandstone, in the neighbourhood of Northallerton exhibited the same peculiarity. The wells "blow" through fissures in the sandstone just above the water-level. The changes in the direction of the currents coincide precisely with the movements of the barometer, and the outward current is made to blow a "buzzer," which is said to be audible at a mile distance.[1] In the years 1879-80 a series of interesting experiments on one of these wells, situated near Solberge, three and a half miles south of Northallerton, was made by Mr. Thomas Fairley, F.R.S.E.[2] After stating that the water has a composition similar to that coming from chalk or limestone, and that, though on the first opening of the fissure a violent outburst of choke-damp had taken place, the gas subsequently issuing did not differ appreciably from common air, Mr. Fairley gives a detailed account of observations made on the volume of air passing. The currents passed through fissures in the sandstone at a depth of forty-five feet from the surface of the ground, and just above the level of the water. The measurements were made firstly by a vane-anemometer, and subsequently by two large dry meters, constructed to pass 3,000 cubic feet per hour; these had been substituted for two of the largest meters in the possession of the Leeds Corporation, which had been thrown out of gear by their incapacity to pass the air fast enough. As a result of these experiments it was found that a fall of the barometer of 0·26 inch was accompanied by an outflow of 83,900 cubic feet of air, and by an application of Boyle's law it was calculated the total capacity of the fissures must amount to nearly 10,000,000 cubic feet.

The existence of currents obeying the same laws is equally obvious in a well at Langton at a few miles distance. The well has been long disused, and the water is exceedingly foul; notwithstanding which a candle burns clearly at the bottom. A third instance occurs at Ornhams near Boroughbridge, where the roar of the air-currents passing into the crevices of the rock has been compared by a workman to that of the water in a mill-race. No observations, further than those necessary to prove the existence of the currents, have yet been made on these wells.

At Hopwas a well has been sunk for the supply of Tamworth to a depth of 168 feet, the water standing naturally a depth of 129 feet. The shaft passes through alternations of shale and sandstone, one of the beds of the latter, met with at a depth of ninety-six feet eight inches, being described as "light fissured sandstone thirty feet four inches."[3] From a fissure in this bed, at 115 feet from the surface, there issued a violent rush of atmospheric air, which soon spent itself, and was succeeded by currents showing variations coincident with the barometic changes. The currents have been noticed in one fissure only, an irregular opening, of two and a half inches in height by one inch in width, in a nearly close-sided vertical joint. Experiments on the amount of air traversing this fissure are now in progress.

The same properties are exhibited in an equally well-marked degree in a well belonging to Mr. A. Potts at Hoole Hall, near Chester. The well is eighty-one feet deep and contains ten feet nine inches of water; it is sunk through glacial deposits, consisting of a tough clay overlying a sand of variable thickness, into the New Red Sandstone, but, being an old well and lined with brick to the water-level, the exact nature of the strata and the position of the fissures is unknown. Communicating with the interior of the well by pipes, are two whistles of a different tone, and a pressure gauge; the deeper-toned whistle sounds to an inward, the shriller-toned to an outward current, and were they allowed to act freely during unsettled weather, these whistles would render sleep in the adjoining house impossible. It is stated by Mr. Potts that changes in the atmospheric pressure are shown more rapidly by the pressure gauge of the well than by a mercurial barometer, and that whenever there is a sudden change for rain, the water in the well becomes agitated and slightly discoloured. An appearance of ebullition was noticed also in the Solberge well, but has been attributed by Mr. Cameron to the falling of fragments of mortar. The movements of the water in the Hoole well are being made the subject of experiment by Mr. Potts. Similar, though less powerful, currents have been observed in two other wells within a distance of 500 yards of Hoole Hall. The wells are in a situation where a similar sequence of glacial deposits probably exists, but further particulars are at present wanting.

The fissures from which the currents in blowing wells issue occur usually near, but just above, the water-level. Above them there is provided an air-tight covering in the glacial clays, or in beds of shale interstratified with the sandstone, cutting off communication with the open air in this direction. Fissures traversing a dry sandstone in such a situation constitute an air-chamber which may clearly be of great capacity. On cutting one of a system of connected fissures, the first effect is frequently to liberate a quantity of pent-up air or choke-damp, as at Solberge; subsequently the opening becomes the sole channel by which equilibrium is preserved between the enclosed air and the atmosphere. It would however seem as probable that the opening should occur below the water-level as above it. In such a case the first effect of an expansion of the pent-up gases would be to force out water, and raise the level of the water in the well. The agitation of the water noticed in the well at Chester is probably due to the openings being partly above and partly just below the surface of the water. That they not infrequently are wholly below appears probable from observations on springs and wells, for it has been noticed that in certain chalk-springs there is an increase in the amount of water flowing when there is a rapid fall in the barometer, though no rain may have fallen, and that under the same circumstances water recommences to flow from land-drains and percolation gauges. The gaugings of deep wells in the chalk have confirmed these observations and show that there is a rise in the water-level under a decrease of atmospheric pressure. These movements have been attributed to the expansion of the dissolved gases.[1] It is probable that the gases when given off by the water, rise into and occupy cavities from which there is no escape upwards.

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[1] J. Rofe, F.G.S., *Geological Magazine*, vol. iv p 106, 1867.

[1] A. G. Cameron, *Geological Magazine*, vol. vii. p. 95, 1880.

[2] *Proc.* York Geol. and Polyt. Soc., N.S., vol vii p 409, 1881.

[3] Mr. H. J. Marten, Eighth Report on the Circulation of Underground Waters to the British Association, 1882.

It is noticeable however that five certainly, and two probably, of the blowing wells described above derive their properties from fissures in the New Red Sandstone; no case is known in either chalk or limestone, though these are soluble rocks peculiarly liable to contain caverns or widened joints. It is not improbable that the fissures are too numerous in these rocks, so that wherever large hollows occur, there are also communications upwards with the open air. In sandstone on the other hand large hollows are of extremely rare occurrence, and in view of this difficulty it has been suggested that the Magnesian Limestone which underlies it about Northallerton, at a depth of about 400 feet, and is known to be extremely cavernous, may have given way in places, and led to the formation of hollows in the sandstone. This explanation however is not applicable to the wells at Tamworth or Chester, where the sandstone is not underlain by limestone. It seems more probable that the strength of the air-currents should be taken in connection with the copiousness of the water-supply as indicative of the great extent of small ramifying fissures in some of the triassic sandstones. That the united capacity of such fissures must be very great to account for the phenomena is undeniable. The volume of air contained in the cavities at the Solberge well was estimated at about 10,000,000 cubic feet, or as much as would fill a chamber measuring 217 feet each way, length, width, and height.[2] In making this estimate no allowance was made for aqueous vapour, or for air held in solution in the water, both of which would come off in increased quantities with a decreasing pressure. The former was known by the state of the meter to have been present in large quantities. But making every allowance for these causes of error, it is impossible to escape the conclusion that the fissures, small as they are individually, must in the aggregate form a reservoir of immense capacity.

In concluding these remarks we may refer to the practical application of the knowledge of these properties in fissures. It has been noticed that the drains of large works begin to smell on the approach of rain, and there can be little doubt that this is partly due to the setting up of an outward current corresponding to a fall in the barometer. In fact every network of covered drains, and every covered cess-pool, where special provision for ventilation is not made, must constitute a natural blowing well. It is not our intention to discuss here the engineering details of drainage. It is sufficient to point out that by a faulty system of ventilation, or by the derangement of a system originally good, sewer-gas might be forced into a house with every fall of the barometer.

Lastly we would allude to the effect of the barometer on the escape of fire-damp from coal-seams. Coal is a rock subject to jointing; seams are not only broken through and displaced by faults, but for some distance from the main fracture are traversed by joints and smaller shifts resulting from the general strain. A brief visit to a fiery portion of a mine is sufficient to show the part played by these small clefts. On every side is heard the monotonous hissing or bubbling of the escaping gas, often accompanied by the deeper note of a "blower," or one of those larger channels often observed in connection with faults. The gas is continously given off as a result of a slow decomposition taking place in the coal, and the amount that comes off indicates a great extent of connected fissuring. For though cavities charged with gas under pressure and liable to exhaustion are found, yet large "blowers" commonly continue active for years, and must therefore drain a large area of the seam. While the movement of the gas in the blower differs from that of the air in sandstone fissures, in being always in one direction, namely outwards, it is at the same time evident that the same cause which induces an outward current in the well would cause an increase in the outward current from the coal. The increase would be proportional to the capacity of the fissures; a fall in the barometer from thirty to twenty-nine inches for example would cause $\frac{1}{30}$th of the body of gas stored in the fissures to be added to the ordinary outflow. The liability to explosion with a falling glass has long been a subject of observation. When it is considered that a wide margin is usually allowed in the ventilation to ensure the sufficient dilution and removal of fire-damp, and that a number of other contingencies may bring about an explosion, it becomes evident that a powerful cause must be operating to make the influence of the barometric changes perceptible.

A. STRAHAN

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 25.—"Internal Reflections in the Eye," by H. Frank Newall, B.A.

The author in this paper describes the appearance and investigates the cause of a faint light seen under certain circumstances now to be related:—Stand opposite a uniformly dark wall in a darkened room. Direct the eye to any point in front, and keeping the eye fixed, and being ready to perceive any appearance out of the line of direct vision without moving the eyes towards it, hold up a candle at arm's length, and move it to and fro over about two inches on a level with the point fixed, and a little to the right or left of it. The faint light may be seen moving with a motion opposite to that of the candle on the other side of the point of direct vision.

Near inspection of the light shows it to be an inverted image of the candle, about equal in size, very faint.

Reasons related in the paper lead the author to offer the following explanation: the physical cause of the faint light or "ghost," is light which, proceeding outwards from the image of the candle, formed on the retina by the lens, is reflected back on to the retina by the anterior surface of the lens. This second image is "referred" outwards, and seems as if produced by a faint source of light outside the eye.

The effects of alterations of the state of accommodation on the appearance of the ghost are described; the question as to whether the retina is to be regarded as a screen or as a *regular* reflector is discussed; and the results of calculations based on numbers given by Helmholtz for his schematic eye are noted as forming a difficulty in the explanation.

If the candle be replaced by sunlight, further observations are to be made: (1) signs of the faulty centering of eye-surfaces, as shown by the fact that the sun and its ghost do not arrive at the centre of the field of vision together; (2) signs of oblique reflection at a concave mirror, as shown by the fact that the ghost is circular in only one state of accommodation, whilst in other states it is extended either in a horizontal direction for near focus, or in a vertical direction for distant focus.

To about four out of fifteen persons the author has failed to show the ghost; but no relation is as yet observed between the visibility of the ghost and the kind of sight of the observer, as defined by the ordinary terms, long- and short-sightedness.

A second "ghost," probably due to reflections entirely within the lens, is referred to in the paper: but this, on account of its indistinctness, has not been investigated, except to establish the fact that its motion is the same in direction as that of the candle in the circumstances above related.

February 1.—On the Electrical Resistance of Carbon Contacts, by Shelford Bidwell, M.A., LL.B.

The experiments described in the paper were undertaken with the object of investigating the changes of resistance occurring in carbon contacts under various conditions.

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[1] Baldwin Latham, Report of the British Association for 1881, p. 614.

[2] *Proc.* York Geol. and Polyt. Soc., *op. cit.*

A short movable carbon rod was placed across and at right angles to a similar rod which was fixed in a horizontal position, and arrangements were made for varying and accurately measuring the pressure of the one upon the other, for varying and measuring the current passing through them, and for measuring the resistance at the points of contact. The following are the most important results arrived at:—

1. *Carbon Contacts.*—Changes of pressure produce proportionately greater changes of resistance with small pressures than with great pressures. Thus, when the pressure was increased from ·25 to ·5 grms., the resistance fell from 16·1 to 11·0 ohms., the difference being 5·1 ohms; whereas, when the pressure was increased from 25 to 50 grms., the resistance fell from 2·1 to 1·8 ohms, the difference being only ·3 ohms.

Changes of pressure produce proportionately greater changes of resistance with weak currents than with strong currents. Thus when the pressure was increased from ·25 to ·5 grm., the resistance fell from 9·27 to 8·45 ohms with a current of ·1 ampere; and from 25·50 to 17·75 ohms with a current of ·001 ampere.

Changes of current, the pressure remaining constant, produce greater changes of resistance with small currents than with large currents, and with light pressures than with heavy pressures.

When the resistance of a carbon contact has been reduced by an increase of pressure, it will, on the removal of the added pressure, rise to approximately its original value.

The passage of a current whose strength does not exceed a certain limit, depending upon the pressure, causes a permanent diminution in the resistance (so long, of course, as the contacts are undisturbed), and the stronger the current, the greater will be such diminution.

When the strength of the current exceeds a certain limit, the resistance is greatly and permanently increased (generally becoming infinite). The greater the pressure, the higher will be such limit.

Unless special means are adopted for maintaining a constant current, the fall in the resistance which attends increased pressure is greater than that which is due to increased pressure alone being partly due also to the increased current.

It is not proved that the diminished resistance which follows an increase of current is an effect of temperature.

2. *Metallic Contacts.*—For the sake of comparison, a few experiments were made with metals. The metal principally used was bismuth, which was selected on account of its high specific resistance, but experiments were also made with copper and platinum.

In the case of bismuth, and probably of other metals:—With a given pressure, the weaker the current the higher will be the resistance. This effect is most marked when the current is small. Thus, with a pressure of ·1 grm. the resistance, with a current of ·1 ampere, was 2 ohms; with ·01 ampere it was 16·92 ohms; and with a current of ·001 ampere it was 143·3 ohms. With a pressure of ·5 grm., the resistance with the same currents as before was 1·45, 1·47, and 3·8 ohms.

The passage of a current, even when very small, causes a permanent adhesion between metallic contacts. This effect had been previously observed by Mr. Stroh.

Increase in the current is accompanied by a fall of resistance, and if the current be again reduced to its original strength, the resulting change in the resistance will be small, and it will in no case return to its original value.

Diminution in the strength of the current is followed by a small fall in the resistance if the metal is clean, and by a small rise in the resistance if the metal is not clean.

Increased pressure produces a greater fall in the resistance with small pressures than with great pressures, and with weak currents than with strong currents.

The resistance, after having been reduced by increased pressure, does not return to its original value when the added pressure is removed.

3. *Reasons for the Superiority of Carbon over Metal in the Microphone.*—The above observations may perhaps furnish an answer to the question, Why does carbon give far better results than any metal when used in the microphone? The mere fact that a current causes delicately-adjusted metal contacts to adhere to each other seems sufficient to account for the superior efficiency of carbon. In addition to this phenomenon of adhesion, and probably connected with it, are the facts that metallic contacts, unlike those of carbon, do not even approximately recover their original resistance when once it has been reduced by increased pressure or increased current, unless indeed complete separation occurs; and even the initial effect of pressure upon resistance is in general much more marked with carbon than with metals.

Lastly, it is to be noticed that in the case of carbon, pressure and current act in consonance with each other: pressure diminishes the resistance, and in so doing, increases the strength of the current; and the current thus strengthened effects a further diminution in the resistance. In the case of metals, on the other hand (or at least in the case of clean bismuth) pressure and current tend to produce opposite effects. The resistance is diminished by pressure, and the current consequently strengthened; but by reason of the increased strength of current, the resistance is *higher* than it would have been if the current had remained unchanged. The effect of this antagonism is not very great, but it seems sufficient to give a material advantage to carbon.[1]

The paper contains fifteen tables, four curves, and three diagrams, illustrative of the apparatus used.

February 8.—"Note on Terrestrial Radiation." By John Tyndall, F.R.S.

On Hind Head, a fine moorland plateau about three miles from Haslemere, with an elevation of 900 feet above the sea, I have recently erected a small iron hut, which forms, not only a place of rest, but an extremely suitable station for meteorological observations. Here, since the beginning of last November, I have continued to record from time to time the temperature of the earth's surface as compared with that of the air above the surface. My object was to apply, if possible, the results which my experiments had established regarding the action of aqueous vapour upon radiant heat.

Two stout poles about 6 feet high were firmly fixed in the earth 8 feet asunder. From one pole to the other was stretched a string, from the centre of which the air thermometer was suspended. Its bulb was 4 feet above the earth. The surface thermometer was placed upon a layer of cotton wool, on a spot cleared of heather, which thickly covered the rest of the ground. The outlook from the thermometers was free and extensive; with the exception of the iron hut just referred to, there was no house near, the hut being about 50 yards distant from the thermometers.

On November 11, at 5.45 p.m., these were placed in position, and observed from time to time afterwards. Here are the results:—

6 p.m.	...	Air 36° Fahr.	...	Wool 26° Fahr.
8.10 ,,	...	,, 36	...	,, 25
9.15 ,,	...	,, 36	...	,, 25

air almost dead calm, sky clear, and stars shining.

November 12, the wind had veered to the east, and was rather strong. The thermometers, exposed at 5 p.m., yielded the following results:—

5.15 p.m.	...	Air 38°	...	Wool 33°
5.45 ,,	...	,, 38	...	,, 34
6.45 ,,	...	,, 38	...	,, 35
9 ,,	...	,, 39	...	,, 36

During the first and last of these observations the sky was entirely overcast, during the other two a few stars were dimly visible.

On November 13, 25, and 26, observations were also made, but they presented nothing remarkable.

It was otherwise, however, on December 10. On the morning of that day the temperature was very low, snow a foot deep covered the heather, while there was a very light movement of the air from the north-east. Assuming aqueous vapour to play the part that I have ascribed to it, the conditions were exactly such as would entitle us on *a priori* grounds to expect a considerable waste of the earth's heat. At 8.5 a.m. the thermometers were placed in position, having left the hut at a common temperature of 35°. The cotton wool on which the surface thermometer was laid was of the same temperature. A single minute's exposure sufficed to establish a difference of 5° between the two thermometers. The following observations were then made:—

8.10 a.m.	...	Air 29°	..	Wool 16°
8.15 ,,	...	,, 29	...	,, 12

Thus, in ten minutes, a difference of no less than 17° had established itself between the two thermometers.

Up to this time the sun was invisible: a dense dark cloud, resting on the opposite ridge of Blackdown, virtually retarded his rising.

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[1] In April, 1882, the author communicated this observation to Mr. Preece, who referred to it in a paper read at the Southampton meeting of the Brit. Assoc., on "Recent Progress in Telephony."

8.20 a.m. ...	Air 27° ..	Wool 12°
8.30 „ ...	„ 26 ...	„ 11
8.40 „ ...	„ 26 ...	„ 10
8.45 „ ...	„ 27 ..	„ 11
8.50 „ ...	„ 29 ...	„ 11

During the last two observations, the newly-risen sun shone upon the air thermometer. As the day advanced, the difference between air and wool became gradually less. From 18° at 8.50 a.m., it had sunk at 9.25 to 15°, at 9.50 to 13°, while at 10.25, the sun being unclouded at the time, the difference was 11°; the air at that hour being 31° and the wool 20°.

In the celebrated experiments of Patrick Wilson, the greatest difference observed between a surface of snow and the air 2 feet above the snow, was 16°; while the greatest difference noticed by Wells during his long-continued observations fell short of this amount. Had Wilson employed swansdown or cotton wool, and had he placed his thermometer 4 feet instead of 2 feet above the surface, his difference would probably have surpassed mine, for his temperatures were much lower than those observed by me. There is, however, considerable similarity in the conditions under which we operated. Snow in both cases was on the ground, and with him there was a light movement of the air from the east, while with me the motion was from the north-east. The great differences of temperature between earth and air, which both his observations and mine reveal, are due to a common cause, namely, the withdrawal of the check to terrestrial radiation which is imposed by the presence of aqueous vapour.

Let us now compare these results with others obtained at a time of extreme atmospheric serenity, when the air was almost a dead calm, and the sky without a cloud. At 3.30 p.m., January 16, the thermometers were placed in position, and observed afterwards with the following results:—

3.40 p.m. ...	Air 43° ...	Wool 37°
3.50 „ ...	„ 42 ...	„ 35
4 „ ...	„ 41 ...	„ 35
4.15 „ ...	„ 40 ...	„ 34
4.30 „ ...	„ 38 ..	„ 32
5 „ ...	„ 37 ...	„ 28
5.30 „ ...	„ 37 ...	„ 30
6 „ ...	„ 36 ...	„ 32

These observations, and especially the last of them, merit our attention. There was no visible impediment to terrestrial radiation. The sky was extremely clear, the moon was shining; Orion, the Pleiades, Charles's Wain, including the small companion star at the bend of the shaft, the north star, and many others, were clearly visible. On no previous occasion during these observations had I seen the firmament purer; and still, under these favourable conditions, the difference between air and wool at 6 p.m. was only 4°, or less than one-fourth of that observed on the morning of December 10.

We have here, I submit, a very striking illustration of the action of that invisible constituent of the atmosphere, to the influence of which I drew attention more than twenty-two years ago. On December 10 the wind was light from the north-east, with a low temperature. On January 16 it was very light from the south-west, with a higher temperature. The one was a dry air, the other was a humid air; the latter, therefore, though of great optical transparency, proved competent to arrest the invisible heat of the earth.

The variations in the temperatures of the wool recorded in the last column of figures are, moreover, not without a cause. The advance of temperature from 28° at 5 p.m. to 32° at 6 p.m., is not to be accounted for by any visible change in the atmosphere, or by any alteration in the motion of the air. The advance was due to the intrusion at 6 p.m. of an invisible screen between the earth and firmament.

As the night advanced the serenity of the air became, if possible, more perfect, and the observations were continued with the following results:—

6.30 p.m. ...	Air 36° ...	Wool 31°
7 „ ...	„ 36 ...	„ 28
7.30 „ ...	„ 35½ ...	„ 28
8 „ ...	„ 35 ...	„ 26
8.30 „ ...	„ 34 ...	„ 25
9 „ ...	„ 35 ...	„ 27
10 „ ...	„ 35 ...	„ 28
10.30 „ ...	„ 35 ...	„ 29

After this last observation, my notes contain the remark, "Atmosphere exquisitely clear. From zenith to horizon cloudless all round."

Here, again, the difference of 4° between the temperature of the wool at 8.30 p.m., and its temperature at 10.30 p.m., is not to be referred to any sensible change in the condition of the atmosphere.

The observations were continued on January 17, 23, 24, 25, and 30; but I will confine myself to the results obtained on the evening of the day last-mentioned. The thermometers were exposed at 6.45 p.m., and by aid of a lamp read off from time to time afterwards.

7.15 p.m. ...	Air 32° ...	Wool 26°
8 „ ...	„ 31 ...	„ 26
9.30 „ ..	„ 31 ...	„ 27

During these observations the atmosphere was very serene. There was no moon, but the firmament was powdered with stars. The serenity, however, had been preceded by heavy rain, which doubtless had left the atmosphere charged with aqueous vapour. The movement of the air was from the south-west and light. Here again, with an atmosphere at least as clear as that on December 10, the difference between air and wool did not amount to one-fourth of that observed on the latter occasion.

The results obtained on February 3 were corroborative. The thermometers were exposed at 6.15 p.m.

7.15 p.m. ...	Air 34° ...	Wool 28°
8.25 „ ...	„ 34 ...	„ 30

Here again, the difference between air and wool is only 4°, although the sky was cloudless, and the stars were bright. The movement of the air was from the south-west and light.

On the forenoon of this day there had been a heavy and persistent rain storm. Heavy rain and high wind also occurred on the night following. The serene interval during which the observations were made lay, therefore, between the two storms. Doubtless the gap was well filled with pure aqueous vapour.

Further observations were made in considerable numbers, but they need not here be dwelt upon, my object being to illustrate a principle rather than to add to the multitudinous records of meteorology. It will be sufficient to say that, with atmospheric conditions sensibly alike, the waste of heat from the earth varies from day to day; a result due to the action of a body which escapes the sense of vision. It is hardly necessary for me to repeat here my references to the observations of Leslie, Hennessey, and others, which revealed variations in the earth's emission for which the observers could not account. A close inspection of the observations of the late Principal Forbes on the Faulhorn proves, I think, that the action of aqueous vapour came there into play, and his detection of this action, while unacquainted with its cause, is in my opinion a cogent proof of the accuracy of his work as a meteorologist.

Postscript.—In the *Philosophical Transactions* for 1882, Part I. p. 348, I refer to certain experiments executed by Prof. Soret of Geneva. My friend has recently drawn my attention to a communication made by him to the French Association for the Advancement of Science in 1872. It gives me great pleasure to cite here the conclusions at which he has arrived.

"The influence of humidity is shown by the whole of the observations; and it may be stated generally that, other circumstances being equal, the greater the tension of aqueous vapour the less intense is the radiation.

"In winter, when the air is drier, the radiation is much more intense than in summer, for the same height of the sun above the horizon.

"On several occasions a more intense radiation has been observed in dry than in humid weather, although the atmosphere was incontestably purer and more transparent in the second case than in the first.

"The maximum intensity of radiation, particularly in the summer, corresponds habitually to days exceptionally cold and dry."

Such are the results of experiments, executed by a most excellent observer, on the radiation of the sun. They apply word for word to terrestrial radiation. They are, moreover, in complete harmony with the results published by General Strachey in the *Philosophical Magazine* for 1866. Meteorologists will not, I trust, be offended with me if I say that from such outsiders, fresh to the work and equipped with the necessary physical knowledge, they may expect efficient aid towards introducing order and causality among their valuable observations.

Mathematical Society, February 8.—Prof. Henrici, F.R.S., president, in the chair.—Capt. P. A. MacMahon, R.A., was admitted into the Society.—The following communications were made:—On the Sylvester-Kempe quadruplane, by Mr. H. Hart.—On curves obtained by an extension of Maclaurin's method of constructing conics, by Mr. S. Roberts, F.R.S.—A generalisation of the "nine-point" properties of a triangle, by Capt. P. A. MacMahon.—On the use of certain differential operators in the theory of equations, by Mr. J. Hammond.—A method for reducing the differential expression $dt/\sqrt{\{t-\alpha, t-\beta, t-\gamma, t-\delta\}}$ to the standard form, by Mr. J. Griffiths. The "nine-point" property was the following:—If through the centre of the circle ABC, and the ortho-centre of the triangle ABC, lines be drawn making angles α and $\pi-\alpha$ with the sides of the triangle, twelve points will be obtained on the sides, and these lie six and six on two circles of radius $\frac{1}{2}R$ cosec α. Each circle also passes through six other points, and they are inscribed circles of the two three-cusped hypocycloids, which are the envelopes of the two tangents, equally inclined to the axis (at angles α), to a parabola inscribed in the triangle ABC. Of course, when $\alpha = \frac{\pi}{2}$, the circles become the ordinary nine-point circle of ABC.

Linnean Society, February 1.—Sir John Lubbock, Bart., F.R.S., president, in the chair.—Messrs. F. W. Burbidge and Joseph Johnson were elected Fellows of the Society.—Dr. W. C. Ondaatje called attention to examples of red coral from Ceylon.—Mr. W. T. Thiselton Dyer exhibited a model of the fruit of the Double Cocoa-nut (*Lodoicea Seychellarum*, Lab.), of an unusual form, obtained from Major-General C. G. Gordon, R.E.—A series of microscopic sections of coal-plants were shown on behalf of Mr. J. Norman.—The following paper was then read:—On the structure, development, and life-history of a tropical epiphyllous lichen, by H. Marshall Ward. The author's observations lead him to believe that the epiphyllous cryptogam in question supports the view that a lichen is a compound organism composed of an alga on which an ascomycetous fungus has become more or less intimately affixed and dependent. It is developed on the leaves of many plants, but it has been more closely watched on *Michelia furcata*. The lichen presents four types, orange-red stellate patches, greyish-green blotches, clear grey spots, and white shining circles, but these pass imperceptibly into one another, and vary in size from a speck to a quarter of an inch in diameter. The reddish spots of the earlier stages is an alga of which the radiating filaments are in part reproductive organs, and in part barren hairs. It subsequently passes into the grey and green stages, and by a modification of growth the invasion of a fungus mycelium succeeds. The white matrix of the complete lichen consists of the same algal thallus invested by dense masses of the fungus hyphæ, which produce shining black dots, viz. the fruit bodies. The author describes in detail the peculiarities of growth and reproduction of the alga and fungus, and formation of the lichen. He alludes to and criticises Dr. Cunningham's account of *Mycoidea parasitica*, which latter is evidently closely related to that described by himself. Assuming that Mycoidea and Ward's Alga are generically the same, either Cunningham discovered a female organ of reproduction which becomes fertilised and produces zoospores, or he confounded these with certain fertile hair organs described by Ward. As regards the systematic position of the alga, a comparison with *Coleochæta* suggests that there is very little in common beyond mode of growth of the disc-like thallus, and the production of zoospores from certain cells. The genus *Chroolepus*, moreover, presents features which agree in several important points, viz., orange-red oily-cell contents, habitat, production of zoospores in ovoid cells developed terminally and laterally. The structure of the thallus, and relative positions of the main masses of fungal and algal portions, agree with what occurs in heteromerous crustaceous lichens, as Graphidea; but the perithecia indicate an angiocarpous alliance, bringing this form nearer such families as Pertusaria and Verrucaria, to the latter of which it may ultimately be referred.—A paper was read by F. Maule Campbell, on the pairing of *Tegenaria Guyonii*, and description of certain organs in the male abdominal sexual region. Two cases are related in which during confinement the males killed the females after union, and an instance is also given of an attempt to impregnate an immature female which was also destroyed by the male. In neither case could hunger have been the cause of the attack. The writer explains the occurrences, and also the accounts of females destroying males after union, on the ground "that those instincts which are habitually practised throughout the far greater portion of the life of the species, and on which it is dependent, would scarcely be suspended for a longer period than necessary for the sexual union." Some of the habits of spiders, and especially of this species, are mentioned as bearing on these sexual conflicts, and the specific benefits which would arise from them are referred to. The paper concluded by a note on some glands (probably for spinning) situated on the convexity of the abdominal sexual region. The ducts are considerably convoluted, and open through transparent tubular spines which are arranged transversely to the axis of the body of the spider. Two papilla-like processes below the opening of the genital sinus are described.

Zoological Society, February 6.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—A letter was read from Mr. F. C. Selous, dated from the Matabele Country, on the possibility of obtaining a White Rhinoceros.—Extracts were read from a letter received from the Rev. G. H. R. Fisk, C.M.Z.S., of Cape Town, giving an account of the habits of some reptiles which he had had in captivity.—A communication was read from Messrs. Salvin and Godman, containing the description of a new species of Pigeon of the genus *Otidiphaps* from Ferguson Island, one of the D'Entrecasteaux group, which they proposed to call *O. insularis*.—Mr. Sclater read some further notes on *Tragelaphus gratus*, and exhibited drawings of both sexes of this antelope, taken from specimens living in the Menagerie of the Jardin des Plantes, Paris.—A communication was read from Mr. E. W. White, F.Z.S., containing some supplementary notes to a former paper on the birds of the Argentine Republic.—A communication was read from the Rev. G. A. Shaw, containing some notes on the habits of an Aye-Aye which he had had in confinement for several months, and other information respecting this animal.—Mr G. A. Boulenger, F.Z.S., read a paper containing the description of a new species of Lizard of the genus *Enyalius* from Peru, which he proposed to name *E. palpebralis*.

BERLIN

Physiological Society, January 12.—Prof. du Bois Reymond, in the chair.—Dr. Falk read a contribution upon the phenomenon lately demonstrated by experiments on animals, that great oedema of the lungs can be produced in a very short time, even in a quarter of an hour, by compressing or otherwise interrupting the function of the left side of the heart; whereas a similar action on the right side does not produce such an effect. Dr. Falk has had an opportunity in two post-mortems of proving the correctness of these observations in respect to man. In one of these cases, a strong, healthy man died in consequence of a discharge of shot, and the post-mortem showed that the cause of death was the penetration of a shot into the wall of the left ventricle. The lung of this previously healthy man exhibited a high degree of oedema. In the second case, a healthy railway-workman was killed by a blow of a buffer upon the chest. The post-mortem showed a rupture of the right ventricle to have been the cause of death; the lungs which were carefully examined, did not show a trace of oedema.—Dr. W. Wolff described the structure of the tactile corpuscles, according to his researches, they contain no nerve-branchings, but consist of a rugose sheath, granular contents, and the free ends of the entering nerve-fibres. In opposition to other histologists he further found the epithelium-cells which he studied in the corneæ of small mammals to be devoid of nerves; and in agreement with this he has always found gland-cells to be without nerve. The sympathic nerve-fibres which enter into the glands according to Dr. Wolff always end in unstriped muscles.—Prof. Kronecker reported on experiments of Dr. Meiss, upon the irritability of the heart under abnormal conditions of nutrition. During experiments, undertaken to study the comparative effects of concentrated and diluted blood upon the frog's heart, and which established the occurrence of a more energetic activity by nutrition with concentrated blood, certain remarkable deviations occasionally occurred from the general law that frog's hearts (always) respond to every stimulation with maximal contractions. These deviations consisted in the occurrence of smaller sub-maximal beats between the maximal beats. Further investigation of this appearance led to the conclusion that this was a result of abnormal conditions of nutrition which was not easily or certainly to be produced. These sub-maximal contractions and the irregular pulse, were chiefly observed when passing a current of asphyxiated blood through the heart, but they always disappeared on supplying the heart with fresh blood.

Physical Society, January 19.—Prof. Roeber in the chair.—Prof. Schwalbe supplemented his former communications to the Society on ice-caves, with additional facts he had recently come to know through literature. He noted, as a most interesting phenomenon, that the occurrence of ice-caves was not confined to limestones, basalts, and lavas, but that they have also been observed in gypsum-hills. Thus, in the gypsum-hill Illetzkaja Satscha, in the Southern Ural, is an ice-cave which Murchison visited in August; situated in the street, it was closed with a simple wooden door, and it was utilised by the inhabitants of the district as a store-room. Its temperature was so low that the drinks kept in it only a short distance from the mouth were frozen. And as with all other caves distinguished by their low temperature and ice-formation, it was stated of this one, that in winter the air in it was very warm, so that people slept in it at night without requiring their sheep-skin furs. Murchison had applied to Sir John Herschel for an explanation of the phenomenon, and Herschel had offered the hypothesis, that it was a case of cold and heat waves, which, penetrating inwards from the surface of the earth, were retarded, and so caused the low temperature in summer and the warmth in winter. Prof. Schwalbe, however, has convinced himself that this explanation is inadequate; for the summers in which ice-caves have been visited and found filled with ice, have been preceded by very mild winters, *e.g.* the winter 1881-2 was very mild, yet the ice-cave in Bohemia, which he had himself visited, was covered with ice; besides, the retardation of cold several months is very improbable. Before a sufficient explanation of these peculiar conditions of temperature can be reached, continuous scientific observations must be made, for a long time, of the course of the temperature throughout the year. As to the warmth of air in the caves in winter, and the melting of the ice in winter, there are only observations by lay-persons, which, however, strikingly agree, even in the assertion that the ice-formation regularly takes place on a large scale in the month of May. With regard to the immediate cause of the ice formation no one (according to the author) can be in doubt, who has visited an ice-cave, and seen how the dropping water from the roof solidifies directly on falling. Water that has trickled through is over-cooled, and solidifies just as after falling to the ground, even when it meets a different solid body; the ice, further, is only met with where water drops. The strong cooling of the water and of the rock through which it has trickled, is, perhaps, connected with the process of filtration through the earth-strata. On this point experimental research must decide, following up the investigation made by Jungk in 1865, who observed a lowering of temperature in filtration of water through porous partitions. Such laboratory experiments, exact long-continued temperature-observations in accessible ice-caves, and topographical examinations of as many as possible of these caves (which are not rare), will surely bring about a solution of this still obscure natural problem.

PARIS

Academy of Sciences, February 5.—M. Blanchard in the chair.—The following papers were read:—On the physical and mechanical constitution of the sun (third and last part), by M. Faye. He deals with the depth of spots, the movements of hydrogen and their effects, the height of protuberances and an illusion attending them, the clouds of the photosphere, &c.—M. Hirn presented an analysis of a *brochure* by himself and M. Hallauer. "Refutations of a second critique of M. G. Zeuner." It relates to the theory of steam-engines.—On the spherical representation of surfaces, by M. Darboux.—On the functions satisfying the equation $\Delta F = 0$, by M. Appell.—On the displacement of the sodium lines, observed in the spectrum of the great comet of 1882, by MM. Thollon and Guy. From the displacement observed with a single-prism spectroscope, he had estimated the rate of recession of the comet at from 61 to 76 km. per second. This is confirmed by M. Bigourdan, who, from a determination of the trajectory of the comet, estimates the velocity at the time in question (3 p.m. on September 18) at 73 km. The spectroscope is thus shown to be reliable for the purpose referred to.—Magnetic action of the sun and the planets; it does not produce secular variation in the great axes of orbits: note by M. Quet.—The distribution of energy in the solar spectrum and chlorophyll, by M. Timiriazeff. Prof. Langley finds, with his bolometer, the maximum of radiant energy in the orange, precisely that part of the spectrum which corresponds to the characteristic band of chlorophyll. M. Timiriazeff is studying the quantitative relation between solar energy absorbed by the chlorophyll of a leaf and that stored up through chemical work produced. It appears that the leaf can transform into useful work as much as 40 per cent. of the energy absorbed.—On some combinations of sulphite of magnesia with alkaline sulphites, by M. Gorgeu.—On hydraulic silica; reply to M. Le Châtelier, by M. Landrin.—On the mutual displacements of bases in neutral salts, the systems remaining homogeneous, by M. Menschutkine.—The microbes of marine fishes, by MM. Olivier and Richet. In all the fishes they examined (about 150) there were, in the peritoneal liquid, the lymph, the blood, and consequently in the tissues, microbes more or less numerous, having all the characters of terrestrial microbes, and reproducing similarly. Besides direct observation, the authors had recourse to experiments (1) of cultivation, (2) of occlusion. (In the latter case the fishes, or parts of them, were put into melted paraffine, which, after solidifying, was coated with several layers of collodion and Canada balsam, to protect from atmospheric germs. In a few weeks microbes were always abundantly developed.) The organisms were mostly *Bacillus*.—On the reaction-time of olfactory sensations, by M. Beaunis. He gives numerical results for this quantity (which is the time between sense-excitation and the moment when the person indicates by a signal that he has experienced the sensation) in the case of ten substances: ammonia, acetic acid, camphor, &c. They range from 37 to to 67 hundredths of a second. The time is longer than that for tactile, visual, and auditory sensations (in the author's case shorter than for tactile sensations).—On the respiration of aquatic plants or submerged aquatic-aerial plants, by M. Barthélemy. He considers the phenomena brought forward as proof and measure of the chlorophyllian function merely exceptional, and produced by the mode of experimentation. Under normal conditions, the special respiration of green organs cannot have the universal importance attributed to it.—Note on the morphological nature of the aerial branches of adult *Psilotum*, by M. Bertrand.—Influence of temperature on the production of wheat, by M. Duchaussoy. He gives in a table the yield of wheat in the department of Cher, and the mean temperature of spring and summer, from 1872 to 1881. The descending scale of the yield is nearly that of the mean temperature of summer. The years 1873 and 1876 are exceptions, and their small yield is explained by the dryness of the summer.

CONTENTS

PAGE
THE TERTIARY HISTORY OF THE GRAND CAÑON DISTRICT. By Dr. ARCH. GEIKIE, F.R.S. 357
CENTRAL ASIA. By A. H. KEANE 359
PHYSICAL OPTICS 361
OUR BOOK SHELF:—
  "The Year-Book of Pharmacy" 361
  "Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux" 362
LETTERS TO THE EDITOR:—
  Natural Selection and Natural Theology.—Dr. GEORGE J. ROMANES, F.R.S.; J. B. HANNAY, F.R.S. 362
  Two Kinds of Stamens with Different Functions in the Same Flower.—Dr. FRITZ MÜLLER (*With Illustrations*) 364
  The Markings on Jupiter.—W. F. DENNING (*With Illustration*) . 365
  Meteor of November 17.—H. DENNIS TAYLOR 365
  Aino Ethnology.—Dr. J. J. REIN 365
  Hovering of Birds.—J. R. 366
  Intelligence in Animals.—Dr. J. RAE, F.R.S.; D. PIDGEON . . 366
  The Sea Serpent.—Dr. J. RAE, F.R.S. 366
  The "Zoological Record."—SYDNEY J. HICKSON 366
SIEVE-TUBES 366
CASSELL'S NATURAL HISTORY (*With Illustrations*) 367
THE CONDENSATION OF LIQUID FILMS ON WETTED SOLIDS. By J. W. CLARK 370
THE STOCKHOLM ETHNOGRAPHICAL EXHIBITION.—Dr. A. B. MEYER 371
BARON MIKLOUHO-MACLAY 371
NOTES 371
OUR ASTRONOMICAL COLUMN:—
  The Comet of 1771 374
  The Cassini Division of Saturn's Ring 374
ON THE CHEMICAL CORROSION OF CATHODES. By G. GORE, LL.D., F.R.S. 374
THE MOVEMENTS OF AIR IN FISSURES AND THE BAROMETER. By A. STRAHAN 375
SOCIETIES AND ACADEMIES 376

THURSDAY, FEBRUARY 22, 1883

PROFESSOR HENRY SMITH

ON Friday, the 9th inst., we lost one of our most gifted men. By the death of Prof. Henry Smith there has dropped out from our roll-call a name which was already known among a wide circle of friends and admirers, but which would assuredly have been more widely known and more fully recognised if he had remained longer in our ranks.

Henry John Stephen Smith was born in Dublin, but when he was about two years old his family, at his father's death, removed to England. His precocity from the earliest age was remarkable; but what was perhaps still more remarkable, the talents which he thus showed did not, as is so often the case, fail him in after life. He was a fair-haired child, and was known among his relations as the "white crow." When he was two years old it was understood that he could read; and on his third birthday it was agreed that he should be tried, on the condition that, in the case of failure, the white crow should be allowed to fly out of the window, which was set open for the purpose. It is needless to add that there was no occasion for flight. At the age of four he was found one day lying flat on the floor, with his face raised slightly above his book (his sight being, even then, short) teaching himself Greek from an old-fashioned grammar full of antique contractions in the characters. His subsequent education was carried on until he was eleven by his mother, and then by tutors. For an account of the rapidity with which he galloped over the ground with one of them, we are indebted to an interesting letter in the *Times* of the 12th inst. With a view to his education the family removed to Oxford in 1840, whence he was transplanted to Rugby. He entered the school in August, 1841, the commencement of the last year of Dr. Arnold's Head Mastership, and was in the Boarding House of the late Rev. Henry Highton, who was himself an old Rugbeian, a pupil of Arnold, and Co-Exhibitioner from the school with the present Dean of Llandaff and the late Dean of Westminster, and had lately graduated at Oxford, taking a First Class in Classics and a Second in Mathematics. Henry Smith had been Highton's private pupil at Oxford, and was so well taught that when he entered Rugby he was (although only then fourteen) placed in the fifth form, which is the highest form but one below the sixth, and which, by the rules of the school, is the highest in which a new boy can be placed. He was distinguished at Rugby for his unvarying gentleness of character, and was a favourite alike with masters and boys. An old schoolfellow writes of him thus: "I was a young boy in the house, and remember being struck with his great gentleness and amiability. It did me good at once, and I felt it, as I believe, to my lasting benefit." He was always much attached to his old friend and tutor, Highton; and ever since the latter's death, in December, 1874, no one has shown more kindness to his widow and children than Henry Smith. At Rugby he progressed as rapidly as elsewhere, and was kept back from entering the sixth form under Arnold, only on account of his age. He was the first boy promoted to that form under Dr. Tait, Dr. Arnold's successor. Nothing in fact seemed capable of stopping his intellectual career. The death of his only brother and his consequent withdrawal from school, which would have thrown most boys entirely out of gear, did not interfere with his gaining, at the age of eighteen the scholarship at Balliol. A severe illness delayed his residence at college, but neither the malady itself, nor absence from England, nor severance from books prevented him in 1848, winning the blue ribbon in classics among Oxford undergraduates—the Ireland Scholarship. In 1850 he took his degree, obtaining an old-fashioned "Double First," namely, in classics and mathematics. The next year he gained the Senior Mathematical Scholarship; and if in this he had but few competitors, it was because his strength and powers were already known. After such a University career, almost unparalleled in the annals of Oxford, it seems but a natural consequence that he should be elected, as was the case, to a Fellowship at his College. In 1861 he was elected successor to the late Baden Powell in the Savilian Professorship of Geometry, which chair he retained until his death. With a view to relieving him from the labour and duties of College tuition, which he had faithfully discharged for five-and-twenty years, Corpus Christi College offered him a Fellowship free from such duties. Notwithstanding his regret at leaving (although, as it subsequently proved, temporarily) his old college, he decided, having reference to the growing calls upon his time, to accept the offer. But Balliol, unwilling to lose all connection with its distinguished alumnus, afterwards bestowed upon him an honorary Fellowship, and, under the recent Statutes, a full Fellowship without emolument.

The malady under which he ultimately sank may be considered hereditary, for his father died from the same cause, and the son showed symptoms of it even at an early age. It is idle now to speculate whether a quieter or less exhausting life would have prolonged his years. There is some truth in the idea that a man can first and last perform a certain amount of work and no more. On this supposition it may be even a gain to the individual to have performed his task in the minimum of time, while those who remain must rest thankful at having lived in his day, and having retained him amongst us as long as was the case.

The testimony of his friends to his ability and other qualities is from all quarters abundant. Prof. Huxley writes: "Henry Smith impressed me as one of the ablest men I ever met with; and the effect of his great powers was almost whimsically exaggerated by his extreme gentleness of manner, and the playful way in which his epigrams were scattered about. They were so bright and sharp that they transfixed their object without hurting him. I think that he would have been one of the greatest men of our time, if he had added to his wonderfully keen intellect and strangely varied and extensive knowledge the power of caring very strongly about the attainment of any object." Although the present writer is not likely to differ much from Prof. Huxley in his estimate of the man, he would still suggest that Henry Smith's care for the attainment of an object was measured rather by his estimate of its ultimate value than by its present advantage. For those who knew him best were most fully aware of the effort which it cost him to postpone (as he often did, with apparent readiness) his beloved mathematics to other claims. Another friend says: "He was a man of rare powers, and as guileless as he was richly gifted."

Of some men it is said that they were never young, of others that they became old while their contemporaries were still lads; and it has been stated as a general law, in scientific thought at least, that the best and most original ideas have always been conceived before the age of thirty. But whatever may be the case in this respect with the generality of men, Henry Smith was as young and vigorous in intellect at the age of fifty-six, the limit to which he attained, as he was when he gained the first of his many University honours. It was his freshness of mind, his vivid appreciation and intelligent enjoyment of everything going on, not only in science, but also in life, whether social or political, which made us forget that his years, like ours, were passing away, and that the number of them was finite. It was his genial presence, his sympathetic attention, his ready counsel, his sound judgment, his happy mode of dealing with both men and things, which make us already feel a loss which we cannot as yet fully appreciate, but which we can never hope again completely to replace.

Of many Greek towns it is related that each has claimed for itself the honour of having been the birthplace of Homer; in like manner, many branches of knowledge, and avocations of life, might claim to have been the favourite pursuit of Henry Smith. But however proficient, or even prominent he may have been in other subjects, it was in mathematics that he mainly showed the originality of his genius, and that he has left any permanent record of work of the highest kind.

Among the great works which it was long hoped that he would have accomplished was his treatise on the Theory of Numbers. This subject, which during the present generation has been so marvellously generalised as to undergo a complete transfiguration since it was presented to us in the work of Barlow and in the ordinary educational books on Algebra, formed for many years a serious study on the part of Prof. Henry Smith. The papers in which the researches of mathematicians on this subject are recorded are scattered through the pages of various periodicals, so that it is not easy to realise the steps which each writer has contributed to the general progress, nor to assign to each his relative position. But this is not all, nor even the worst; it has been a prevailing custom, too prevalent we think, among mathematicians of late years, to publish results alone, without proof of their statements, and even without indication of the train of argument which led them to their conclusions. This naturally entails on the part of the reader either a strong act of faith or a difficult and, as we hold, unnecessary effort. It need hardly be added that in endeavouring to digest and present to his readers what had been done by others in his subject, Henry Smith adopted the latter course; and, with a sagacity in which few could have rivalled him, he assimilated all these fragments, and utilising the valuable among these *disjecta membra*, and rejecting the worthless, he brought them into harmony, and was in a fair way to produce from them a structure intelligible in itself, and capable of forming a groundwork for further developments. But while our author was discussing what had been already done, the very materials upon which he was engaged were growing apace, and his self-imposed task accumulated upon him. Of unfinished work, or of "ragged ends" as he used to call them, he was as nearly intolerant as he could be of anything; and it is not clearly known whether he ever made up his mind to complete what he had undertaken up to a certain date or not. In any case what he had already long ago achieved in this matter must have been a gigantic work; and it remains only to hope that his manuscripts have been left in such a state that others may be able to wield the weapons which he had forged.

The results of his preliminary studies were given in his six invaluable Reports on the Theory of Numbers, published in the volumes of the British Association for 1859 and following nearly consecutive years; and these alone are sufficient to show the extent of his reading and the firm grasp which he had of the subject. The following extracts from the first and third of these Reports indicate both the wide range of the theory and the magnitude of the portion which still remains to be achieved:—

"There are two principal branches of the higher arithmetic: the Theory of Congruences and the Theory of Homogeneous Forms. In a general point of view these two theories are hardly more distinct from one another than are in algebra the two theories to which they respectively correspond, namely, the Theory of Equations and that of Homogeneous Functions; and it might, at first sight, appear as if there were not sufficient foundation for the distinction. But, in the present state of our knowledge, the methods applicable to, and the researches suggested by, these two problems, are sufficiently distinct to justify their separation from one another."

"It is hardly necessary to state that what has been done towards obtaining a complete solution of the Representation of Numbers by Forms, and the Transformation of Forms, is but very little compared with what remains to be done. Our knowledge of the algebra of homogeneous forms (notwithstanding the accessions which it has received in recent times [1861]), is far too incomplete to enable us even to attempt a solution of them co-extensive with their general expression. And even if our algebra were so far advanced as to supply us with that knowledge of the invariants and other concomitants of homogeneous forms, which is an essential preliminary to an investigation of their arithmetical properties, it is probable that this arithmetical investigation itself would present equal difficulties. The science, therefore, has as yet had to confine itself to the study of particular sorts of forms; and of these (excepting linear forms, and forms containing only one indeterminate) the only sort of which our knowledge can be said to have any approach to completeness are the binary quadratic forms, the first in order of simplicity, as they doubtless are in importance."

Prof. Smith's sphere of utility was, as indeed is pretty well known, not confined to his University, nor to science as such, but extended, among other directions, even to departments of the State. Passing over the Royal Commissions on Scientific Education and on the University of Oxford, of both of which he was a leading member, mention must not be omitted of the Meteorological Council of which he was chairman. That body, nominated by the Royal Society, but appointed by the Government, holds a position intermediate between a public department and an independent institution. While on the one hand this intermediate position presents some advantages, at all events in the present stage of the subject as a science, it undoubtedly, on the other, requires no inconsiderable tact and judgment in its management. For the yearly administration of a large sum of public money, for the management of a considerable staff at home, and of a variety of observers at out-stations in all parts of the country, and for communication with similar departments of State in foreign countries, science alone would not have sufficed. But at the same time few branches of natural knowledge stand more in need of a strong scientific guide to keep it from the crotchets of dabblers in the subject, or from relapsing into an indiscriminate accumulation of loose observations from which no valuable result can ever be derived. For this post the President and Council of the Royal Society unanimously nominated him, nor had they ever reason to regret the step which they then took.

The case of the Meteorological Council was, however, but one instance out of many in which his name came uppermost in the minds of men when they were looking for a leader, or a chairman, or a president. Whether as President of the Mathematical Society (1874-6), or of the Mathematical and Physical Section of the British Association (1873), or as Chairman of Committees too many to enumerate, he always succeeded in commanding the respect of those with whom he was associated, and in carrying through the business to a satisfactory issue.

In one matter only did he fail of success; but in that case the failure was not really his, but that of those who should have given him support. The case was that of his candidature for the representation of the University of Oxford, when, in 1878, Lord Cranbrook received his peerage. Instances of the candidature of leading University men, both in Oxford and in Cambridge, have not been unknown, from the time of the late Sir John Lefevre to that of Prof. Stuart; but all have terminated in the same result, namely, the total defeat of every man of University distinction, whatever other qualifications he may have for the office. With these instances we may compare, not without interest and instruction, the choice which has been made by the University of London on the only two occasions on which a vacancy has yet occurred.

It was sometimes thought that his mind became diverted from mathematics by his many other distracting avocations; there are, however, reasons for doubting this. It is true that he did not pour out the amount of mathematical papers of which he was certainly capable; but those which he did publish showed that he cared little to add fringe-work to the borders of our knowledge, and that he reserved himself for questions of real importance. We remember his alluding to the subject of one of his later papers contributed to the Mathematical Society, on Modular Equations, as relating to "a point on which people had puzzled themselves for a long time," and the following passages from his address to the London Mathematical Society were certainly not penned by a president for whom that subject had lost its charm. "Of all branches of mathematical inquiry, this is the most remote from practical applications; and yet, more perhaps than any other, it has kindled an extraordinary enthusiasm in the minds of some of the greatest mathematicians." Then he quotes Gauss as having held Mathematics to be the Queen of the Sciences, and Arithmetic to be the Queen of Mathematics. I do not know that the great achievements of such men as Tchébychef and Riemann can fairly be cited to encourage less highly gifted investigators; but at least they may serve to show two things—first, that nature has placed no insuperable barrier against the further advance of mathematical science in this direction; and secondly, that the boundaries of our present knowledge lie so close at hand that the inquirer has no very long journey to take before he finds himself in the unknown land. It is this peculiarity, perhaps, which gives such perpetual freshness to the higher arithmetic. It is one of the oldest branches perhaps the very oldest branch, of human knowledge; and yet some of its most abstruse secrets lie close to its tritest truths. I do not know that a more striking example of this could be found than that which is furnished by the theorem of M. Tchébychef. To understand his demonstration requires only such algebra and mathematics as are at the command of many a schoolboy; and the method itself might have been invented by a schoolboy, if there were again a schoolboy with such an early maturity of genius as characterised Pascal, Gauss, or Evariste Galois."

The following is another instance of the interest which he retained in mathematics to the very last. In the address above quoted he alluded to a problem, at that time still unsolved, in the following terms:—"It was first shown by M. Liouville that irrational quantities exist which cannot be roots of any equation whatever, having integral coefficients. We may perhaps be allowed to designate by the terms arithmetical and transcendental the two classes of irrational quantities which M. Liouville has taught us to distinguish; and it becomes a problem of great interest to decide to which of these two classes we are to assign the irrational numbers, such as ϵ and π, which have acquired a fundamental importance in analysis. To Lambert, the eminent Berlin mathematician of last century, the first great step in this direction is due. He showed that neither π nor π^2 is rational; with regard to ϵ he was even more successful, for he was able to prove that no power of ϵ, of which the exponent is rational, can itself be rational. There (with one slight exception) the question remained for more than a century; and it was reserved for M. Hermite, in the year 1873, to complete, by a singularly profound and beautiful analysis, the exponential theorem of Lambert, and to prove that the base of the Napierian logarithms is a transcendental irrational. But, in a letter to M. Borchardt, M. Hermite declines to enter on a similar research with regard to the number π. 'Je ne me hasarderai point,' he says, 'à la recherche d'une démonstration de la transcendence du nombre π. Que d'autres tentent l'entreprise; nul ne sera plus heureux que moi de leur succès; mais croyez m'en, mon cher ami, il ne laissera pas que de leur en coûter quelques efforts.' It is a little mortifying to the pride which mathematicians naturally feel in the advance of their science to find that no progress should have been made for a hundred years and more toward answering the last question, which still remains unanswered, with regard to the rectification of the circle."

Last year, Lindemann, starting from Hermite's researches, succeeded in supplying the proof required with reference to the number π. And while speaking of this achievement with the satisfaction which his generous nature prompted, Henry Smith added that it was a problem on which he had long fixed his eye with a view to attacking it seriously so soon as he had leisure for the undertaking.

He was doubtless then looking forward to some University vacation; for vacation time formed the period for his original investigations, while term time was devoted to current work and to society, which he himself so keenly enjoyed, and in which he was always an honoured and a welcome guest.

It has been much the fashion of late years to raise memorials to the departed; and in some cases it may be doubted whether a wise discrimination has been exercised in the matter. No one, however, who has any interest in science, would doubt for a moment that the memory of Henry Smith was in the highest degree deserving of perpetuation. But in our opinion the best and only suitable memorial of him will be the publication of his works, in the fullest and most complete form of which they are now capable. And it is sincerely to be hoped that his MSS. may be placed in the hands of a mathematician who may prefix to them an introduction as worthy of these works as was Prof. Smith's introduction to the remains of Clifford.

During his last few years he lived, as Keeper of the University Museum, at the house adjoining the main building, previously occupied by his predecessor, John Phillips. His companion was his sister, whose useful and sympathetic life worthily supplemented his own. It is to be hoped when the wave of sorrow which is now passing over her has in some degree subsided, and when time has brought an alleviation which may now seem impossible, that she may derive satisfaction, although it be a melancholy one, in having learnt through the event how much her brother was appreciated and beloved by many, and by some even unknown, friends.

W. SPOTTISWOODE

PUBLIC ELECTRIC LIGHTING

MUCH attention is being given at the present moment to the operation of the Electric Lighting Act passed during the last session of Parliament. Under the terms of that Act, licenses and provisional orders will be granted to local authorities, companies, and private individuals to supply electricity for the purpose of electric lighting over definite areas. A large number of applications for licenses and provisional orders have already been submitted to the Board of Trade, in a few instances by local authorities, but in the majority of cases by joint-stock companies formed for working one or other of the different systems for electric lighting. A number of the "Provisional Orders" now being promoted lie before us, the majority of them being drawn in almost identical terms. A perusal of these documents cannot fail to impress the reader, firstly, with the great complexity of the question, secondly, with the extreme difficulty of striking a fair balance between vested interests and public convenience, thirdly, with the great amount of knowledge and skill displayed in the drafting of these provisional orders. It is an open secret that not only the main outlines but also most of the details of these orders are from the hand of Mr. J. Fletcher Moulton, F.R.S., whom we must congratulate upon the success with which he has applied himself to the task of preparing them. Now that Parliament is once more in session we shall probably hear of further legislative proposals; but if all provisional orders are as well and as wisely drawn as the majority of those before us appear to be, there can be little doubt that the necessity for separate further legislation and for the promotion of private bills for electric lighting will be removed.

As to the provisional orders themselves it would be impossible within reasonable limits to deal with a tithe of the important topics which are therein set forth. Many of the provisions are naturally directed toward questions of municipal rights and parochial law. Leaving aside all these matters we come to the more scientific points. Four separate systems of distribution are recognised in the provisional orders. These are (*a*) "direct" system, more familiar under the name of distribution in parallel arc, with "distributing mains" throwing off "service lines" for individual consumers; (*b*) "storage" system, with service lines in parallel arc from storage batteries charged in series intermittently from a generating station; (*c*) either of the above with "earth" returns; (*d*) "series" system, supplying customers in one undivided circuit. We may remark in passing that it appears to us that the use of "earth" for return should be in every case forbidden. If currents of the intensity employed for electric lighting are sent through earth in our crowded cities we shall have constant derangements of telegraphs, telephones, electric bells, in fact of all electric appliances which work by feeble currents and use earth returns. Moreover, as "earth" in practice means usually the employment of existing gas-pipes or water-pipes as returns the proposal to utilise "earth" for electric light returns, is doubly to be deprecated. Amongst other limitations set forth in the provisional orders are some which bind the "undertakers" to lay down their mains within two years, some which prescribe the hours during which the supply of currents must be maintained, and some which limit the conditions of supply. Amongst the latter we observe in several of the orders before us that it is proposed that "the potential at corresponding points of the positive and negative distributing mains shall differ at each point by a constant difference, not being less than thirty volts, and not being more than four hundred volts." And that "such constant difference of potential" is to be termed "the standard pressure." It is to be hoped that the Board of Trade will be much more precise in its limitations. Thirty volts is so low a "pressure" as to be practically out of the question except with a gigantic outlay in copper conductors, whilst four hundred volts is equally inadmissible on account of the danger to person. No less an authority than Sir W. Thomson has said that nothing above two hundred volts should ever be admitted into a dwelling-house. The provision that "the standard pressure may be different for different points of the said mains, and for different hours during the period of supply" is bad, and if permitted will greatly militate against convenience and uniformity in using the current both for light and for motive-power. Where the undertakers distribute "alternating" currents it is provided that the mains should have a "constant"(?) difference of potential" or standard pressure of not less than forty-five and not more than six hundred volts. Here again we think that the Board of Trade might very wisely insist on a further restriction. If steady currents at a pressure of four hundred volts are dangerous, alternating currents at four hundred are far more so. Yet here the undertakers talk of six hundred! Indeed, considering the risks involved, and the difficulty in distributing alternating currents through long lines or lines where there is great self-induction; and also considering that the supply of electric currents is not for lighting alone but for the providing also of motive-power, it would not be any loss to the public if the use of alternating currents under the provisional orders were absolutely disallowed. It is true that the patentees of certain specific forms of machine might cry out loudly against such a prohibition; but the public would be guaranteed against one source of danger and difficulty. According to the orders the undertakers may charge consumers either by the amount of electric energy consumed, or by the quantity of electricity supplied, or by time, or by a yearly agreement. In connection with the first of these methods the proposal is made to call by the name "one unit" the energy contained in a current of 1000 amperes flowing under an electromotive force of one volt during one hour. Most of the provisional orders name sevenpence per unit as the price of electrical energy. We have here for the first time an actual quotation-price for *energy;* a fact which should be interesting to those who have striven so hard to drive into the popular mind exact ideas concerning energy and its conservation. One "unit" thus defined for commercial purposes being 1000 volt-amperes (*i.e.* 1000 watts) for one hour, and one horse-power being 746 watts, we see that the scale of payment is about 5¼ pence per hour per (electrical) horse-power.

Into the further provisions for the inspection and testing of mains, the inspection of meters, the testing of insulation, provisions for safety, and penalties for default in supply, we cannot here enter. Suffice it to say that there is no detail that does not appear to have had thought expended upon it, no provision that is really superfluous or harassing, no possible want or eventuality that does not appear to have been anticipated. Such masterly treatment cannot but greatly facilitate the work of the Board of Trade in agreeing to orders and licenses, and will tend to bring about unity of method in the organisation of the actual work of laying down town supplies so soon as such orders and licenses shall have been granted. If it be true that the effect of the Electric Lighting Act has been to produce a temporary lull in the progress of electric lighting, we are convinced that such a lull will be in the end an unmixed good, since it gives the opportunity for thought to ripen, and for projects and inventions to mature, if not to survive. Two dangers indeed seem yet possible in the future of public electric lighting, and either of them may be sufficiently serious to damage public confidence in this new industrial factor. Firstly, some better guarantees ought to be insisted on that the Companies or other parties who obtain orders or licenses as undertakers should be possessed of capital adequate to carry out the projects in hand. A very hasty glance at the list of applicants for provisional orders will suffice to show that this fear is not unfounded. Secondly, it ought to be made impossible for a Company which has obtained an order for any limited district to delegate the responsibility of supplying any section of such district to a sub-company. No Company should be allowed to hold a monopoly (if the limited monopoly created by the provisions of the Electric Lighting Act be a monopoly at all) of a single square yard of territory which it cannot with its own resources supply under the terms of the order or license which has been granted. If this principle be not upheld, serious abuses will creep in, to the detriment of progress and in contravention of the interests of the public.

CRYPTOGAMIC FLORA OF GERMANY, AUSTRIA, AND SWITZERLAND

Dr. L. Rabenhorst's Kryptogamen-Flora von Deutschland, Oesterreich, und der Schweiz. Zweiter Band: Die Meeresalgen Deutschlands und Oesterreichs. Bearbeitet von Ferdinand Hauck. 1-3 Lieferung. (Leipzig: Eduard Kummer, 1883.)

SINCE the appearance of the original work (1845-53) the systematic study of living algæ has, through a more accurate knowledge of the structure and fructification of these plants, led to great changes in their diagnosis and classification. Hence the necessity of a new edition of Rabenhorst's work.

In order to render it more valuable, the preparation of the parts of which it is composed have been intrusted to authors specially conversant with the subjects of which they treat. The first volume, five numbers of which have already appeared, contains the Fungi, and is edited by Dr. G. Winter of Zurich; the second comprises the Marine Algæ (exclusive of the Diatomaceæ); then will follow the Fresh-water Algæ, edited by Herr Paul Richter of Leipzig; the Diatomaceæ, by Dr. A. Grunow of Vienna; and the Frondose Mosses and Hepaticæ, by Herr G. Limpricht of Breslau. To these will succeed works on the Lichens, Chariceæ, and Vascular Cryptogams.

The second volume, which forms the immediate subject of this notice, has been intrusted to M. F. Hauck, who, from his residence on the coast at Triest, has, during many years, had opportunities of studying marine algæ in a living state; and by his connection with German and other algologists, has been able to obtain authentic examples of most of the species. It may also be mentioned that M. Hauck has published "A List of the Algæ of the Adriatic" (*Beitr. z. Kenntn. d. adriat. Algen*, Wien, 1878).

The present work, of which three numbers have appeared, includes not only the algæ inhabiting the Austrian coast and islands of the Adriatic, but also those of the Baltic and North Seas, and the coasts of Heligoland with the adjacent islands: the latter have been found especially rich in species.

In the Introduction to his work, M. Hauck gives instructions for the collection and preparation of the various kinds of marine algæ. The list of instruments and appliances used in collecting is rather formidable, but it must be remembered that the object of the algologist is to obtain specimens in as perfect a state as possible, for the purpose of instituting a searching examination into the structure and fructification of the plants; and this cannot be done without much labour and pains. In the case of small plants which adhere closely and spread over rocks and other objects, M. Hauck recommends that, instead of scraping off the algæ, portions of the rocks on which they grow should be chipped away with a geological hammer, and preserved with the growing plants upon them. Directions are also given for the treatment of the Corallinæ and other algæ which are covered with carbonate of lime, in order to divest them of the lime, and thus prepare them for microscopic examination. There are also instructions for preparing and mounting specimens of algæ for the microscope.

Every one who has endeavoured to cut sections of algæ for microscopic observation, must be aware of the difficulty, occasioned by the different structures of the plants, of performing this operation. The author shows how some of these difficulties may be avoided; but he has omitted to mention whether the sections should be made with a machine, or in the old-fashioned way, by holding the portion to be cut firmly with the forefinger nail of the left hand, while cutting the section with a sharp, thin knife.

We now come to the work itself. M. Hauck thus classifies the marine algæ: I. RHODOPHYCEÆ, plasma coloured red; II. PHÆOPHYCEÆ, plasma coloured brown; III. CHLOROPHYCEÆ, plasma chlorophyll-green; IV. CYANOPHYCEÆ, plasma bluish-green. Commencing with the Rhodophyceæ, he treats of the Florideæ, describing their structure and fructification. A summary of the families, with the names of the genera contained in each family, follows. M. Hauck's classification of the Florideæ is novel; it remains to be seen whether it will meet with the general approval of algologists. We have next a description of the genera and species. This part of the work is illustrated with figures drawn on zinc, of at least one species of each genus, as seen by transmitted, not reflected, light, the objects being represented as if transparent. Some of these illustrations are original, but the greater part are borrowed from Kützing, Thuret, Zanardini, and others. They are inserted in the text near to the species delineated,—an extremely convenient arrangement.

Besides these illustrations, there are five plates, representing different species of Lithophyllum and Lithothamnion. They were printed by the "Albertotype" process, from negatives executed under the supervision of the author. These plates are admirable, and give more correct and characteristic figures of these singular and in this country but little-known vegetable productions than can be obtained by any other process. Several species of Lithophyllum and Lithothamnion have been found in our seas, and it is probable that more would be found if sought for. They abound in the Adriatic and Mediterranean, and some species are known on the French coast.

M. Hauck seems to have bestowed much pains and care in the preparation of the work, and it will be seen that he has added very considerably to our knowledge of the fructification of numerous species. It may, however, be as well to remind him that the cystocarpic fruit of *Callithamnion Thuyoides*, *Call. polyspermum*, *Call. Borreri*, *Ceramium tenuissimum*, and *Grateloupia filicina*, which he does not mention, were described and figured in Harvey's *Phyc. Brit.* (Pls. 269, 281, 259, 90, 100). Also that the tetraspores of Nemaleon, which M. Hauck says (pp. 14, 59) are unknown, were described by Dr. Agardh, who had examined the living plant (see "Sp. Gen. et Ord. Algarum," vol. ii. p. 417 (1852).

It is to be hoped that we have found in this work the solution of a problem which for a long time has exercised the minds of algologists, namely, Does Porphyra belong to the Chlorosperms or to the Florideæ?

Although the colouring of Porphyra assimilates it to the Florideæ, yet the apparent agreement of its vegetative structure with that of the Ulvas, and especially of some of the species of Monostroma, had induced the elder algologists to place Porphyra among the Chlorophyllaceæ. The discovery of the fructification of the plants of both genera has however shown that they are widely separated. In Monostroma the only kind of fructification known consists of zoospores, which, when they first issue from the mother-cell, are endowed with active motion. In Porphyra the tetraspores were first discovered, then the antheridia; the antherozoïds are motionless. Algologists, however, still hesitated to admit Porphyra among the Florideæ, because no cystocarpic fruit had yet been found. M. Hauck now tells us that the cystocarps of some species are known (p. 21), and he describes those of *P. leucosticta*, as well as the tetraspores and antheridia of this plant (p. 25). There can, therefore, be no longer any hesitation as to including Porphyra among the Florideæ, of which it constitutes the lowest family.

On looking through the present instalment of this work, it will be seen that out of the 122 species, or thereabouts, which are described in it, about seventy are found on the British coasts,—nineteen of the latter are common to the North Sea and Adriatic—twenty-seven of them inhabit the Adriatic, and twenty-four the North Sea. The work, when complete, cannot fail therefore to prove of great interest to algologists in this country.

The type is good, as well as the figures with which it is illustrated, and readers will no doubt be glad to know that in the printing German characters have not been used.

MARY P. MERRIFIELD

THE CHURCHMAN'S ALMANAC

The Churchman's Almanac for Eight Centuries (1201 *to* 2000), *giving the Name and Date of every Sunday.* By W. A. Whitworth. Pp. 23. (London: Wells Gardner, Darton, and Co., 1883.)

THERE never surely was such an age of almanacs. The social change whose effects meet us on every side has worked a revolution here. Some of us can call to mind the time when "Old Moore" ruled the reckoning in his peculiar, old-fashioned way, and Murphy blazed out like a meteor to expire like a farthing candle, and Zadkiel "Tao Sze" began to trade on human curiosity and credulity. But those days are past. Instead of being left, as of old, to make our own quiet, though limited, choice, as the year draws to its close we find ourselves surrounded by a swarm of calendars: the insurance-office, the journalist, the general-storekeeper, the stationer, the watchmaker, the grocer, all vie in pressing on our acceptance something to remind us how time flies; often padded with most irrelevant pieces of innovation, but sometimes, it must be owned, got up in a very attractive form. We do not quite see how all this can be made to pay. We should have thought it a very expensive and often uncalled-for mode of advertising. But that is no affair of ours. Living in "a nation of shopkeepers," whatever may be our private impressions, we are bound to believe that it is found a remunerative mode of expressing gratitude, or anxiety, as the case may be. But whatever may be the donor's purpose it is not quite easy to see what corresponding purpose is, generally speaking, to be answered on the part of the receiver: for, with certain exceptions, it really signifies very little to the bulk of the community, how the fifty-two ensuing weeks are arranged. One great exception of course is the festival of Easter, and the others that depend upon it. But as to these there is always a sufficient general understanding, as there was in our least educated days, when there were comparatively few that knew how to use a calendar: and as to the phases of the moon, the only other leading feature in ordinary almanacs, their notification is rather convenient than necessary, excepting for those who believe, as old-fashioned people still do, with Prince Bismarck at their head, that the moon has an influence distinct from its attractive power. But, say what we will against the necessity of a general diffusion of almanacs, public feeling is on the other side, and even those who could do very well without these favourite articles, and seldom refer to them, would not feel satisfied if they did not possess them.

One curious feature in the case, however, is that so few comparatively have any correct idea of the principles on which almanac-making proceeds. We suspect that even among such as pass for educated people it would be easy to find those who would not be very comfortable if they were required to explain the want of correspondence between the reckoning by weeks and that by months, the unequal length of the latter, the necessity of intercalation, or the cause of the difference between the "styles"—important as that was thought in its day, even to the excitement of popular indignation. As to such matters, if it is true that "we take no note of time but by its loss," it is nearly as true of a large portion of even civilised society, that they take no note of the arrangement of time—except perhaps by misunderstanding it. However, there is no excuse for such ignorance (if we may be forgiven the expression) for the future, if people will take the trouble of referring to the little work whose title we have quoted above. It will not indeed enlighten us much as to the root of all the difficulties—the incommensurable durations of the day and month and year, or help us to make out the strange old machinery of cycles and epacts and golden numbers by which the calendar was kept right, but it will do what is practically of much more value, set before us something of the processes, and all needful results, of the most accurate computations.

The title was a puzzle to us at first, for we had been for so many years acquainted with a very unpretending though most useful *Churchman's Almanac*, that we did not comprehend how it should now find its place in NATURE, till we remarked the continuation of the title; this, promising perennial instead of annual information, at once made a claim to attention which we find is well deserved. There are a good many curious and out-of-the way pieces of information in the three pages of introduction—among which we may mention the explanation of the reason, hitherto to us so incomprehensible, why the accounts of the public revenue are made up to the odd-looking epoch of April 5—and this is followed by a perpetual calendar, as far as Sundays are concerned, to a period that the youngest now living will never see; while, for historical purposes, the retrospective portion is an authentic and valuable resource as to many indefinite matters in chronology, the correct determination of which, as antiquaries well know, often involves considerable trouble. The author has fulfilled his undertaking, as far as we can judge, with especial care and attention; and if his work, which is one of the thinnest of folios, is so far less in accordance with the ideas of this "handy-book"-loving age, we must bear in mind that the form was imposed by the extent of its tabular matter, and that though there is little to attract in its formidable array of figures, its intrinsic value is, for those who need such aid, of a high and enduring character.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Hovering of Birds

MR. AIRY asks for a diagram explaining my views as to the hovering of the kestrel and of other birds, asserting at the same time that these views would establish a "miracle."

If Mr. Airy will be so good as to look at the beautiful drawing of a kestrel in the act of hovering, by Mr. W. Wolf, at p. 160 of the "Reign of Law," he will see an illustration far better than any diagram. Mr. Wolf is an excellent naturalist as well as an accomplished artist, and his drawing of the kestrel was made to represent his own knowledge and observation of the act of hovering, and not to set off any theory of mine.

It will be seen that the body of the bird is represented as at a considerable angle to the horizon, and (of course) to any horizontal current of wind.

It is by placing itself in this position to the wind, and by a wing-action accurately proportioned to the strength of the breeze, that the bird accomplishes the feat of hovering—which is no miracle, but the mechanical result of the "resolution of forces."

The hovering of a boy's kite is a miracle of the same kind. The element of weight is here represented by the string, held at the surface of the ground.

Mr. Airy is, however, mistaken in his description of the facts. He speaks of hovering being performed with "wings motionless, not fluttering." Now I have never seen a kestrel's wings motionless when hovering. Always when the air is still, and always when the breeze is only moderate, the wings have a rapid and tremulous action, varying from moment to moment according as the "muscular sense" directs it, and feels it to be needed for the "poise." But sometimes when the breeze is very stiff this action may be suspended for a moment or two. I have seen this occasionally. But even in this case I could detect the quivering of the quills.

The sea-swallows perform the evolution perpetually over the water when it is as still as a millpond. In all cases the inclined position of the body of the bird to the plane of the horizon is observable. The miracle is always performed by the use of the appropriate means. ARGYLL

Cannes, February 12

I AGREE with "J. R." that the term "hovering" is likely to be misunderstood. I used it because it had been used in the earlier correspondence in NATURE to which I referred. If "J. R." (or any other of your correspondents on this subject) has never seen a hawk hanging in motionless poise above a hill-side, I would ask leave to refer him to NATURE, vol. viii. pages 86 and 324, for a description of the act.

February 19 HUBERT AIRY

I HAD once a very unusual opportunity of observing accurately the flight of buzzards, from the summit of Acro-Corinthus. As this unique natural fortress rises sheer from the plain, on the side toward Attica, to the height of eighteen or nineteen hundred feet, a group of these birds, hanging at that height above the surface, were thus brought in a line with the eye. I could detect the minutest movement of wings or tail. Again and again there were considerable intervals, of many seconds' duration, during which one bird and another would hang, with pinions horizontally outstretched, absolutely motionless, neither descending nor drifting, but as if his balance in the air were one of delicately adjusted equipoise. And when, by a just perceptible movement of wing, he stirred again, it seemed rather to be to change his position than that he needed any kind or degree of effort to maintain it. The kestrel is an unfortunately chosen bird for Mr. Hubert Airy's observation, because though it hangs for a minute or two over the same spot watching its prey, it is always "by short and rapid motion of its wings"; from which fanning motion it has acquired, I think, its popular name of windhover, and not because, as Mr. Airy supposes, it is upborne by the wind. But were my Corinthian buzzards upborne by the wind? There was none. The day was one of dead calm. No doubt of necessity there was some upward current of air from the sun-warmed surface of the ground by which the birds profited; but if at all sufficient to sustain them, their actual gravity, when in that position and so willing it (by which I mean nothing so absurd as that gravitation can be counteracted by the *vis vitæ*, but that by inflating its lungs, and perhaps suspending its respiration, the bird may have the power at will of lessening its comparative weight in the air), must be very near to that of the atmosphere around and underneath them. It is evident that Mr. Airy could only claim my observation as being in favour of his theory if there had been a breeze from Attica striking against the face of the citadel. There was none perceptible; and I drew the attention of my companions to the curious problem presented by such an ease of flight. HENRY CECIL

Bregner, Bournemouth, February 13

P.S.—Will you allow me just to mention that the letter reprinted from NATURE by Dr. George J. Romanes in his "Animal Intelligence," as mine, is by Mr. Merlin, our present Consul in Athens. I sent it, but he wrote it, and the observation is wholly his.

The Auroral Meteoric Phenomena of November 17, 1882

MR. BACKHOUSE remarks in his letter (NATURE, vol. xxvii. p. 315): "It would be well to ascertain whether such a motion (in a curve) would not agree better with the observations of the beam than Dr. Groneman's hypothesis that it was a straight line."

When a straight line lies within or without the (celestial) sphere, on whose surface we wish to trace the perspective projection of that line (the eye being placed in the centre of the sphere), the perspective of the line will of course always be a great circle. When inversely the apparent path of the same meteor, seen from one place of observation is a great circle, the true path must lie in a plane. When the apparent paths, seen at the same time from two different places, not situated in the direction of the apparent path, are both great circles, the true path lies in two different planes, and *must be a straight line*. Now Prof. Oudemans at Utrecht says positively that the apparent path of the phenomena of November 17 was a great circle, cutting the horizon (and also the equator) in two opposite points. Of the English observers I will cite Mr. Saxby (p. 86), who describes "the trajectory as much flatter than that of the stars." Moreover the general fact is, as I proved in my paper, that this trajectory, having been seen of regular form and consequently probably of equal curvature in its whole length, intersected the great circle of the horizon in two opposite points, and therefore must have been a great circle itself. The above-mentioned condition being fulfilled, I was under the necessity of taking the true path as a right one. I think this peculiarity indicates the meteoric nature of the phenomenon and of all the auroral arcs (*les arcs proprement dites* of my theory) showing as great circles of the sphere. In fact a curve cited by Mr. Backhouse, lying at equal height above a terrestrial parallel, will show itself *but in one case* as a great circle, namely where the observer is within its plane. From all other places it will be seen as a small circle of the sphere. In this case is the apparent boundary of an aurora in the north, the arch of the dark segment cutting the horizon in two not opposite points.

I dare not occupy more space to answer Mr. Backhouse further on the influence exercised by cosmic matter on terrestrial magnetism, and the consequence of the general direction east to west of these currents when passing in the neighbourhood of the earth, but I think that this direction east to west *must* be deduced from the observed facts.

I am much obliged to Messrs. Petrie and Muirhead for their information. As to the remark of the former on the spectrum observed by Dr. Rand Capron, I think that the auroral character of some phenomena will be proved the best when it shows the auroral lines, whatever may be the origin of its light. When its other properties point out its meteoric character, a strong argument is found in favour of the cosmic theory of auroræ. H. J. H. GRONEMAN

Groningen, Netherlands, February 14

The Orbit of the Great Comet of 1882

I AM very much obliged to those gentlemen who have kindly given me the information required in my letter published in NATURE, vol. xxvii. p. 314.

They all agree on the same point, which confirms my opinion that in all the good observations the same or very nearly the same point of the head was observed during the brightest appearance of the comet.

I remarked especially in the sketches shown to me by Mr. A. A. Common, who was the first to see the comet in England, on September 16, and who continually made careful observations of it, that, although the nucleus was seen since October 30 divided into two parts, always one of these (which I shall call the main part next to the following end of the nucleus) remained the brightest. Mr. Common in every drawing marks this part with the word "brightest." At the Washington Observatory also this same bright point was always observed with the transit instrument, as it is stated by Mr. W. C. Winlock in his letter (NATURE, vol. xxvii. p. 129).

Mr. W. L. Elkin, Cape Observatory, in a communication to the *Astronomische Nachrichten*, No. 2490, speaks about this orbit. He used the first observation made at the Cape on September 8, the observation [of the] disappearance of the comet at the sun's limb on the 17th of the same month, and a normal observation on November 17, to calculate either a parabolic orbit or an elliptic one; but none of these gave the positions of the comet according to intermediate observations.

Mr. Elkin believes it is possible to take as the most probable value of $\frac{1}{a}$ the value 0·0075, and consequently the comet has a very long period, while Mr. Morrison in his calculation of the orbit had $e = 0{\cdot}9998968$, and a period of 652·5 years.

As errors of observations are of course inadmissible, it is now the question to study what produces such great differences in calculating the orbits.

Are they due to disturbances during the comet's passage through the solar system, and especially at its passage through the sun's corona? or are they due to the hypothesis specified by Mr. Elkin and others that the centre of the nucleus is not the point gravitating around the sun? This question cannot be decided but by a careful discussion of all the positions of the comet during the whole period.

The observations before perihelion are of course very important. Unfortunately at the Cape the astronomers were prevented making observations between September 8 and September 17 because of bad weather; but there are some observations made in Melbourne and in other observatories before September

17; and besides, the important observation of the disappearance of the comet at the sun's limb is very valuable. Now then, if it will be possible to secure some observations in the remaining days the comet will be visible, I am sure we shall have a large amount of material to study upon.

I may add that Mr. Common and I saw the comet a few days ago. With magnifying power of 120 and 150 we were not able to distinguish the division of the nucleus, but with a higher power we saw five bright points; one of these, corresponding to that seen before, remains the brightest. The comet has all the appearance of a little curve convex to the horizon, and is still a very bright object, as Mr. Common was able to see it pretty well with only six inches aperture and in moonlight.

13, Pembridge Crescent, Bayswater, W. E. RISTORI

Aino Ethnology

LET me hasten to assure Herr Rein that nothing could have been further from my intention than to question the "love of truth," which is conspicuous in his work on Japan. I trust he will consider as absolutely withdrawn any expression of mine which he fancies might at all bear such a construction. His authorities I did not quote, because I attached much more importance to the weight of his name than to theirs. The almost unanimous opinion of original observers is opposed to their conclusions, which I was certainly somewhat surprised to find adopted by Herr Rein. But as he has not himself visited the Aino people, the question of their affinities need not be further argued here. I may state, however, that to Steube and von Siebold must now be added Herr Kreitner, of the Szechenyi expedition, who emphatically removes them from the Mongolic, and "assimilates them to the Caucasic type" ("Im Fernen Osten," Vienna, 1881, p. 318). A. H. KEANE

Auroral Experiments in Finland

IN the note in NATURE, vol. xxvii. p. 322, in which you refer to my telegrams from Sodankyla, there is a misunderstanding concerning the apparatus which I made use of in the experiments. This apparatus, which I call in Swedish "Utfrömningo-apparat" (streaming apparatus), was constructed of uncovered copper wire, provided at each half-metre with fine erected points. That wire was led in slings to the top of the hill, and reposed on the usual telegraph insulators. From one end of this wire was conducted a covered copper wire on insulators to the foot of the hill (600 feet high), and there joined a plate of zinc interred in the earth. In this circuit was put a galvanometer.

It was this apparatus which produced both the yellow-white halo at Oratunturi and the straight beam of aurora borealis at Pietarintunturi, as the positive current in the galvanometer at both places. The terrestrial current diminishes (or ceases) below the belt of maxima of the aurora borealis. S. LEMSTRÖM

Helsingfors

Flamingoes and Cariamas

IN NATURE, vol. xxvii. p. 334, an account is given of the curious behaviour of a flamingo towards a cariama. May I point out that this habit of the flamingo was observed in 1869 by Mr. Bartlett, and will be found in a P.S. to a paper of his entitled "Remarks upon the Habits of the Hornbills," read before the Zoological Society, February 25, 1869. The liquid was examined by Dr. Murie, and is said to have consisted almost entirely of blood. A short notice of the habit, communicated by Mr. Bartlett, appears also in Buckland's Edition of "White's Selborne." JAMES CURRIE

Cambridge, February 19

THE APPROACHING FISHERY EXHIBITION

FROM the cheerful note of preparation which is now being sounded, we presume the opening of the International Fisheries Exhibition will take place punctually on the day which has been fixed for that event—May 1. That the Exhibition will be successful, both in a pecuniary sense and as an exposition of fishery economy and of the natural history of our food fishes, may, we think, be even now predicted. The two exhibitions by which it has been preceded, those of Edinburgh and Norwich, not only paid all expenses, but left a handsome surplus; so that, with the vast population of London and the strangers who daily come within its gates to work upon, the promoters of the exposition are warranted in believing that it will prove a success. It will undoubtedly be the greatest affair of the sort which has yet been designed, and will occupy a site twice as large as the Norwich and Edinburgh exhibitions joined together. The fishery exhibition which was held at Berlin three years ago was visited by nearly half a million persons, but it was only open for ten weeks, whilst the show to be held at South Kensington will remain open for six months, and as the population of London is more than four times greater than that of Berlin, we may calculate on the visitors to the Fishery Exhibition running into big figures;—two million persons at a shilling each would represent a sum of one hundred thousand pounds. Already a large guarantee fund has been subscribed by corporations and private persons, and there is no reason why Parliament should not be asked for a grant in aid, although any money that might be granted may not be required. It is right to say that as a nation we play a rather "mean" part in such matters, and are quite outdone in liberality by other countries. America, for instance, is sending us an "exhibit" which will cost that country ten thousand pounds, and other foreign countries are acting in an equally liberal spirit. If we were asked on any occasion to reciprocate, what answer could we make? We have positively nothing that we could send. With the exception of the toy museum left to the country by Mr. Frank Buckland, we possess nothing in the shape of a national collection illustrative of fishery economy; hence the Exhibition which is about to open assumes very much the shape of a commercial enterprise, and becomes a gate-money show. But that is better than nothing, and it is to be hoped that from the debris of the approaching exposition a substantial addition may be made to the Buckland Museum of economic fish culture, and if we may be permitted to make such a suggestion, the aquarium should, if that is possible, be so arranged that it could be left as a permanent attraction for all who are interested in the natural history of fish and in the proper ingathering of the harvest of the sea.

Great expectations are entertained as to the value of the lessons to be taught at the approaching Exhibition. We are undoubtedly in need of knowledge of all kinds regarding the natural history of our fishes. From the whitebait to the whale we are singularly deficient in those details of fish life that would prove valuable to persons engaged in fishery enterprise. In the matter of well-planned investigation into the natural history of the British food fishes we are far behind America, where information of the most valuable kind is systematically collected and disseminated. As a matter of fact, we have (as a nation) done almost nothing in respect of adding to the knowledge of the public. Some individuals have been toying with the subject of *Pisciculture*, whilst in the seas that pertain to the United States fish-breeding on an extended scale has been long in operation under the auspices of the Government. It will not be the fault of the promoters of the approaching Exhibition if attention is not aroused to our want of interest (as a people) in the sea-fisheries of the country. We have therefore every reason to be grateful to those who have stepped to the front in order to promote this enterprise; the men who have assumed the lead have nothing to gain personally by its success—they are working in the interests of the public, knowing well that the fisheries of the surrounding coasts contribute largely to the commissariat of the country.

A portly prospectus, so far as its contents are concerned, has been issued, indicative of what will be shown in the Exposition, and from that document we gather that a large sum of money will be distributed in prizes for inventions and improvements of fishing gear; the special prizes in this department alone will number over 100, ranging in value from 600*l.* to 2*l.* 10*s.* Over 1000*l.* will also be given for essays on various topics connected with the economy of the fisheries and the natural history of our more important food fishes, as also for papers on fishery legislation. The dissemination of the knowledge to be obtained from such essays as may be awarded prizes is important. None of the essays contributed to the Norwich Exhibition have been published, except that of Sir James Maitland, printed presumably at his own expense, so that whatever information was contained in the Norwich prize essays remains only in the cognisance of those who read them. The Edinburgh prize essays are, we believe, being printed. Surely they might have been published ere this, and it might be taken into consideration by the executive of the present Exhibition, whether it is possible to have the essays judged, the prizes awarded, and a print of such as are worthy of being published on sale in the building in the course of the summer: a popular "handbook" to the Exposition will, we may presume, be issued. As to "exhibits" of a useful kind, such as those of fishing gear of every description, men with a practical turn of mind will be able to take stock of them and perceive at a glance how far they can be utilised. As a class, fishermen are slow to learn and chary in the way of trying experiments, but it is not impossible that the approaching Exhibition may contain the germs of some new ideas which may prove alike practical and profitable.

THE PROGRESS OF TELEGRAPHY

THE first of the series of six lectures on the Applications of Electricity was delivered on Thursday evening, February 15, at the Institution of Civil Engineers, on "The Progress of Telegraphy," by Mr. W. H. Preece, F.R.S., M.Inst.C.E., of which the following is an abstract:—

Telegraphy is the oldest practical application of electricity. It grew about the railway system, and was rendered a practical agent by the foresight of Robert Stephenson, I. K. Brunel, Joseph Locke, and G. P. Bidder, who were its godfathers in England. Electric currents are, as a rule, maintained for telegraphic purposes by the combustion of zinc, and in the innumerable forms of batteries in use, the conversion of zinc into sulphate of zinc is the root of the transformation of energy into that form which was utilised as electric currents. There are three forms of battery in use in the British Post-Office Telegraph system, and in the following numbers:—

Daniell	87,221 cells.
Leclanché	56,420 ,,
Bichromate	21,846 ,,

Every administration has its own adopted form, differing in design, but based on one or other of these types. Magneto-electricity is employed for some forms of apparatus, and dynamo-machines are sometimes used to supplement batteries. Experiments are now being made with secondary batteries. The various terms employed—electromotive force, resistance, induction, and current—though measurable in definite units, have not yet become household words; but, being admitted into commercial, legal, and Parliamentary lore, they will soon be as familiar as feet, gallons, or pounds.

Electric currents are conveyed from place to place either overground, underground, or submarine.

Overground.—Wooden poles preserved in creosote are employed in England, but iron poles are extensively used in the colonies. The conducting wire is almost universally of iron, but copper wire is much used through smoky places where iron is liable to rapid decay. Phosphor-bronze wire is under trial, and is a very promising material, as it possesses the conductivity of copper with the strength of iron. The improvements made in the quality of iron wire have been very great, and it conducts now fully 50 per cent. better than it did a few years ago. Electric tests have had a marvellous effect upon the production of pure metallic conductors; copper has improved in even greater ratio than iron; samples have been produced better even than the standard of purity. The insulators remain principally of porcelain, and their forms vary nearly with the number of individuals who use them; the only improvement of any value recently made is one which facilitates the very necessary process of cleaning.

Underground.—Wires are almost invariably carried underground through towns. Copper wire, insulated with gutta-percha, incased in iron pipes, is the material used. There are 12,000 miles of underground wire in the United Kingdom. There is a great outcry for more underground work in England, owing to the destruction to open lines by gales and snow storms; but underground telegraphs, wire for wire, cost at present about four times as much as overground lines, and their capacity for the conveyance of messages is only one-fourth; so that overground are, commercially, sixteen times better than underground wires. To lay the whole of the Post-Office system underground would mean an expenditure of about 20,000,000*l.* Hence there is no desire to put wires underground except in towns. Besides snowstorms are few and far between, and their effects are much exaggerated. Of the numerous materials and compounds that have been used for insulating purposes, gutta-percha remains the oldest and the best for underground purposes. It, like all other materials used for telegraphy, has been improved vastly through the searching power that the current gives the engineer.

Submarine.—The past ten years has seen the globe covered with a network of cables. Submarine telegraphs have become a solid property. They are laid with facility and recovered with certainty, even in the deepest oceans. Thanks to such expeditions as that of H.M.S. *Challenger*, the floor of the ocean is becoming more familiar than the surface of many continents. There are at present 80,000 miles of cable at work, and 30,000,000*l.* have been embarked in their establishment. A fleet of twenty-nine ships is employed in laying, watching, and repairing the cables. The Atlantic is spanned by nine cables in working order. The type of cable used has been but very little varied from that first made and laid between Dover and Calais; but the character of the materials, the quality of the copper and the gutta-percha, the breaking strain of the homogeneous iron wire, which has reached 90 tons to the square inch, and the machinery for laying, have received such great advances, that the last cable laid across the Atlantic, by the Telegraph Construction and Maintenance Company, was done in twelve days without a hitch or stoppage.

Ideas are conveyed to the mind by electric signals, and in telegraphy these signals are produced at distant places by using two simple electrical effects: (1) that a magnetic needle tends to place itself at right angles to a wire when an electric current passes through it; and (2) that a piece of iron becomes a magnet when a current of electricity circulates around it. An innumerable quantity of tunes can be played on these two strings. Various companies were established at different times to work certain systems, but when the telegraphs were absorbed by the State the fittest were selected to survive, and their number consequently declined.

The A B C instrument is the simplest to read, for it indicates the letters of the alphabet by causing a pointer to dwell opposite the desired letter. There are 4398 in use. Its mechanism is, however, complicated and expensive, and it is being rapidly supplanted by the telephone. The needle instrument is the simplest in construction, but it requires training to work it. There are 3791 in use in the Post Office, and 15,700 among different railway companies. As a railway instrument it is the simplest, cheapest, and most efficient ever devised. The Morse instrument, of which there are 1330 in use in the Post Office and 40,000 on the Continent, records its letters in ink in dots and dashes on paper tape, and, like the needle and A B C, appeals to the consciousness through the eye; it also indicates the letters of the alphabet by sound, and thus utilises the organ of hearing. Sound-reading is gaining ground in England with great rapidity. There are now 2000 sounders in use: in 1869 there were none. In America scarcely any other instrument is used. On the Continent there is scarcely one.

Acoustic reading attains great perfection in Bright's bell instrument, where beats of different sound replace the dot and dash of the Morse alphabet. Sound-reading is more rapid and more accurate than any system of visual signals or permanent record. In fact no record is kept in England, for the paper tape is now destroyed as soon as it has been read. Errors are of course inherent in all systems of telegraphy. A telegraphist cannot see what he writes, or hear what he says, and who is there that does not make mistakes whose eye follows his pen, or whose ear takes in his own words? The Hughes type-instrument, which prints messages in bold Roman characters, is much used on the Continent; it is, in fact, recognised as the international instrument, but it has had to give way in England to a more rapid system of telegraphy. It is, however, solely used for the Continental circuits by the Submarine Telegraph Company. All long cables are worked by Sir William Thomson's beautiful siphon-recorder.

In ordinary working only one message can be sent in one direction at one time; but by a simple and ingenious contrivance, by which the neutrality of opposite currents is utilised to convey signals, duplex telegraphy is rendered possible, so that two messages can be sent on the same wire at the same time; and by a still further improvement, where currents of different strength are utilised, four messages are sent on one wire—two simultaneously in opposite directions—at the same time. There are in England 319 duplex and 13 quadruplex circuits at work.

The acme of efficiency in telegraphy is attained in the automatic system, in which manual labour is supplanted by mechanism in transmitting the messages. There are 71 circuits worked by these instruments, and 224 instruments in use, and a speed of working of 200 words per minute is easily maintained upon them. When the hand alone is used, from 30 to 40 words per minute is the maximum rate attained, but by automatic means the limit is scarcely known. Since this system can be duplexed, and in many cases is so, 400 words per minute on one wire are easily sent. By the use of high-speed repeaters, the length of circuit for automatic working is scarcely limited; it would be easy to send 100 words per minute to India.

The growth of business since the telegraphs have been acquired by the State is enormous: 126,000 messages per week have grown to an average of 603,000; but the mileage of wire has not increased in anything like the same proportion, the excess of traffic having been provided for by the great improvements made in the working capacity of the apparatus. In 1873, the average number of messages per mile of wire was 147, it is now 256. It is in press work that the greatest increase has taken place: 5000 words per day at the time of the Companies have grown to 934,154 words per day now. 340,966,344 words of press matter were delivered in the year ending March 31, 1882.

The development of railways has necessitated a corresponding increase in the telegraphs required to insure the safety of the travelling public, and while 27,000 miles of wire in England, Scotland, and Wales were used for that purpose in 1869, at the end of December, 1882, the total had increased to 69,000 miles, equipped with 43,176 instruments, against 8678 in 1869.

The growth of business is equally discernible in the great cable companies. In 1871 the number of messages dealt with by the Eastern Telegraph Company was 186,000; in 1881, it was 720,000. This growth is equally striking in all civilised countries, and even in Japan 2,223,214 messages were despatched last year, of which 98 per cent. were in the native tongue. The mode of transacting the trade of the world has been revolutionised, and while wars have been rendered less possible, their conduct has been expedited, and their penalties alleviated.

CENTRAL AND WEST AFRICA[1]

THE brilliant journey of Major Serpa Pinto across Africa from Loanda, by the Zambesi to Natal, must be fresh in the recollection of our readers. The present narrative may be regarded as complementary of the major's exciting story. Captains Capello and Ivens were members of the original expedition along with Major Pinto, and for the first part of the journey the three companions worked together. The object of the expedition, which was organised by the Portuguese Government, was to thoroughly survey the great artery which—a tributary of the Congo—runs from south to north between 17° and 19° E. of Greenwich, and is known as the Cuango, as also to determine all the geographical bearings between that river and the west coast, and make a comparative survey of the hydrographical basins of the Congo and Zambesi. The three travellers started from Benguella in November, 1877, but had not proceeded far on their journey, when a difference of opinion arose as to the future route of the expedition. Messrs. Capello and Ivens did not feel at liberty to depart from the original letter of their instructions, while the bold Major Pinto conceived that he would be carrying out the spirit of their instructions by making a dash across the continent. We have nothing to do with the quarrels of the travellers; experience proves that in such an expedition there should be one supreme head, and that the best exploring work has often been done by a white traveller single-handed. Major Pinto's presence with the other two was really unnecessary, and it was certainly to the advancement of geographical knowledge that he took an entirely different route. Messrs. Capello and Ivens are evidently two pleasant and agreeable gentlemen, though we have some doubts if exploration is exactly the *métier* to which they are best adapted. At all events they have written a narrative that contains much pleasant reading, and some additions to our knowledge of the geography and natural history of the limited region which they traversed. Their real work lasted for about two years, during which they traced the Cuango northwards to about 5° S. lat., when they were compelled to turn back, partly owing to the exhaustion of their supplies, and partly to the arid nature of the country beyond their farthest point. During their journey they crossed innumerable streams, some of them adding their waters to the Cuango and others joining the Cuanza, which discharges into the Atlantic south of Loanda. The sources of the Cunene, Cuanza, and Cuango were visited and determined, and a pretty careful survey of the region all along the route made. The country traversed is mostly mountainous, cut up by innumerable streams and valleys, rich in many parts in vegetation, and even in metals, and having a considerable population clustered in villages, each of which is ruled by its chief. With each of these chiefs much diplomacy had to be used in order that the explorers and their followers might obtain provisions and be allowed to pass; but the repetition of the same story of petty troubles and difficulties becomes ere long somewhat tiresome. The habits and dwellings, the implements and weapons, the dispositions and superstitions of the people in this region are pretty much the same as those of the other Bantu tribes with which Pinto, Stanley, Cameron, and other recent explorers have made us familiar. Among the Ganguella we find considerable manufactures of iron, while Bihé is rich and fertile, and its inhabitant the greatest native travellers in Africa. In reference to the Bihenos the authors have some curious remarks on the well-known African prefix in its varying forms *ma*, *ba*, &c. They seriously lament the ignorance of ethnologists who call the Kafirs "Bantu," a word, they tell us, which simply means "persons." This is in strict analogy with the customs of nearly all peoples, who almost invariably refer to themselves by terms which mean *the people*, *the men*, &c. Bantu has come to have a well-defined ethnological significance, and is not likely to be displaced by the not too well-informed criticisms of our travellers.

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[1] "From Benguella to the Territory of Yacca; description of a journey into Central and West Africa." By H. Capello and R. Ivens. Translated by Alfred Elwes, Ph.D. Two vols. (London: Sampson Low and Co., 1882)

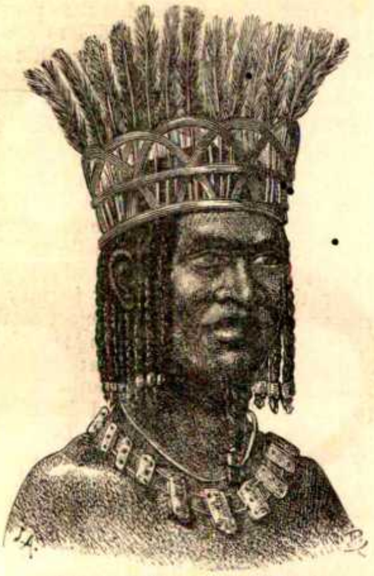

Fig. 1.—A Muata of the T'chibcco.

Among the people of this region we find the same elaborate methods of dressing the hair, so common in Central and Western Africa, and with which readers of recent African travel must be familiar. We have some interesting details as to the history of some of the leading tribes of the region, from which it is evident that for centuries the various African peoples have been in a state of almost constant migration, that the so-called states are exceedingly unstable, and that even here it would be hazardous to regard any one race as unmixed.

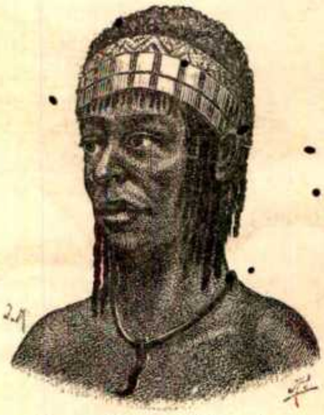

Fig. 2.—Woman of Cangombe

We give here two types: Fig. 1, a Muata, or ruler, of the T'chiboco; and Fig. 2, a woman of Cangombe.

The sources of the Cuango were found at a height of 4756 feet, at about 11½° S., and a little east of 19° E., in one of the most extraordinary watersheds to be met with anywhere. It is thus described:—

"An extensive tract of land, all hill and dale, marks this culminating point, a sort of St. Gothard of the African waters. On the north, running through a narrow and tortuous valley, appeared the Cuango, which, shortly after its birth, flows at the foot of the plantations of manioc and massambala, growing abundantly upon the slopes, and at that time filled with girls and women engaged in hoeing and other field labours. A bluish streak of land was visible in a south-west direction, and on the western slope, in *Canica*, appeared the sources of the Caüeu rivulet, which constitutes the modest commencement of the great Cassai. To the north-east stretched out the T'chibungo range, on whose eastern slope were visible the sources of the T'chipaca at about twenty-five miles from the point of observation, and whose latitude was 11° 27′ and longitude 19° 11′ 30″. Finally, the eye took in at various distances, approximately determined by the compass, an infinity of spring-heads, the sources of various affluents of the T'chipaca, the Cuango, the Cassai, the Lume, and the Loando, which, glittering in all directions, poured their ever-increasing waters to the Congo-Zaire, the Cuanza and the Zambese, till they were lost to sight in the valleys and ravines, where a denser vegetation still hinted at their sinuous course. The aspect of the country was magnificent. In the east, extended as far as the eye could reach, the rich green valley of the upper Cassai, clothed with numerous senzalas of *ma-quioco* and *ma-cosa*, indicated by the white patches of manioc flour spread to dry upon the *luandos* or mats of the *mabu*."

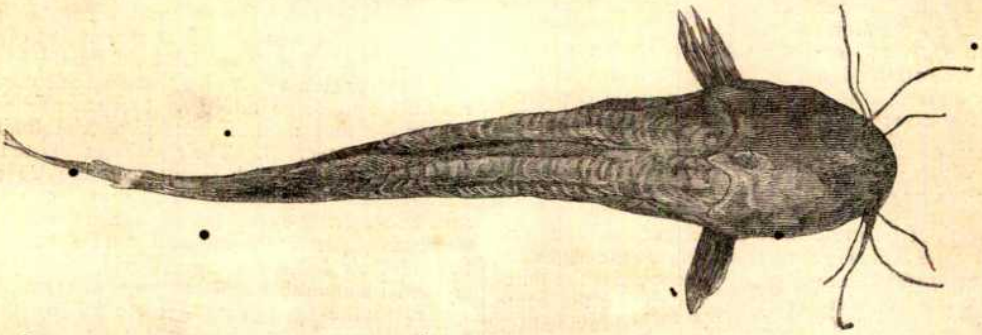

Fig. 3.—Ébande (Fish of the Cuango).

Speaking again of the same remarkable region, the writers say:—

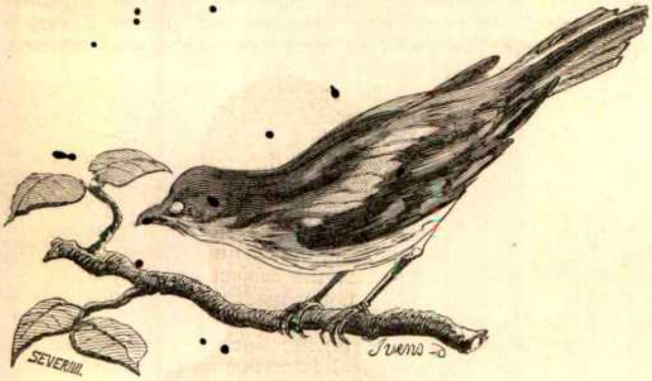

Fig. 4.—Fiscus Capelli (Cassange).

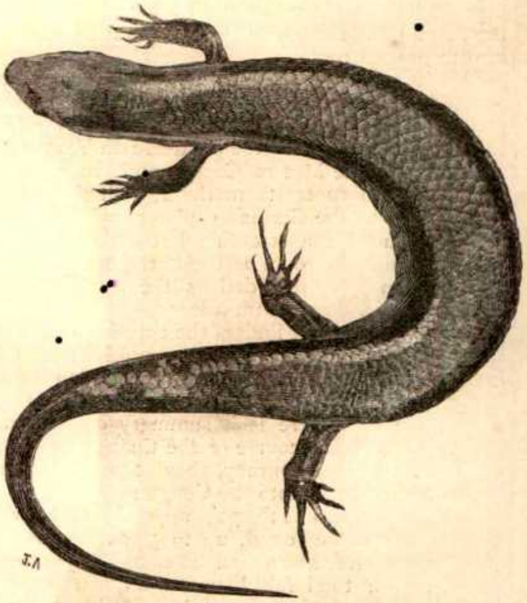

Fig. 5.—Euprepes Ivensi (new species), River Cuanza.

"In a lofty position—the mean altitude being 1531 feet—the intense heat of the tropics is far from predominant, and the breeze which is stirring during a part of the year renders the climate soft and salubrious to the European. Standing upon a granitic plateau, the region may properly be described as the *Mother of the African Waters*, a veritable hydrographic centre whence issue, through deep gullies, the streams that flow to the two great oceans by the channels of the Congo-Zaire, the Cuanza, and the Zambese. Its mineral wealth is considerable, abounding chiefly in oligist iron; native copper exists more to the eastward, where, if we may rely upon the reports of the natives, the lodes are easily worked. The vegetable products, more especially upon the banks of the great rivers, are important. There are *Apocinaceas*, or india-rubber trees; *Burseraceas*, which yield aromatic resins such as the *Eleni*; *Herminieras*, used in the building of canoes; *Rubiacias*, or teak, mixed with *Erythrinas*, producing cork; several *Euphorbias*, acacias used for dyeing purposes; *Typhas*, and a species of *Borassus*; grasses of various kinds, such as the *panicum* and *andropogon*, the *pennisetum*, both smooth and barbed (massango), hemp, and a large number of *Convolvulaceas*; all these we ourselves saw. Among the variety of wild fruits of T'chiboco are distinguishable the *fungo*, not unlike a plum, but less pulpy and more sour, which grows upon a medium-sized tree; the *macolla*, of the granular species,

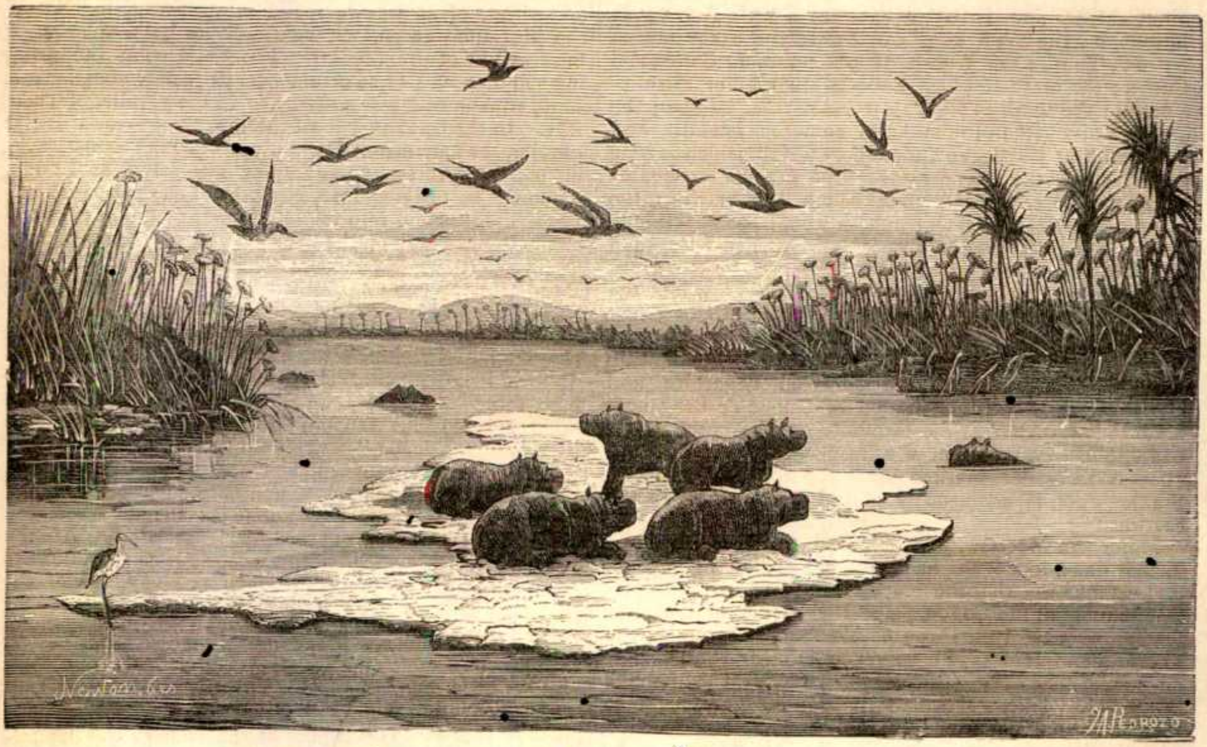

Fig. 6.—The Cuango in Yacca.

having the shape and size of an orange, but resembling internally the American muruenia, that produces purgative effects when taken in large doses; the *tongo*, similar in form and dimensions to the white plum; and the *tundo*, almost equal to a cherry in taste, and having black seeds. The abundance of wax is really remarkable, and towards the south and south-east it constitutes an important branch of industry."

At this point the two travellers separated in order to proceed northwards on different sides of the Cuango, and met again at Cassange, where they fell in with Dr. Max Buchner, on his way to the great Muata Yanvo. Of this famous potentate Messrs. Capello and Ivens give a fancy portrait, which contrasts markedly with that taken from the original by the German explorers who have recently done so much for a scientific knowledge of the region through which the route of the Portuguese travellers lay. Cassange may be regarded as the furthest Portuguese outpost, and a busy centre it is.

Yacca, the furthest limit of the expedition, was reached in May, 1879, and although innumerable small lakes and many streams had been passed, the region beyond was found to be an arid desert, brooded over by "the silence of the grave." Here is a summary of the travellers' observations on the course of the Cuango from its source to the limit of their journey, about 140 miles from where the river discharges into the Congo:—

"From parallel 11° 30′, approximately, where its sources are to be found, up to 5° 05′ at the Quicunji cascade, the river has a sinuous course of 580 geographic miles, and a total fall between its extreme points of about 3 feet 4 inches per mile. Rocks, stones, rapids, and cataracts interrupt the stream, and twelve of the points at which they do so are known to us, namely, the first at parallel 10° 17′, to the east of Muene-songo; the second at 10° 25′, near the Camba rivulet; the third at 10° 08′, Caxita rocks; the fourth at 10° 05′, the Louisa falls; the fifth at 10° 05′, a cataract a little above Port Muhungo; the sixth at 9° 20′, Zamba; the seventh at 19° 19′, Tuaza; the eighth at 9°, cataract Cunga-ria-Cunga; the ninth at 7° 42′, Suco-ia-Muquita or Suco-ia-n'bundi; the tenth at 7° 38′, just below the Camba; the eleventh at 7° 35′, in the midst of numerous islands; and the twelfth at 5° 05′, the Quicunji waterfall, which is only passable after the heavy rains. The greatest navigable tract, therefore, is that space which lies between the cataract at 7° 35′ and Quicunji, or about 190 geographic miles. The river there is of variable width, never less than 76½ yards, and from 5 to 20 feet in depth. The current loses a little of its speed in the upper section, where the stream in the summer season has a fall of about 3 feet 2 inches per mile. We think it well to mention that our longitudes being strictly correct, as the record, partly chronometric, was compared both on departure and arrival at the Portuguese station of Duque de Bragança, and the latter again at the terminus on the coast, it appears to us that the point of affluence of the Cuango (or Ibari-N'Kutu) as marked upon the maps, just above Stanley Pool, is erroneously placed considerably to the eastward."

Major von Mechow, who has been exploring the river further down its course, has found it equally unnavigable, and we may say that the maps illustrating Mr. Stanley's last journey to the Congo place the mouth of the river further west than on those of his famous trans-African expedition. It was this river which Mr. Stanley ascended in his little steamer, and found it expanding into a broad lake. Messrs. Capello and Ivens came to the conclusion, confirmed by Major von Mechow, that no such lake as Aquilonda exists in this quarter. The travellers returned by a somewhat different route, staying for some time at Pungo N'Dongo, with its famous rocks, and reaching Loanda in October, 1879.

The work abounds with illustrations of the country and the people, many of them devoted to natural history. On the animal and plant life of the district traversed there are many valuable notes, and in the appendix will be found, besides tables of geographical observations and heights above sea-level, lists of additions to the fauna and flora, tables of African dialects, and a N'Bunda Vocabulary. There is a good summary of the general results in the concluding chapter, in which the authors have the following observations on the geology of the continent:—

"The physical configuration of the African continent, and more especially of the portion south of the equator, is nowadays too well known to require minute description. It may be summed up in these few words: a depressed central basin surrounded by a vast circle of high land, gradually descending to the sea, and rent by deep ravines, through which rush huge watercourses, engendered in the interior, till they overflow and seek the lower level fronting the ocean. From a very general geological point of view we may define the regions running from the littoral to the interior in the following order, viz. limestone, sandstone, and granite. But on going more minutely into the subject we shall find that these distinctions are not very exact; inasmuch as the component parts frequently run into each other and change places, while precise lines of demarcation are wanting. The geological formation on the western coast at the points observed by us between Loanda and Mossamedes, and even further to the north, exhibits generally near the sea a belt of tertiary deposits, with abundant masses of sulphate of lime and sandstone, from which they are separated by beds of white chalk alternating with primary rocks, for the most part gneiss, abounding in quartz, mica, hornblende, granite, and granulated porphyry. Towards the south large tracts of feldspar become visible. At Mossamedes whole mountains are composed of sulphate of lime; while carbonate of lime, accumulated in shells, is very frequent. Both rock-salt and nitrate of potash are found in stratification. Along the Mocambe chain, we were informed, there exists a basaltic line of great length. From that point the shifting soil may be said to commence, extremely abundant in sand, constituting true *saharas*, as in the parallel of Tiger Bay. In the transition from the lower zone towards the interior, for instance at Dondo, vast tracts of schist rock, in perfect laminæ, compose the soil; and sandstone, reddened by oxide of iron, is visible in every direction. Proceeding further into the interior we find, in a perfectly mountainous region, the ground to be composed of granite-quartzy rock, extremely hard and compact; this is the case throughout the belt crossed on the way and up to Pungo N'Dongo, the surface soil being formed by the disintegration of the granite itself. These geological characteristics will naturally be repeated to the south and north in identical parallel regions, with variations in the high table-land, where we meet occasionally with hard and tough red sandstone and rocks of feldspar as in the basin of the Lucalla."

In the same chapter will be found abundant notes on the various tribes visited, which, although the authors' ethnology appears to us by no means sound, are still a valuable contribution to a knowledge of the African peoples. As evidence of the important contributions to the natural history of West Africa, we give a few of the illustrations bearing on the subject.

ON THE AURORA BOREALIS[1]

HAVING been requested by this journal to give an account of my latest researches into the nature of the aurora borealis, I must explain that my lateness in complying with this request arises from the fact that I had this winter changed my residence from Bergen, where the communication was directed, to this spot—Kautokeino, in Ultima Thule.

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[1] In reference to the present interesting communication from Herr Sophus Tromholt, from his station in Ultima Thule, we ought to point out that Herr Tromholt was, at the time of writing, not aware of the important discovery as regards the nature of the aurora made by Prof. Lemström at the Finnish station of Sodankylä during December last, and of which an account appeared in NATURE, vol. xxvii. p. 322.

Since September last I have, for the sake of the aurora borealis, been residing here in North Finmarken (69° N. lat., 23° E. long), in a quarter, therefore, where the auroræ attain their maxima, and where the phenomena, consequently, are so frequent and on such a scale that there cannot be a question of selecting and analysing one in particular. I therefore prefer to give briefly a description of its general appearance here, its character and occurrence.

My winter sojourn here has two objects in view—viz. firstly, to frame a pendant to the observations of the auroræ made at Bossekop, 1838-39, by the French Commission du Nord ("Voyages en Scandinavie," &c.), which, by the bye, later students of the phenomenon seem to have entirely ignored; and secondly, by means of altitudinal measurements corresponding with those now being made at the Norwegian Meteorological Station at Bossekop, to procure sufficient materials for fixing the parallax of the aurora borealis. I choose the remote Kautokeino for my observatory for several reasons—viz. that this place is situated almost exactly south of Bossekop, while the distance between the two places is very nearly a degree, a distance which is exactly suited to the opinion I have formed as to the height of the aurora, viz. 150 kilometres, and also for the reason that Kautokeino possesses a very free horizon, and that its situation, very far inland, would insure favourable weather conditions.

As previously stated, observations are made simultaneously here and at Bossekop on a common pre-arranged plan, and measurements made in the common vertical plane by the so-called auroral theodolite, constructed by Prof. Mohn. A similar arrangement has also been effected with the Finnish Meteorological Station at Sodankyla, which is, however, situated at a great distance from this place and in a somewhat unfavourable direction (about 45° S.E.). We shall not, of course, be able to compare notes before the spring, so I am unable at present to lay before the reader the final results; but judging from my own researches here, I feel convinced, in spite of assertions made by scientists to the contrary, that the exact height of the aurora may be ascertained by the method I advocate, and that from the observations made at these three stations we shall glean sufficient materials to solve a problem hitherto deemed an insoluble one.

Auroræ occur here, I may say without exaggeration, every night, and an evening without them would be a phenomenon as remarkable as their appearance under the equator. Unfortunately, however, unfavourable weather has during the last two months, accompanied by cloud masses unusual in these latitudes, sadly interfered with the number and completeness of my observations. Still, the magnitude of the auroræ is not the same every night. Sometimes they appear as short, faint, arc-shaped phenomena, similar to those so frequently seen in South Norway, while at others they assume an extent and grandeur which mocks every attempt at description.

In one respect my researches here have been of great moment to me, *i.e.* with regard to understanding the various types of the auroræ, their real strike and shape, and their exterior appearance, which changes in the different altitudes above the horizon; while on account of their frequency, and the circumstance that they now appear in the north, then in the south, and at last in zenith, there is a splendid opportunity to study the modifications which one particular form of aurora is subjected to when changing its position to the observer. It appears now conclusive to me that the many forms usually described in researches may be reduced to a few, almost similar, types. In most instances the aurora runs in zones, belts, in the direction of the magnetic east-west, and either as a more or less diffuse luminosity, or as thin shining bands, which I have found to be parallel with the indication of the inclination needle. But the appearance which the phenomenon assumes is entirely dependent on the relative position which the observer occupies to the same. If he is thus greatly distanced from the aurora he will only observe, a few degrees above the horizon, a continuous arc with streamers, but if he approaches nearer, he will notice several such arcs with clearly defined constituents and a greater vibratory motion, and if still closer, he will see the "belts" or bands mentioned by Weyprecht far above the horizon; and if these then travel towards *his* zenith, he will distinctly see the auroral "corona." I have just stated that the main strike of the auroræ is magnetic east-west; this is, however, only stated as a general rule, particularly with those of the luminous or "glory" type, while the "belts" may, besides their slight folds, be twisted and slung in almost any direction. I have thus seen them stretch from north to south, and even form a continuous circle, which, with zenith as centrum, has engirdled the entire heavens at an elevation of about 30°. The variable position of these luminous belts is the cause of the many peculiarities and the deviations from the normal which are so *frequently* observed with the arcs, as, for instance, their unsymmetrical position in relation to the magnetic meridian, and their uneven shape, viz. that they are often bent ecliptically back at the points, or even take the appearance of regular eclipses. I ought, however, to point out that the faint retrograding bend which great arcs assume near the horizon is due simply to optical causes. The study of the auroral corona here is very instructive. When a belt of streamers travels towards the magnetic zenith, the radiations seem to become shorter and shorter, caused by the circumstance that they are seen obliquely, and when the belt passes the magnetic zenith, its lower rim only is seen, which makes it appear as a bent and folded luminous belt. In this position one may observe that every individual streamer has only a very limited depth, but that the belt consists of several, sometimes of a great number, of luminous "sheets" in a parallel position to one another.

Besides this form of aurora, which thus embraces two kinds, viz. the continuous and the radiating, I know only one more of a character distinctly differing from the same. I do not thus consider the individual knots of ray-auroræ, or the streamers, as anything but incomplete belts; while the luminous gatherings I consider are merely remnants, so to say, of previously radiant auroræ. I may also here state that the large purple auroral clouds peculiar to this phenomenon, when observed during considerable electrical disturbances in southern climes, I have never seen at Kautokeino.

Of quite a different nature is, however, the phenomenon which I have named "coruscation." This phase of the aurora, which almost without exception belongs to the earliest hours of the morning, and after large and extended oscillations of the aurora, is developed, I believe, by the luminous clouds. But while these remain quiet, or show at least subdued oscillations, the "coruscation," as I term it, is so violent and of such a peculiar nature, that I have not even yet succeeded in ascertaining whether the motion is horizontal or vertical, or whether it is the luminous clouds themselves which flood the heavens, or their merely momentary "blazing up" under the influence of some passing waves of energy. The entire heaven is sometimes for hours a bath of liquid fire by this force, which seems, by the bye, to possess the same remarkable rapidity around zenith as at lower elevations.

As regard the colours of the auroræ, I have only noticed, when the substance of light is great, and when the oscillations are very rapid, two well-known forms, viz. green and red. These are, however, only seen in the arcs as their lower rim, and by the forward movement one part assumes a red, another a green tint. The red colour sometimes changes into violet or ochre.

The spectroscope I have not had much opportunity of using here, but the well-known auroral "line" I can always see; any others I have not observed.

With regard to the height of the aurora I have, judging from observation, come to the conclusion that it does not appear at a lower elevation here than it does in the south of Norway, while I am convinced that its plane is to be found far above that of the clouds. There has often enough been an opportunity of observing auroræ and clouds simultaneously, but never has there been the slightest indication of the auroræ having descended to the sphere of the clouds, not even under the most violent oscillations and the most intense luminosity and play of colour. In fact I have come to the conclusion that the auroræ which I have watched at Kautokeino are identical with those I have studied in southern latitudes, while their plane is at the elevation which I estimated when choosing Kautokeino as my station of observation.

I may in conclusion state that I have never myself heard the slightest approach to any auroral "noise," and this in spite of my most earnest attention to this so-much-disputed question. Still if I ask the native people (Lapps) about here as to the "noise" there is not a single one who doubts its existence, while several even assert that they have heard it.

I have several times attempted to photograph the aurora borealis, but without success. Thus even by using the most sensitive English "dry" plates, and exposing them from five to seven minutes, I have not obtained a trace of a negative. The cause of this is, I believe, the exceedingly limited substance of light possessed by the auroræ: were thus even the entire heavens flooded by the most intense auroræ, their aggregate lighting capacity would not equal that of the moon when full. I may therefore assume that photographing the aurora borealis is an impossibility. SOPHUS TROMHOLT

Kautokeino, Finmarken, Norway, January 28

PROFESSOR HUXLEY ON EDUCATION

ON the 16th inst., Prof. Huxley gave an address in connection with the distribution of prizes at the Liverpool Institute, a revised report of which will appear in the next number of the *Journal of Education*. By the courtesy of the editor of that journal, we are enabled to give a few extracts from Professor Huxley's address. He began by referring to certain propositions which he laid down in the address he gave in Liverpool fourteen years ago as to the practical value of instruction in physical science, its superiority to any other study as a mental discipline, and the certainty that in the future physical science would occupy a much larger share in the time allotted to teaching than had been the case previously. He also laid special stress upon the fact that he was no advocate of the exclusion of other forms of culture from education, but, on the contrary, insisted that it would be a serious mistake to cripple them for the sake of science. He had no sympathy, he said, with a kind of sect or horde of scientific Goths or Vandals who think that it would be proper and desirable to sweep away all other forms of culture and instruction except those in physical science. After referring to the great variety of his past experiences, his familiarity with every form of society, from the uncivilised savage of Papua and Australia, to the occasionally somewhat over-civilised members of our upper ten thousand, and to his interest in every branch of knowledge and form of art, Prof. Huxley insisted on the vast importance of science in education, when properly taught. He pointed out, however, that unless the knowledge conveyed in the teaching of science or in the teaching of history were actually realised to themselves by the learners, it would be worse than useless.

"Make it as little as you like, but unless that which is taught is based on actual observation and familiarity with facts it is better left alone. There are a great many people who imagine that elementary teaching might be properly carried out by teachers provided with only elementary knowledge. Let me assure you that that is the profoundest mistake in the world. There is nothing so difficult to do as to write a good elementary book, and there is nobody so hard to teach properly and well as people who know nothing about a subject; and I will tell you why. If I address an audience of persons who are occupied in the same line of work as myself I can assume that they know a vast deal, and that they can find out the blunders I make. If they don't, it is their fault and not mine; but when I appear before a body of people who know nothing about the matter, who take for gospel whatever I say, surely it becomes needful that I consider what I say, make sure that it will bear examination, and that I do not impose upon the credulity of those who have faith in me. In the second place, it involves that difficult process of knowing what you know so well that you can talk about it as you can talk about your ordinary business. A man can always talk about his own business. He can always make it plain; but if his knowledge is hearsay he is afraid to go beyond what he has recollected and put it before those that are ignorant in such a shape that they shall comprehend it. That is why, to be a good elementary teacher, to teach the elements of any subject, requires most careful consideration if you are a master of the subject; and if you are not a master of it it is needful you should familiarise yourself with so much as you are called upon to teach—soak yourself in it, so to speak—until you know it as part of your daily life and daily knowledge, and then you will be able to teach anybody. That is what I mean by practical teachers, and although the deficiency is being remedied to a large extent, I think it is one which has long existed, and which has existed from no fault of those who undertook to teach, but because until within the last score years it absolutely was not possible for any one in a great many branches of science, whatever his desire might be, to get instruction which would enable him to be a good teacher of elementary things. All that is being rapidly altered, and I hope it will soon become a thing of the past."

Then as to the important question of time, Prof. Huxley said that all he asked for was that scientific teaching should be put into what politicians and statesmen call the condition of the "most favoured nation"; that is to say, that it shall have as large a share of the time given to education as any other principal subject. On the important question as to what should be regarded as "principal subjects," Prof. Huxley remarked as follows:—

"I take it that the whole object of education is, in the first place, to train the faculties of the young in such a manner as to give their possessors the best chance of being happy and useful in their generation; and, in the second place, to furnish them with the most important portions of that immense capitalised experience of the human race which we call knowledge of various kinds. I am using the term knowledge in its widest possible sense, and the question is what subjects to select, by training and discipline in which the object I have just defined may be best attained. I must call your attention further to this fact, that all the subjects of our thoughts, feelings, and propositions, leaving aside the mere materials and occasions of thinking and feeling—our sensations as all our mental furniture—may be classified under one of two heads: as either within the province of the intellect, something that can be put into proposition and affirmed or denied, or as within the province of feeling, or that which, before the name was defiled, was called the æsthetic side of our nature, and which can neither be affirmed nor denied, but only felt and known. According to the classification which I have put before you then, the subjects of all knowledge are divisible into two groups, matters of science and matters of art; for all things with which the reasoning faculty alone is occupied come under the province of science, and, in the broadest sense, and not in the narrow and technical sense in which we are now accustomed to use the word art, all things feelable, all things which stir our emotions, come under the term of art, in the sense of subject matter of the æsthetic province. So that we are shut up to this,—that the business of education is, in the first place, to provide the young with the means and the habit of observation; and, secondly, to supply the subject matters of knowledge, either in the shape of science or of art, or of both combined. Now, it is a very remarkable fact—but it is true of most things in this world—that there is hardly anything one-sided or of one nature, and it is not immediately obvious what, of the things that interest us, may be regarded as pure science, and what may be regarded as pure art. It may be that there are some peculiarly constituted persons, who, before they have advanced far into the depths of geometry, find artistic beauty about it, but, taking the generality of mankind, I think it may be said that when they begin to learn mathematics their whole souls are absorbed in tracing the connection between the premisses and the conclusions, and that to them, geometry is pure science. So I think it may be said that mechanics and osteology are pure science. On the other hand, melody in music is pure art. You cannot reason about it; there is no proposition involved in it. So, again, in the pictorial art, an arabesque, or a 'harmony in grey,' touch none but the æsthetic faculty. But a great mathematician, and even many persons who are not great mathematicians, will tell you that they derive intense pleasure from geometrical reasonings. Everybody knows that mathematicians speak of solutions of problems as 'elegant,' and they tell you that a certain mass of mystic symbols is 'beautiful, quite lovely.' Well, you do not see it. They do see it, because the intellectual process, the process of comprehending the reasons symbolised by these figures and these signs, confers upon them a sort of pleasure, such as an artist has in visual symmetry. Take a science of which I may speak with more confidence, and which is the most attractive of those I am concerned with. It is what we call morphology, which consists in tracing out the unity in variety of the infinitely diversified structure of animals and plants. I cannot give you any example of a thoroughly æsthetic pleasure more intensely real than a pleasure of this kind—the pleasure which arises in one's mind when a whole mass of different structures runs into one harmony as the expression of a central law. That is where the province of art overlaps and embraces the province of intellect. And if I may venture to express an opinion on such a subject, the great majority of forms of art are not in the sense what I just now defined them to be—pure art; but they derive much of their quality from simultaneous and even unconscious excitement of the intellect. When I was a boy I was very fond of music, and I am so now; and it so happened that I had the opportunity of hearing much good music. Among other things, I had abundant opportunities of hearing that great old master, Sebastian Bach. I remember perfectly well—though I knew nothing about music then, and I may add know nothing whatever about it now—the intense satisfaction and delight which I had in listening by the hour together to Bach's fugues. It is a pleasure which remains with me, I am glad to think; but of late years I have tried to find out the why and wherefore, and it has often occurred to me that the pleasure, in musical compositions of this kind, is essentially of the same nature as that which is derived from pursuits which are commonly regarded as purely intellectual. I mean that the source of pleasure is exactly the same as in most of my problems in morphology—that you have the theme in one of the old masters' works followed out in all its endless variations, always appearing and always reminding you of unity in variety. So in painting; what is called truth to nature is the intellectual element coming in, and truth to nature depends entirely upon the intellectual culture of the person to whom art is addressed. If you are in Australia, you may get credit for being a good artist—I mean among the natives—if you can draw a kangaroo after a fashion. But among men of higher civilisation the intellectual knowledge we possess brings its criticism into our appreciation of works of art, and we are obliged to satisfy it as well as the mere sense of beauty in colour and in outline. And so the higher the culture and information of those whom art addresses, the more exact and precise must be what we call its 'Truth to nature.' If we turn to literature, the same thing is true, and you find works of literature which may be said to be pure art. A little song of Shakespeare or of Goethe is pure art, although its intellectual content may be nothing. A series of pictures is made to pass before your mind by the meaning of words, and the effect is a melody of ideas. Nevertheless the great mass of the literature we esteem is valued not merely because of having artistic form, but because of its intellectual content, and the value is the higher the more precise, distinct, and true is that intellectual content. And if you will let me for a moment speak of the very highest forms of literature, do we not regard them as highest simply because the more we know the truer they seem; and the more competent we are to appreciate beauty, the more beautiful they are? No man ever understands Shakespeare until he is old, though the youngest may admire him; the reason being that he satisfies the artistic instinct of the youngest and harmonises with the ripest and richest experience of the oldest. I have said this much to draw your attention to what, to my mind, lies at the root of all this matter, and at the understanding of one another by the men of science on the one hand, and the men of literature and history and art on the other. It is not a question whether one order of study should predominate or that another should. It is a question of what topics of education you shall select which will combine all the needful elements in such due proportion as to give the greatest amount of food and support and encouragement to those faculties which enable us to appreciate truth, and to profit by those sources of innocent happiness which are open to us, and at the same time to avoid that which is bad and coarse and ugly, and to keep clear of the multitude of pitfalls and dangers which beset those who break through the natural or moral laws."

Professor Huxley then went on to point out the worthlessness of the kind of literary education that used to prevail in English schools, and gave his idea of what a literary education ought to be. If, he said, he could make a clean sweep of everything, and start afresh, he would in the first place secure the training of the young in reading and writing, and in the habit of attention and observation both to that which is told them and that which they see; and he would make it absolutely necessary for everybody, for a longer or shorter period, to learn to draw, and there is nobody who cannot be made to draw more or less well.

"Then we come to the subject-matter, whether scientific or æsthetic, of education, and I should naturally have no question at all about teaching the elements of physical science of the kind I have sketched in a practical manner; but among scientific topics, using the word 'scientific' in the broadest sense, I would also include the elements of the theory of morals and of that of political and social life, which, strangely enough, it never seems to occur to anybody to teach a child. I would have the history of our own country and of all the influences which have been brought to bear upon it, with incidental geography, not as a mere chronicle of reigns and battles, but as a chapter in the development of the race and the history of civilisation. Then with respect to æsthetic knowledge and discipline, we have happily in the English language one of the most magnificent storehouses of artistic beauty and of models in literary excellence which exists in the world at the present time. I have said before, and I repeat it here, that if a man cannot get literary culture of the highest kind out of his Bible, and Chaucer, and Shakespeare, and Milton, and Hobbes, and Bishop Berkeley, to mention only a few of our illustrious writers—I say if he cannot get it out of those writers, he cannot get it out of anything; and I would assuredly devote a very large portion of the time of every English child to the careful study of the models of English writing of such varied and wonderful kind as we possess, and what is still more important and still more neglected, the habit of using that language with precision and with force and with art. I fancy we are almost the only nation in the world who seem to think that composition comes by nature. The French attend to their own language, the Germans study theirs; but Englishmen do not seem to think it is worth their while. Nor would I fail to include in the course of study I am sketching translations of all the best works of antiquity or of the modern world. It is a very desirable thing to read Homer in Greek; but if you don't happen to know Greek, the next best thing is to read as good a translation of it as we have recently been furnished with in prose. You won't get all you would get from the original, but you may get a great deal, and to refuse to know this great deal because you cannot get all seems to be as sensible as for a hungry man to refuse bread because he cannot get partridge. Finally, I would add instruction in either music or painting, or if the child should be so unhappy, as sometimes happens, to have no faculty for either of these, and no possibility of doing anything in an artistic sense with them, then I would see what could be done with literature alone; but I would provide in the fullest sense for the development of the æsthetic side of the mind. In my judgment these are all the essentials of education for an English child." Prof. Huxley concluded by saying that if the educational time permitted, there were one or two things he should be inclined to add to these essentials (which fitted an Englishman to go anywhere or to enter on any career); among these additional subjects he mentioned Latin and German. Beyond that, let each man take up his special line.

NOTES

The Emperor of Germany has raised Prof. Helmholtz to noble rank.

The two English observers, Messrs. Lawrence and Woods, detailed to secure photographs of the total eclipse of the sun on May 6, left Southampton for Panama on Saturday last. The operations will be exclusively photographic. The Treasury only determined to grant the necessary funds some fifteen days before the last date on which the observers could sail; the instruments sent out, therefore, were most hurriedly put together; and the greatest praise is due to Messrs. Hilger and Meagher for their work against time. Detailed instructions and a time table stating the work to be done for every second from ten minutes before totality till ten minutes afterwards, have been sent with the observers. If all goes well more than fifty photographs will be secured.

In reply to the Memorial addressed to the Council of the British Association on the subject of the proposed meeting of the Association in Canada in 1884, signed by 144 members of the General Committee, Mr. Bonney states that the Council of the British Association are fully alive to the difficulties which will attend the visit to Canada decided upon by the General Committee at Southampton in August last. As this decision was obtained in accordance with the usual forms and does not appear to contravene the express wording of the rules of the Association, the Council feel bound to recognise it as a valid one, and believe that they would not be justified in summoning a special meeting of the General Committee to reconsider the question. They have, however, in effect already taken steps to ascertain the general feeling of the members of the Association. In the month of November last, after a consultation with Sir A. T. Galt, the High Commissioner for Canada in this country, the officers of the Association addressed to their intending hosts in Montreal a number of questions, upon the answers to which the success of the projected visit must greatly depend. To these questions they are now daily expecting a reply. As soon as this is received, information will be given to the Members of the Association, and inquiries made as to their willingness to visit Canada. The replies will enable the Council to judge whether it will be possible to hold a successful and fairly representative meeting at Montreal.

M. Raoul Pictet has recently tried, on the Lake of Geneva, a specimen of his "rapid vessel," the general idea of which was indicated a short time ago. The vessel is figured in *Archives des Sciences* for January, and M. Pictet gives details of the theory and working. With a length of about 67 feet, and a width of 13 feet, this vessel is peculiar chiefly in having a bottom that is of parabolic form lengthwise, the concavity downwards; transversely the bottom is nearly straight; the sides are vertical. A keel reaching from about the middle of the length, incloses a screw shaft. Among other results M. Pictet shows that the force of traction of this vessel is always less than that of an ordinary vessel of the same general form and going at the same rate. The advantages of the parabolic curve only become apparent at a certain speed, depending on the width, length, and tonnage, and the parameters of the parabolic curve. The force of traction passes through a maximum, at a certain velocity for each vessel; beyond that point, the work of the motor, and so the expenditure of fuel, diminishes, though the speed increases. Experiment has yet to decide the limits of this second period. The emergence of the vessel, very small for small velocity, grows very quickly when a speed of 5 metres (say 17 feet) per second has been reached; and it converges rapidly towards an upper limit. The recoil of the screw for different velocities increases to a maximum, then constantly diminishes and tends to become *nil* for an infinite velocity. For other features of the action we must refer to the original. The engine we note proved faulty, and in several of the experiments the vessel was towed by a steamer, at velocities rising to 27 kilometres (say 17 miles); when this last is reached, an economy of one-half is realised (growing from 16 kilometres).

The recent death of the Rev. Titus Coan, an aged and much-esteemed missionary at Hilo, Hawaii (where he laboured nearly forty-eight years), has been announced (*Am. Journ. Sci.*). He took a deep interest in the volcanic mountain at whose foot he lived, and at each eruption was generally the first on the ground to observe and report on the movements. Three times he ascended to the scenes of the eruptions connected with the summit crater. Though not a geologist, his accounts (many of them in the journal named) have always been of geological value. He was the principal historian of the great eruption of Kilauea in 1840, and the summit eruption of 1843, when the flow was uninterrupted for twenty-five miles and continued six weeks. It was after the latter eruption that he made the very important observation (since confirmed) that

Kilauea, though 10,000 feet lower in level than the summit crater, showed no change, no signs of sympathy whatever.

A DEPUTATION from a number of the Scientific Societies of London had an interview with Sir John Lubbock on Tuesday, for the purpose of asking him to oppose the Bill to authorise the construction of a railway through Epping Forest. It was stated that the line would greatly destroy the natural beauty of the Forest, and that the existing means of access to it were abundant. Sir John Lubbock said he would be prepared to assist in opposing the Bill; but it was pointed out that, as the Corporation and the Verderers had given their sanction to the scheme, it would be difficult to secure its rejection.

THE death is announced of Herr Thomas Dickert, well known by his geographical relief-maps. He died on January 11 at Poppelsdorf, near Bonn, aged eighty-two. Also of Dr. Bohdalek, formerly Professor of Descriptive Anatomy at Prague University, who died at Leitmeritz on February 2.

AT a public meeting held in Glasgow last week, called at the suggestion of Sir William Thomson and Mr. John Burns of Castle Wemyss, it was agreed to collect the money to establish a permanent and efficient observatory on Ben Nevis. The building will cost 2000*l.*, the instruments 1000*l.* In all 5000*l.* are required, and of that sum 1400*l.* has already been subscribed. The Government has refused to assist in the matter.

M. TRESCA read before the Paris Academy of Sciences on Monday his report on the experiments of M. Marcel Deprez; the distance being exactly 17,000 metres instead of 20,000 as at first asserted, and the motive-power 6·21 horse instead of 5, the percentage is exactly 0·326, a little less than one-third. It may be supposed that the percentage of primary engines, telegraph wires, and secondary engines is 0·70, so that the result obtained is just $(0{\cdot}70)^3=0{\cdot}343$, almost exactly the real value. The measurements have been taken with accuracy, and no error can be adduced. The number of revolutions of the primary machine was 588 in a minute. Others were tried on Monday with 814 revolutions, but it is too soon to judge of the result. M. Tresca having declined to do so, an Academical Commission has been appointed to report upon M. Deprez's theories. M. Tresca praised Mr. Hutchinson who made the electrical measurement with apparatus brought from London for measuring differences of potential and number of amperes. The electrical measures were verified with dynanometers.

DR. WARREN DE LA RUE has been elected by the Committee a Member of the Athenæum Club under Rule 2, which provides for the admission of persons eminent in literature, science, or the arts, or for public services.

THE usual sitting of the Congrès des Sociétés Savants will take place in Paris on March 27, 28, and 29 next. The Minister of Public Instruction will preside over the concluding meeting on the 30th. For the first time the Academy of Aerostation has been summoned to send delegates.

FROM the beginning of the next financial year Kew Gardens will be opened an hour earlier than at present, viz. at 12 o'clock instead of 1.

AT the Technical College, Finsbury, the introductory address was given by Mr. Philip Magnus, Director and Secretary of the Institute, on Monday evening last. Sir Frederick J. Bramwell, F.R.S., was in the chair.

WE are glad to see that the new and spirited Scottish quarterly, the *Scottish Review*, does not neglect science. In the February number, which is just out, there appears an article on Medical Reform and an appreciative estimate of the late James Clerk Maxwell.

THE *Journal Télégraphique du Bureau Central de Berne*, summarising the principal lacunæ in the universal system of telegraphy, notes as one the construction of a line to Iceland for recording the principal atmospherical events observed in the Polar regions.

A DISTINGUISHED Swedish entomologist, Gustaf Wilhelm Belfrage, has recently died in Texas, where he had been for some years residing. The deceased had collected and forwarded a number of entomological specimens to the Swedish Academy of Sciences in Stockholm, for which he had received a State grant.

AN International Exhibition of Garden Produce and a Botanical Congress will be held in St. Petersburg this summer.

REPORTS from Lower Bavaria announce the discovery of auriferous and argentiferous sand deposits. They are confined to a layer of gneiss which occurs in the granitic rocks for a length of about fifteen or eighteen miles, between the villages of Innernzell and Zenting. It appears that 100 kilogrammes of the sand contain about 10 to 15 grammes of pure silver and between 2 and 10 grammes of pure gold; the sand from 4·6 metres depth is even richer. The weathered gneiss partly carries gold and silver and partly gold only; no special form is marked in the occurrence of the auriferous sand; there are deposits that seem to be alluvial, others which occur in the firm rocks, others again in distinct veins of mica slate, and still others in exposed gneiss which is many yards high.

IN a recent communication to the Vienna Academy, Prof. Graber, of Czernowitz, describes a long series of experiments with regard to the "skin-vision" of animals; affording exact proof that certain animals, without the aid of visual organs proper, can make not only quantitative but qualitative distinctions of light. These experiments relate chiefly to the earthworm, as representing the eyeless (or "dermatoptic") lower animals, and to the *Triton cristatus*, as representative of the higher ("ophthalmoptic") eyed animals. In a table Prof. Graber presents columns of numerical "coefficients of reaction," indicating how many times more strongly frequented a space illuminated with bright red, green, or white without ultra-violet, is, than one illuminated dark blue, green, or white with ultra-violet respectively, the conditions being the same as regards light-intensity, radiant heat, &c. In one set of experiments, the animals were in the normal state; in another, the anterior end of the worm, and the eyes of the triton were removed.

"CATALOGUES of the New Zealand *Diptera*, *Orthoptera*, and *Hymenoptera*, with Descriptions of the Species," by F. W. Hutton, F.G.S., Professor of Biology at Canterbury College, N.Z., have been published by the Colonial Museum and Geological Survey of the Colony. They consist of reprints of the original descriptions of such species in the orders named as have been described from New Zealand, without, as a rule, critical remarks, and form an amplification of Lists already published in the *Trans. N.Z. Institute*. Only 227 species for the three orders are enumerated. Although this publication is dated 1881, it has only just been received in England. In some respects it is already obsolete, especially in *Hymenoptera*. Mr. Kirby in 1881 enumerated 81 species in this order, Prof. Hutton enumerates about 71, which should be still further reduced from synonymic considerations.

THE Belgian Academy offers a prize of 3000 francs (120*l.*) for the best treatise on the destruction of fishes by the pollution of rivers. Several points are to be treated of which relate to the impurities which find their way into rivers from the principal branches of trade and the manufactures, and also to the practical means for rendering these impurities harmless. The treatises competing for this prize are to be sent in before October 1, 1885.

EARTHQUAKES are reported from Silesia and North-Eastern Bohemia. Two shocks were noticed on January 31, at 2.40 p.m., at Trautenau. Their direction was from south-west to north-east. They were also felt at Braunau, Jungbuch, Freiheit, Marschendorf, Grossaupa, Spindelmühle, and Johannisbad, and also at Gorbersdorf and Landeshut. The motion was undulatory and lasted from three to five seconds. No damage was done.

THE Paris papers report the extraordinary run of a small hydrogen gas balloon, capacity about two gallons, which, having been liberated at Bercy, was discovered at Grodno in Poland, having travelled more than two thousand miles; it is the longest air journey on record for so small an object.

THE French gas companies have instituted at their common expense a laboratory for testing the several inventions reported in electric lighting, and proving whether they are valuable or not. After alluding to this foundation, and the much-spoken-of experiments tried at the French Great Northern Railway Station, a French scientific periodical says: "Mieux vaut un sage enemi qu'un imprudent ami."

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, presented by Mr. J. F. Williams; a Punjaub Wild Sheep (*Ovis cycloceros* ♂) from North-West India, presented by Lieut.-Col. C. S. Sturt, C.M.Z.S.; a Thar (*Capra jemlaica*) from the Himalayas, presented by Lieut.-Col. Alex. A. A. Kinloch, A.Q.M.G., C.M.Z.S.; a Blyth's Tragopan (*Ceriornis blythi* ♂) from Upper Assam, a Fythch's Partridge (*Bambusicola fythchi*) from Upper Assam, presented by Capt. Brydon; a Small Hill Mynah (*Gracula religiosa*) from South India, presented by Dr. Rogers W. Taylor; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Common Cormorant (*Phalacrocorax carbo*), British, deposited; three Stump-tailed Lizards (*Trachydosaurus rugosus*) from Australia, purchased.

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—The following places for Berlin midnight are derived from Dr. Kreutz's ellipse:—

1883		R.A. h. m. s.	Decl. ° ′	Log. Distance from Earth.	Sun.
February	26	5 52 10	−15 43·3	0·4551	0·5122
	28	5 51 43	15 17·1	0·4629	0·5158
March	2	5 51 23	14 51·5	0·4705	0·5193
	4	5 51 8	14 26·5	0·4781	0·5227
	6	5 51 0	14 2·1	0·4856	0·5261
	8	5 50 57	13 38·4	0·4930	0·5295
	10	5 51 0	−13 15·4	0·5003	0·5329

Mr. E. E. Barnard, of Nashfield, U.S., reports that on the morning of October 14 he found to the south of the comet a large, distinct cometary mass, fully 15′ in diameter, and a similar but less bright object close beside this, their borders touching, and on the opposite side of the first a third fainter mass: the three were almost in a line, east and west. More of these cometary masses were found towards the south-east: there were at least six or eight within about 6° south by west of the head of the great comet. Their appearance was that of distinct comets with very slightly brighter centres, several being in the field at once. They were not seen again after being obscured by daylight on the morning of October 14.

Dr. Julius Schmidt's observations of a cometary mass near the head of the great comet are already published in No. 2468 of the *Astronomische Nachrichten*.

On the 5th inst., with the large refractor at Strasburg, the comet had two stellar nuclei, the fainter of the two on an angle of 246°, and 38″ distant from the brighter, which was observed for position. On January 27, Mr. Ainslie Common, of Ealing, with his large reflector, saw the nuclear part of the comet larger but less bright than previously, and resolved into a string of brightish points, the second and third of which were much the brightest. The position-angle was 240° 20′, and the distance between the brighter points was 31″·5, so that they doubtless correspond to the two "fixternartige Kerne" observed at Strasburg. In a sketch with which Mr. Common has favoured us, five points of condensation are shown; it was made at 9 p.m. on January 27.

VARIABLE STARS.—Dr. Julius Schmidt has published his usual summary of results of observations of variable stars, made at Athens in 1882. Minima of Ceraski's variable U Cephei occurred on November 25 at 8h. 57·2m. mean time at Athens, and on November 30 at 8h. 36·5m. Minima of Algol on November 29 at 11h. 30·4m., and December 2 at 8h. 7·1m., the first determined from observations extending over 5·4h., and the second from an interval of 7·5h. R Hydræ was at maximum on March 8, when it attained 4·3m. Mira Ceti at minimum on February 4, magnitude 9·5; the statement in some of our popular treatises on astronomy, that this star disappears at minimum is erroneous; its average brightness at that time is about 9m. on Argelander's scale, according to the most experienced observers. χ Cygni was at maximum September 1·5, the predicted date being August 25. The variations of α Herculis during the year were small, but well fixed by numerous observations; the period, as usual, irregular; the same may be said of *g* Herculis. T Cephei at maximum on January 11, 6·7m., the increase of light much quicker than the decrease; V Coronæ at maximum September 15·6; the fine variable R Leonis was at maximum on May 20, 6·5m., and at minimum on November 6, 9m.; R. Piscium at maximum on December 5·3, the increase of light slower than previously; Palisa's variable in Scorpio at maximum July 9·7, 12m.; of R Scuti, a maximum occurred October 11, well-determined minima, on June 21 and December 6; Harding's variable R Virginis was at maximum April 16·6, and at minimum June 30·5, the limits of brightness being 7m. and 11·7m.

It is much to be desired that the number of observers of these interesting objects should be largely increased; their observation opens up a field of useful work, even to an amateur with the most modest of optical appliances. At present our knowledge of the subject is mainly due to the systematic labours of the indefatigable director of the Observatory at Athens.

A NEW NEBULA.—Mr. Barnard notifies his discovery of a new nebula 1° 48′ north, and 5m. 39s. west of φ Virginis. It was observed with the 15-inch refractor at Harvard College by Mr. Wendell, and described as "rather diffuse and faint, but gradually a little brighter in the middle"; its position for the beginning of 1882 is in R.A. 14h. 16m. 19·6s., Decl. +0° 9′ 14″. This nebula is not found in the Harvard Zones, Nos. 53 and 54, observed on May 9 and 11, 1853, and which would overlap its place, though three new and faint nebulæ were first detected in those Zones, viz. Nos. 33-35 of Prof. Auwer's Catalogue of new nebulæ in the Konigsberg observations. This object may be worth watching, on the score of possible variability.

GEOGRAPHICAL NOTES

IN NATURE last week we announced that an Arctic expedition this summer had been decided on in Sweden. This expedition, which has been promoted by the well-known Swedish Mæcenas, Dr. Oscar Dickson, will be in command of Baron Nordenskjöld, whose intention it is on this occasion to explore the east and north-east coast of Greenland. It was originally his intention to have proceeded this summer into the Siberian seas, but seeing the delay caused to the Danish Polar Expedition, which will now be there during the summer, this idea was abandoned and Greenland decided on instead. Baron Nordenskjold, having formerly visited the country, is of the opinion that some kind of "break," or oasis, is to be found in the interior of Greenland. He purposes to proceed along the east coast of Greenland, as far as the ice will allow, and then to penetrate into the interior, some 300 miles across the inland ice. The country inland is nearly the whole year covered by ice and snow, which, during the summer months, render it almost entirely one bog. The enormous stretch of inland ice has also always been a barrier to exploration. Another object in view by Baron Nordenskjold is to attempt to find traces of the Norse colonies, which existed in Greenland from about the year 1000 until the end of the 14th century. The ultimate fate of the Norse settlers in Greenland is shrouded in mystery, as there is no authentic record of their existence after the end of the fourteenth century. There has also in later days been great diversity of opinion where to seek for the settlements; thus the Danish explorer Graah, who, in the years 1828-31, searched for remains of the same, sought them west of Cape Farewell, but without success. Baron Nordenskjold is, however, of the opinion that the Österbygd and the Norse settlements were situated east of the Cape, and it is here that he intends to search for them. It is hardly necessary to enlarge on the interesting and important results to science which would accrue from the discovery of these "dead cities" on the shores of the Arctic Ocean. Baron Nordenskjold will start on his journey early in May next, and although the general expenses of the expedition, no doubt, will be defrayed by King Oscar and Dr. Oscar Dickson, it is the intention of the latter to apply to the Swedish Parliament for the use of one of the vessels of the Navy for the voyage.

More details have now reached us concerning the expedition of the African travellers, Lieut. Wissmann and Pogge. The travellers proceeded along the Kassai River during the autumn of 1881, passed through Kimbunda and reached Kidimba, the residence of Chingenge, the chief of the Tooshilange tribe, in November. Then they proceeded northwards. They reached the frontier of the West African savannah-forests and entered upon the densely populated prairies of Central Africa. In the middle of December they reached the Mukamba Lake. Now they traversed the well-populated country of the Bashilange and reached the Lubi, a magnificent river bordered by rich tropical vegetation, and which is a tributary of the Lubilash river. The opposite shore of the Lubi is inhabited by the Bassonge, a handsome and powerful tribe, which possesses numerous clean and cheerful villages adorned by palm and banana trees. On January 14, 1882, the travellers reached the capital on the left bank of the Lubilash, in 5° 7′ 18″ lat. S. Kachich, the chief of the Kotto district, whose power is based upon his reputation of fetishero (high priest), caused many obstacles to be thrown into their way. At last, on January 29, the expedition crossed the Lubilash, which is identical with the Sankura, and which flows into the Congo. This was in 5° 13′ lat. S. Then they passed through well-watered prairies, inhabited by the warlike Bassonges, by the Beneckis, who have villages 17 kilometres in length, and the Kalebues, reaching and crossing the Lomami River on March 8. All these tribes are cannibals. Between the Lubi and Lake Tanganyika, Wissmann found remains of what must once have been the natives of these parts, viz. the Batuas, little, undergrown, slender, dirty, and savage-looking people, who live only by the chase and on wild fruit, speak a curious language, and whose arms and implements indicate a very low state of civilisation. The Lomami was crossed in 5° 42½′ lat. S. The direction towards Nyangwe was now taken through flooded prairies and marshes, alternating with parts where the grass had grown to a perfect carpet resembling felt. The Lufubu River was crossed on April 2. By April 11 two canoes had been made. On April 16 the expedition reached the Lualaba River, and Nyangwe on the 17th, where they were well received by the Arabs. Here they resolved to separate. Pogge was to return to the Mukenge Station with the caravan, and Wissmann to the east. On May 5 Pogge left. Wissman started on June 1 with only a few companions, and eventually reached Cassongo and then Lake Tanganyika. At Manyema he had gone south of Stanley's and Cameron's route, and afterwards crossed it at Ca=Bambarre, passing northward into the land of the Wasi-Malungo and Ubngwe tribes towards Uguhla. On the shores of Lake Tanganyika Wissmann rested for fourteen days, staying at the missionary station of Ruande. He made an excursion to the Lukuga River and crossed the lake to Ujiji. On August 9 he left the caravan track, proceeding in a northerly direction to Uhha, to visit the renowned chief, Mirambo. Passing through many great dangers he reached Mirambo's residence, and was most hospitably received. On September 3 Wissmann reached the French mission-station at Tabora, from whence he made an excursion to the German African Society's station at Gonza. There he considered his geographical work as completed, inasmuch as Dr. Kaiser had proceeded to Gonza from the east coast. Wissmann found Dr. Boehm and Reichard both in good health, Dr. Kaiser having left a few days before. On November 18 Wissmann reached the east coast near Saadani.

It is announced by the hon. secretaries of the Egyptian Exploration Fund that Sir Erasmus Wilson, LL.D., F.R.S., has accepted the office of President of the Society, and has headed the subscription list with a donation of 500*l.* Thus launched, the Society has commenced excavations at Tel-el-Maskhûta, in the Wady Tumilat—this mound being the supposed site of Raamses, one of the two cities specified in the first chapter of Exodus as built by the forced labour of the Hebrews. M. Edouard Naville, the eminent Swiss Egyptologist, in co-operation with Prof. Maspero, has undertaken the direction of the excavation on this important site, where he is now at work, aided by an experienced engineer, and a gang of eighty labourers. The results to be anticipated from discoveries at Tel-el-Maskhuta are inscriptions which shall enable Egyptologists to identify the Pharaoh of Moses, to assign a dynastic date to the period of the oppression, and to settle the much-disputed question regarding the route of the Exodus. More funds are needed for the prosecution of the work already begun, and it is hoped that the public will liberally support the action of Sir Erasmus Wilson. Pending the election of a treasurer, subscriptions will be received by the hon. secretaries, Mr. Reginald Stuart Poole, British Museum, and Miss Amelia B. Edwards, the Larches, Westbury-on-Trym, Bristol.

In the March number of *Petermann's Mittheilungen* the principal paper is an account of Herr Fr. von Schenck's journey in the United States of Columbia in 1880, an important contribution to the physical geography of a country on which we have no very recent information. Dr. Capus gives some interesting information on the valley of Yagnan and its inhabitants, about 170 versts east of Samarcand. There is a brief sketch of Herr Schuver's journey to the sources of the Tumat, Jabus, and Jal, in the region lying between the Upper Bahr-el-Azrek and Bahr-el-Abiad. This number contains the Necrology for 1882. —In Nos. 10, 11, and 12 (in one) Band xxv. of the *Mittheilungen* of the Vienna Geographical Society is a paper, with map, by Dr. J. Morstadt on the mountain structure of South Tyrol. An important work in ten vols. on the peoples of Austria-Hungary, by many authors (Vienna, Prochaska), is reviewed by Dr. Paulitschke.—Nearly the whole of the *Compte Rendu* of the Paris Geographical Society for December 15 is occupied by M. Desiré Charnay's account of his explorations in Yucatan.

ON THE PRESENT CONDITION OF THE SODA INDUSTRY

An interesting and important paper with the above title was read by Mr. Walter Weldon, F.R.S., at a meeting of the Society of Chemical Industry held at Burlington House on January 8. The following abstract is condensed from this paper as published in the *Journal* of the Society:—

A few years ago there were twenty-five alkali-works in the neighbourhood of Newcastle-on-Tyne; now there are only thirteen. Seven or eight works are standing idle in Lancashire; in Belgium the manufacture of soda by the Leblanc process has entirely ceased. The following table represents the

Present Soda Production of the World in tons

	Leblanc soda.	Ammonia soda.	Totals.	Ammonia soda per cent of total soda.
Great Britain	380,000	52,000	432,000	12·0
France	70,000	57,125	127,125	44·9
Germany	56,500	44,000	100,500	43·8
Austria	39,000	1,000	40,000	2·5
Belgium	—	8,000	8,000	100·0
United States	—	1,100	1,100	100·0
Totals	545,500	163,225	708,725	23·0

The ammonia process for making soda dates, as a practical manufacturing method, from 1866, in which year M. Solvay of Brussels established works at Couillet, near Charleroi. M. Solvay is now manufacturing soda by the ammonia process at the rate of about 75,000 tons per annum.

The production of soda has very rapidly increased on the Continent within the last five years; the greater part, but not the whole, of this increase is due to the introduction of the ammonia process. The production of soda by this process in England is entirely in the hands of one firm—Messrs. Brunner and Mond: in 1875 this firm produced 2500 tons of soda, in

1880 they produced 18,800 tons, and their output is now at the rate of 52,000 tons per annum. The new works now in course of construction in this country and on the Continent, when completed, will at once increase the production of ammonia soda by 65,000 to 70,000 tons annually.

What then can the manufacturer of Leblanc soda expect save utter collapse? But the state of the alkali-maker threatens to become even worse than it is. The source of the sulphur which is used in the Leblanc process is pyrites; the pyrites employed in this country is almost exclusively imported by three large companies from Spain and Portugal; it contains from 2 to 3 per cent. of copper, and very small quantities of silver and gold. When the soda manufacturer has burnt off the sulphur, he sends the residual ore to the copper extractor, who is able to sell the iron oxide which remains when he has taken out the copper at about 12*s*. per ton. Now the French soda-manufacturers make use of pyrites of their own, which contains little or no copper; one of the large companies which supplies the English market purposes, therefore, to start works in France, which shall employ Spanish pyrites, but which shall depend for their profits, not on the soda which they manufacture, but on the copper and iron oxides remaining after the sulphur has been burnt off from the pyrites. This company, which starts with a capital of over a million sterling, speaks of building five large works in France, and one in the neighbourhood of Antwerp.

The Leblanc soda manufacturers have tried to persuade themselves that the price of ammonia must rise considerably, and that thus they may be able to compete with the ammonia soda-makers on more equal terms than at present. But in place of ammonia becoming dearer, its price is steadily falling. New sources of ammonia are being found; a process for collecting ammonia and other volatile products from coke-ovens, which is easily applied to existing ovens, has recently been patented by Mr. J. Jameson of Newcastle-on-Tyne. If this method should be generally applied to the coke-ovens in this country, a quantity of ammonia corresponding to 180,000 tons of ammonium sulphate, worth about three and a half millions sterling, would be annually saved.

Mr. Ferrie—a member of the great iron firm of William Baird and Co.—has also contrived a method whereby the ammonia and tarry matters which are present in the gases of the blast furnace may be condensed; this process has been at work for some time at Gartsherrie, and by its help about 20 lbs. of ammonium sulphate are obtained per ton of coal burnt in the blast furnaces.

Another difficulty which presses heavily on the manufacturer of soda by the Leblanc process consists in the want of an outlet for the great quantities of hydrochloric acid which accumulate during the soda manufacture.

This difficulty is not felt by the Continental manufacturer because he finds a ready market for the chlorine which can be extracted from hydrochloric acid; but in England the supply of chlorine at present much exceeds the demand. But Mr. Weldon holds out hopes to the English chlorine-maker; he says: "I think that our English manufacturers of Leblanc soda will have to cease to devote their hydrochloric acid—when they do not throw it away—exclusively to chlorine making; . . . the difficulty hitherto has been how to turn it to account otherwise. I believe that difficulty is about to disappear. I am not free to enter into that matter now; . . . but I have very great confidence that new applications of hydrochloric acid, admitting of being applied very extensively, at comparatively small expense, are among the things of the immediate future."

Mr. Weldon then considers the ways in which the English manufacturer of Leblanc soda may hope to recover himself and again make soda at a reasonable profit. First of all, he must get his pyrites about 50 per cent. cheaper than the price he now pays for it; the present combination between the pyrites companies will expire at the end of next year; after that time the price of pyrites must, in Mr. Weldon's opinion, fall very considerably.

Secondly, the soda-manufacturer must recover all the sulphur in his alkali waste; if he can recover the sulphur at a cost not exceeding 2*l*. per ton; he will become master of the sulphur market, as the actual cost of Sicilian sulphur delivered at Marseilles is now about 5*l*. per ton.

The third thing which the soda-manufacturer must do is to distil the coal which he now uses as fuel, condense and sell the volatile products, including tar, oils, and ammonia, and employ the residual coke as fuel; he will thus get his fuel for nothing, and at the same time will confer an inestimable boon on the towns where coal is now largely used as fuel.

These three courses, says Mr. Weldon, must be all adopted by the English soda-maker. If, in addition to doing this, the strictest economy in manufacture is practised and the purest and best product that can be made is always turned out, the manufacturer of soda by the old Leblanc method may yet hope to hold his own against the new and wonderfully successful ammonia process.

M. M. P. M.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The following persons have been elected Members of the Committee for the nomination of examiners in the Natural Science Schools: Prof. R. B. Clifton; Prof. W. Odling; and Prof. H. N. Moseley. The Vice-Chancellor and Proctors complete the Committee. Up till this term the nomination of examiners lay with the Vice-Chancellor and Proctors, who appointed in turn.

The Examiners for the Burdett-Coutts Geological Scholarship have recommended Mr. F. W. Andrews of Christ Church, for election.

Magdalen College advertises a demyship in Natural Science to be competed for in June.

CAMBRIDGE.—The following further appointments of Boards of Electors to Professorships have been made.—

Mineralogy:—Prof. Story-Maskelyne (Oxford), Dr. H. C. Sorby, Profs. Stokes, Warrington Smyth, and Liveing, Dr. Phear, Dr. Percy, and Mr. Glazebrook.

Mental Philosophy and Logic:—Prof. Croom Robertson (Univ. Coll. Lond.), J. B. Mayor (King's Coll. Lond.), and Adamson (Owens College), Messrs. H. Sidgwick, J. Ward, I. Todhunter, Shadworth H. Hodgson, and the Master of Trinity College.

Music:—Sir F. Ouseley, Messrs. Pole, T. P. Hudson, G. Grove, Sedley Taylor, G. F. Cobb, R. Pendlebury, and E. S. Thompson.

MR. ALBERT SCHAFER, F.R.S., Fullerian Professor of Physiology at the Royal Institution, has been appointed Jodrell Professor of Physiology at University College, London, in the vacancy occasioned by the resignation of Dr. J. Burdon Sanderson, LL.D., F.R.S., appointed Wayneflete Professor of Physiology in the University of Oxford.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, February 1.—Dr. Gilbert, president, in the chair.—The following were elected Foreign Members:—F. Beilstein, P. T. Cléve, H. Debray, E. Erlenmeyer, R. Fittig, H. Helmholtz, D. Mendeleeff, Victor Meyer, Lothar Meyer. The following were elected ordinary Fellows:—H. C. Bond, G. C. Basu, J. Brock, A. M. Chance, J. T. Donald, H. C. Foote, W. Fox, W. R. Flett, J. A. M. Fallon, E. C. Gill, F. Gothard, J. Hunter, H. Jones, B. R. Lee, A. H. Jackson, Joowansinghi, T. Jenner, J. E. Johnson, W. W. J. Nicol, F. W. Richardson, E. S. Spencer, C. A. Serré, T. Turner, J. E. Tuit.—The following papers were read:—On derivatives of fluorene, by W. R. E. Hodgkinson and F. E. Matthews. The fluorene was crystallised five or six times from alcohol; it melted at 113°; when pure, it does not fluoresce. A dibrom and monobrom derivative were obtained, and a fluorene sulphonic acid; by the action of caustic potash on the potassium sulphonate, a trihydroxy-diphenyl was formed; and by dropping the hydrocarbon into fused caustic potash, a dihydroxy-diphenyl was procured.—On the action of chlorine on certain metals, by R. Cowper. As observed by Wanklyn, dry chlorine has no action upon melted sodium; the author finds that dry chlorine has no action upon Dutch metal, zinc, or magnesium; it acts very slowly upon silver and bismuth; tin, arsenic, and antimony are attacked rapidly, with evolution of heat.—Some notes on hydrated ferric oxide, and its behaviour with sulphuretted hydrogen, by L. T. Wright. The author found great difficulty in obtaining ferric hydrate, by precipitating the chloride with ammonia, free from basic chloride. Having poured some ferric chloride into an excess of ammonia, he evaporated to dryness at 100°. The residue, when treated with water, gave a reddish solution which would not yield a clear filtrate, some of the iron being probably in the so-called "colloidal" condition. Such ferric hydrate is not turned black by sulphuretted hydrogen; ordinary ferric hydrate is turned black at once, and the sulphide of iron dissolves in excess of potassium cyanide, forming potassium sulphide and ferrocyanide.—On alpha cyanonaphthalene sulphonic acid, by W. K. Dntt. The author first prepared the naphthalene sulphonic acid, then distilled the potassium salt with dry potassium ferrocyanide, and converted the cyanonaphthalene by sulphuric hydrochloride into the above substance.

The Institution of Civil Engineers.—February 13, Mr. Brunlees, president, in the chair. The paper read was on "The Design and Construction of Repairing-Slipways for Ships," by Mr. T. B. Lightfoot, M. Inst. C.E., and Mr. John Thompson.

Edinburgh

Royal Society, January 29.—Mr. Thomas Stevenson, M.Inst.C.E., vice-president, in the chair.—Dr. Knott read a paper by Mr. H. R. Mill on the rainband, being a description of the author's observations during the last six months of 1882. The observations were all made with Mr. Hilger's smallest size of pocket spectroscope, in which the presence of the rainband is indicated only by an apparent broadening of the D line. Mr. Mill measured the varying intensities of the rainband by comparing D with the other evident lines in the spectrum—E, *b*, F. The distinctness of the fine lines in the green was also found to be an additional factor in prognosticating the weather; the less distinct these lines the greater the chance of rain. An analysis of the observations showed that of the "rain" predictions 78 per cent. came true; of the "no rain" predictions 64 per cent.—The Rev. J. L. Blake read his third communication on the theory of monopressures applied to rhythm, accent, and quantity.—Mr. John Aitken read a paper on the effect of oil on a stormy sea, in which it was proved by experiment that the presence of the oil film did not calm the waves, but merely prevented them from breaking. The reason given was that the wind had no power to produce wavelets on the oil-surface, since in virtue of the action of surface-tension any forward motion of a portion of the oil-film necessitated the forward motion of the whole. In the case of a clean water surface, again, the wind acting strongly upon any small surface portion would push it *over* the contiguous surface, and so give rise to a wavelet. Some beautiful experiments on the effect of surface-tension were shown as bearing upon the subject.—A note was read from the Astronomer-Royal for Scotland calling attention to the remarkably high temperature maximum which had occurred some time during the preceding night.

Cambridge

Philosophical Society, February 12.—The following communications were made to the Society:—On the isochromatic curves of polarised light seen in a uniaxal crystal cut at right angles to the optic axis, by Mr. R. T. Glazebrook.—On a spectrophotometer, by Mr. R. T. Glazebrook. The paper describes an arrangement for viewing simultaneously the spectra formed by the light from two different sources after traversing the same set of direct-vision prisms. These two spectra are polarised in two planes at right angles and their relative intensity is determined by the position of a Nicol in the eye-piece through which they are observed.—On a common defect of lenses, by Mr. R. T. Glazebrook. The author exhibited some lenses which, when placed between two crossed Nicol's prisms, showed strong elliptic polarisation.—On the motion of a mass of liquid under its own attraction, when the initial form is an ellipsoid, by Mr. W. M. Hicks.—On functions of more than two variables analogous to Tesseral Harmonics, by Mr. M. J. M. Hill.—Observations of the transit of Venus across the sun, taken near Kingston, Jamaica, December 6, 1882, by Dr. J. B. Pearson. In this paper the author described observations taken by himself of the late transit of Venus. He unfortunately missed seeing the first external contact, and only first saw Venus when she had intruded about one-third of her sphere on the sun's disc. On the internal contact he noticed no kind of black drop, or sympathetic attraction or assimilation between the limb of the planet and that of the sun. It seemed to him that when the planet was actually projected on the sun's disc, about 20″ before the time he assigned for actual contact, the black surface of the planet adjoining the atmosphere seemed to begin to be picked out with little white dots commencing very probably from either side. He could not say that he actually saw two horns of light gradually advancing until their points touched, but rather that the segment of the planet nearest the sun's limb, and still obscure, began to be speckled with white dots which in not more than twenty seconds, or twenty-five at the outside, developed into a white line. He saw nothing like an atmosphere around Venus, though he looked carefully for it; it was possible that his telescope, considerably smaller than what might be called the authorised size, was not large enough to show it.

Berlin

Physiological Society, January 26.—Prof. du Bois-Reymond in the chair.—Prof. Fritsch, who, in his study of the torpedo at the zoological stations of Naples and Villafranca, has discovered, in addition to the facts already published, a series of new facts in reference to the development of this electric fish, combined these facts with those already discovered by previous investigators, and thus produced a general sketch of the development of this remarkable animal before the Society, illustrated by numerous preparations. The torpedo exhibits so many different forms in its ontological development that already de Sanctis distinguished a squaliform stage, a raiform stage, and a torpediform stage; and in fact the different stages, as the lecturer demonstrated in his series of preparations, first resemble shark-embryos, afterwards pass over into the form of rays, and finally change into that of torpedoes by the development of the electric organ. The first embryonic beginnings of the electric organ have the greatest resemblance to embryonic muscular fibres. Upon longitudinal section, there are to be seen in the interior of sheaths consisting of connective-tissue cells very distinct longitudinal fibrous striæ, with traces of transverse striation and many oval nuclei. In a later stage, on making a longitudinal section, the longitudinal fibration and transverse striæ are seen to have entirely disappeared; the nuclei have become much more numerous and circular, and in the interspaces the disc-like elements of the pillars that are to be developed are already to be seen as transverse striæ. The whole represents, in a sheath of connective-tissue, a granular mass of protoplasm with numerous nuclei. On making a transverse section, we see in the first stage, in which the organ resembles embryonic muscular-tissue, the cut ends of the longitudinal fibres as circular contours in an homogeneous connective-tissue. When the electric organ is further developed, there is seen, on making a transverse section, a polygonal net of connective-tissue, in whose meshes the round pillars lie, being separated from the walls by cellular masses. Hence Prof. Fritsch believes that the histological development of the electrical organ is analogous to the transformation of normal muscle in myomata, and that it would not be incorrect to call the electric organ a normal myoma. The phylogenetical development of the torpedo has already been described in the account of its ontogenetical development. The electric organ is developed from muscle, and indeed from the outer gill-muscles of the fifth gill-arch. The gill-arch muscle, which develops in rays and sharks into the extraordinarily powerful lower-jaw muscle, is wanting in the torpedo, and in its place we find the electrical organ, which is, comparatively speaking, a more serviceable weapon of offence and defence to the small animal than the lower-jaw muscle of the related predatory-fishes. The lecture was illustrated by a great number of microscopic and macroscopic preparations.

Physical Society, February 2.—Prof. v. Helmholtz in the chair.—Dr. Hertz described a series of peculiar light-phenomena which he had observed in the case of electric discharges. When, in a moderately rarefied space (pressure about 20 to 30 mm. of mercury), the electric discharge takes place between electrodes, one of which is fixed in a tube that is closed at one end and drawn out to a small opening at the other end, while the second electrode is placed laterally near the opening of the tube, the spark of discharge springs from the opening, laterally, to the second electrode; at the same time, however, one sees a ray of yellow-brown light break forth from the tube, reaching out a few centimetres in the prolongation of it. With stronger or with weaker pressure, the ray is shorter and less luminous; and if a Leyden jar be inserted, the ray is also shorter, but it is more luminous. The form of this ray (which broadens at the end) is very varied; and if it impinges on the wall of the vessel inclosing the rarefied space, it produces whirling there. The colour of the ray is different according to the gas: yellow with air and oxygen, blue with hydrogen, &c., and spectrum analysis shows that it is the respective gases that glow. If a small mica-disc be introduced into the luminous ray, it enters into oscillation; and a small mill is set in rotation by the ray. This proves that real material particles, glowing masses of gas, are driven forth in the discharge from the tube. The wall on which the ray impinges is strongly warmed, and a thermometer put into the ray rises 10° to 20°. If the ray, which to the naked eye seems quite continuous, be looked at through a slit in a rotating disc, so arranged that the slit, in different, very short intervals of time after each opening of the primary current of the induction-coil, passes before the eye, one sees in the first moment a small ray at the opening, then, at a later moment, a small cloud above the opening, and finally a larger luminous cloud floating at a greater distance from the opening. The light-ray is thus discontinuous; and at each spark-discharge separate clouds of glowing gases are driven out from the tube, which are ever enlarging. Even at atmospheric pressure these light-phenomena may, with careful observation, be perceived. They occur mostly in the air as yellow sheaths about the aureoles of the sparks, and with different electrodes present manifold forms: sheaths, swellings, whirls, and the like. In moist air the phenomenon is quite absent, and in hydrogen it soon ceases. The great variety of the appearances have not yet been brought under one common standpoint.—Dr. Goldstein had observed similar phenomena to those just described by Dr. Hertz, and made a number of experiments regarding them. In spectral tubes he saw the yellow light appear at the places of passage from the thin to the wider parts, in cylindrical tubes, on the other hand, the yellow light always surrounded the red discharge-light as an envelope, which in the neighbourhood of the cathode gradually widened, and from there progressively filled the tube. If evacuation be effected during the discharge, one sees that the yellow light, with the air, is driven out of the tube. This glowing of the gas Dr. Goldstein connects with the long-known after-luminosity of Geissler tubes, which he has sometimes found to last many seconds, and even some minutes, after discharge. The essential thing in the case of phosphorescent Geissler tubes is the change between wider and narrower parts, because only at the places of transition does the after-luminosity develop that light—yellow in air, blue in hydrogen, and other colours in other gases.

PARIS

Academy of Sciences, February 12.—M. Blanchard in the chair.—The following papers were read:—On the difference of barometric pressures at two points of a given vertical, by M. Jamin. He shows from records of the double observatory at the base and at the top of the Puy de Dôme, for 1880, that the difference of pressures varies very regularly every day and throughout the year, diminishing till 3 p.m., then increasing till sunrise, also increasing from the summer to the winter solstice. Kaemitz, in 1832, proved such variation with the season in Switzerland. Similar effects, due to temperature, doubtless occur everywhere. We have to conceive an atmospheric enlargement, a kind of air-tide, moving round with the sun. The resulting phenomena are complex. M. Jamin shows how the variations of the difference of pressures in a given vertical, with changes of temperature, pressure, and hygrometric state, may be calculated.—Researches on chromates, by M. Berthelot.—On the groupings of the animal world in primary times (second note), by M. Gaudry. Each of the epochs seems to have had special expansions, beings that began with it and ended with it. The irregularities met with do not favour the idea of a struggle for life in which the victory was to the strongest and best-endowed. There are many striking personalities, *rois de passage* (so to speak), giving the epochs a character of their own, so that as we speak of the age of Charlemagne, &c., we may say the age of *Paradoxides*, of *Pterichthys*, &c. But it is often the most specialised and perfect beings that have disappeared. Other types, representing the just mean, have persisted.—On the numbers of unequal ordinary fractions which may be expressed by using figures which do not exceed a given number, by Mr. Sylvester.—Refutation of a second critique by M. Zeuner, &c. (continued), by M. Hirn.—Researches on the *rôle* of inhibition in a special kind of sudden death, and with regard to the loss of consciousness in epilepsy, by M. Brown-Séquard. The losses of function and activity of the brain, in certain cases, are pure effects of inhibition, arising from irritation more or less distant.—Influence of subterranean humidity and of capillarity of the soil on the vegetation of vines, by M. Barral. The fruitfulness of the vine on the sandy soil of Aigues-Mortes is due to abundant water in the subsoil (from 1 m. depth) rising to the roots by capillarity. The author describes several laboratory experiments.—On treatment of the vine with sulphur in Greece, by M. Gennadius. This treatment (for oidium) is thought successful only if carried out on a day without wind, rain, or clouds, and with a burning sun. This fine weather must last twenty-four hours. It is the sulphurous vapour, and not the sulphur powder, that kills the spores in the air and on the vine, though the powder may act mechanically (and other fine powders will do the same) by protecting tender parts from contact with spores.—On germinated wheat, by M. Ballard. The gluten is profoundly altered; there is more acidity and more sugar and lignin; less fatty matter.—On the relations that exist between covariants and invariants of binary forms, by M. Perrin.—On the theory and experiments of MM. Mercadier and Vaschy tending to establish the non-influence of the di-electric on electro-dynamic actions, by M. Lévy.—General method for strengthening telephonic currents, by Mr. Moser. He introduces more induced coils.—On chlorides of lead and of ammonia, and oxychlorides of lead, by M. André.—Preparation of ethers of trichloracetic acid, by M. Clermont.—Contribution to the study of isomerism in the pyridic series, by M. Œchsner de Coninck.—On the relative toxical power of metallic salts, by Mr. Blake. His tabulated data of experiments show why he cannot accept the law formulated by M. Rabuteau (that metals are more active the greater their atomic weight and the smaller their specific heat).—Penetration of actinic radiations into the eye of man and of vertebrate animals, by M. de Chardonnet. He finds that no medium of the eye is transparent for the ultra-solar radiations, that is, for waves shorter than T or U, the limits of the ultra-violet solar spectrum. The mitilating membrane in sparrow-hawks and fowls is translucid for part of the ultra-violet spectrum (up to O and Q). The absorbing power of the vitreous humour, cornea, and crystalline lens varies in different species. The general fluorescence corresponds to actinic absorption, but there are exceptions.—New researches on the production of monsters in the hen's egg by the effect of late incubation, by M. Dareste. This takes place more slowly in winter than in summer. Also eggs of the same age grow old more or less quickly.—On the tonic and inhibitory *rôle* of the sympathetic ganglions, and their relation to vaso-motor nerves, by MM. Dastre and Morat.—The mode of fixation of the suckers of the leech studied by the graphic method, by M. Carlet. The movements of the animal on smoked paper were observed. It has been received that the oval sucker is attached first by the centre, then by the borders, but the author finds that the borders are fixed first. Detachment, too (which does not seem to have attracted attention), begins at the borders.—On a new fixed Crinoid, *Democrinus parfaiti*, obtained in dredging from the *Travailleur*, by M. Perrier. This makes only the fifteenth species known. It is distinguished by a long funnel-like cup, formed of five basal pieces.—Geological and chemical researches on the saliferous formations of the Swiss Alps, and especially on that of Bex, by M. Dieulafait. These beds the author regards as products of evaporation of ancient seas.

CONTENTS

PAGE
PROFESSOR HENRY SMITH. By Dr. W. SPOTTISWOODE, P.R.S. . . . 381
PUBLIC ELECTRIC LIGHTING 384
CRYPTOGAMIC FLORA OF GERMANY, AUSTRIA, AND SWITZERLAND. By Mrs. MARY P. MERRIFIELD 385
THE CHURCHMAN'S ALMANAC 386
LETTERS TO THE EDITOR:—
    Hovering of Birds.—THE DUKE OF ARGYLL, F.R.S.; HUBERT AIRY; HENRY CECIL 387
    The Auroral Meteoric Phenomena of November 17, 1882.—Dr. H. J. H. GRONEMAN 388
    The Orbit of the Great Comet of 1882—E. RISTORI 388
    Aino Ethnology.—A. H. KEANE 389
    Auroral Experiments in Finland.—S. LEMSTROM 389
    Flamingoes and Cariamas.—JAMES CURRIE 389
THE APPROACHING FISHERY EXHIBITION 389
THE PROGRESS OF TELEGRAPHY 390
CENTRAL AND WEST AFRICA (*With Illustrations*) 391
ON THE AURORA BOREALIS. By SOPHUS TROMHOLT 394
PROFESSOR HUXLEY ON EDUCATION 396
NOTES 398
OUR ASTRONOMICAL COLUMN:—
    The Great Comet of 1882 400
    Variable Stars 400
    A New Nebula 400
GEOGRAPHICAL NOTES 400
ON THE PRESENT CONDITION OF THE SODA INDUSTRY 401
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 402
SOCIETIES AND ACADEMIES 402

THURSDAY, MARCH 1, 1883

RECENT ARMOUR-PLATE EXPERIMENTS

AT the conclusion of their labours the "Iron Plate Committee" reported, in 1865, that the best material for the armour of war-ships was wrought iron of the softest and toughest nature. Steel, or steely iron, or combinations of iron and steel were all pronounced unsuitable for the purpose, after a long course of careful experiments. Accepting this verdict the designers of armoured ships continued to specify for soft iron armour, the makers of guns and projectiles aimed at the perforation of this kind of armour, and the manufacturers sought to secure the desired qualities of softness and toughness in the thicker and heavier plates which they were constantly being called upon to produce. All the armoured ships built from 1860 to 1876 were "ironclads," and in that time the thicknesses of armour plates carried on the sides or batteries of completed ships had advanced from 4½ inches to 14 inches, while the weights had risen from 4 or 5 tons to 20 or 25 tons. Greater aggregate thicknesses of iron had been arranged for prior to 1876. For example, the *Inflexible* had been designed to carry 24 inches of iron on her sides, but this was in two layers of 12-inch plates. The adoption of the so-called "sandwich-fashion" of armour plating was based upon experiments made at Shoeburyness, and it had certain advantages of a constructive character; it also enabled broader and longer plates to be produced within the fixed limits of weights with which the manufacturers could deal, and enabled them to insure excellence of quality which might not have been so certain of attainment in plates of 20 inches or upwards in thickness.

While the two great Sheffield firms and their rivals in France were thus developing the manufacture of iron armour plating, the Creusot Company, of which M. Schneider was the head, were attempting to reverse the verdict against steel armour, and to produce specimens which could hold their own against the best iron armour of equal thickness. The Italian Admiralty brought the claims of the rival materials to the test of experiment at Spezia in October, 1876. In order to decide on the kind of armour to be used on the *Duilio* and *Dandolo*, specimen targets were erected and a series of firing trials made against them on a scale of unprecedented magnitude. A gun weighing 100 tons, manufactured at Elswick, was brought to bear upon targets protected by iron or steel plates 22 inches thick, backed by great masses of timber and strong supports. Other guns of considerable weight and power were also used, but their performances were overshadowed by those of the monster weapon. The results of these trials may be briefly summarised. Against the 10-inch and 11-inch guns the 22-inch iron plates had a decided advantage over the steel plate of equal thickness. The penetration was somewhat greater in the iron plates, but the steel plate cracked badly. On the other hand, when the 100-ton gun was brought against the targets the iron plates and their backings were completely perforated as well as broken up: whereas the steel plate, although smashed to pieces, prevented the shot from passing through the backing. Various opinions were formed as to the deductions which should be made from these trials. On the one side it was urged that as steel plates of great thickness could be gradually cracked and destroyed by guns incapable of perforating them, steel ought not to be used instead of iron, which could be battered by a great number of projectiles from such guns, and be neither perforated nor cracked. On the other side it was maintained that there was small probability of any single armour plate on a ship's side being struck repeatedly in action; and consequently that the material should be preferred which could best resist perforation by a single projectile from the most powerful gun, even if the resistance to perforation involved the partial destruction of the plate struck. The Italian authorities adopted the latter view, and the *Duilio* and *Dandolo* have steel armour, being the first ships protected in that manner.

Although these steel armour plates were made in France, the French authorities did not follow the Italian lead and abandon iron armour. Nor was a similar course followed in England. Change was seen to be inevitable, but it was endeavoured to make the change in a direction which should combine the high resistance to perforation of steel with the power to resist cracking and disintegration possessed by tough rolled iron. To Messrs. Cammell and Co. of Sheffield belongs the honour of taking the lead in this direction; Messrs. Brown speedily followed, and the Admiralty gave substantial assistance in the conduct of the necessary experiments. In the earlier stages many failures and disappointments were experienced; but eventually better results were obtained, and "steel-faced armour" became recognised as the substitute for iron on English war-ships. Steel-faced armour, as the name implies, consists of a rolled iron back-plate, on the face of which is welded a layer of steel. The hard steel face resists perforation, and breaks up or deforms the projectiles, while the intimate union of the tough iron back with the hard steel face prevents the serious cracking which occurs in steel alone. Curiously enough the idea was not merely an old one, but a small plate made on this principle, 4½ inches thick, had been fired at in 1863. This early steel-faced plate was broken into two pieces at the first shot of a light gun, and was condemned by the Iron Plate Committee. Fourteen years later plates of a similar character, so far as the combination of steel and iron is concerned, but of improved manufacture, were successfully resisting three shots, either of which would have perforated an iron plate of equal thickness.

The first steel-faced plates were used on the *Inflexible's* turrets: they were 9 inches thick, worked "sandwich-fashion" outside 7-inch iron armour. It was part of the contract that a test-piece from each steel-faced plate should be fired at with a 12-ton gun, and should receive three shots without being broken up or perforated. This was considered to be a very severe test at the time, and undoubtedly was so when the novel conditions of the manufacture are considered. It was successfully met, however, and from that time onwards the manufacture has steadily improved. As an indication of what has been done, it may be stated that steel-faced plates 11 inches thick have received no less than eight shots from the 12-ton and 18-ton muzzle-loading guns, with battering charges and at 10 yards' range, without perforation or very serious cracking, this enormous "punishment" having been sustained by an area of 48 square feet only. Most of the trials made against steel-faced armour have been against plates from 10 to 12 inches in thickness. For thicknesses up to 12 inches it is probably within the truth to say that for *normal impact* the steel-faced plates of recent manufacture have been equal in their resistance to perforation to iron plates 25 to 30 per cent. thicker and heavier. For oblique impact the hard armour is probably still more superior to iron, glancing the projectiles at angles of obliquity when they would have "bitten" into the iron. A few experiments have been made in this country and abroad on much thicker steel-faced plates, ranging up to 18 or 19 inches in thickness, and of these the most recent and important are the trials made at Spezia in November, 1882. Three targets were constructed for these trials, the armour plate on each being nearly 11 feet long, 8½ feet wide, and 19 inches thick. One of the targets was covered by a steel-faced plate made by Messrs. Cammell, another by a steel-faced plate made by Messrs. Brown, and the third by a steel plate made at Creusot. All three plates were similarly backed and supported by 4 feet of oak; the Creusot plate was fastened by no less than 20 bolts, and the Sheffield plates had only 6 bolts each. Against these targets the 100-ton muzzle-loading gun was brought into action. At first the powder-charge used (329 lbs.) was that which gave such a velocity to the chilled cast-iron projectiles—2000 lbs. in weight—as would have perforated a 19-inch iron armour plate. The actual penetrations were from 3½ to 5 inches in the steel-faced plates, and 8¼ inches in the steel plate, showing that the actual superiority of all the plates over iron considerably exceeded the estimate. The steel plate did not crack at the first shot: the steel-faced plates did, but not to any serious extent. Next followed a more severe attack, the powder charge being increased to 480 lbs., giving the projectiles a velocity estimated to be capable of perforating about 24 inches of iron armour. The total energy of the projectile moving at this velocity exceeded 33,000 foot-tons. All the plates were broken into pieces by this terrific blow. The steel plate was split into six pieces, but the numerous bolts held these pieces in position, and still preserved the defensive power of the target. Each of the steel-faced plates was broken into five pieces; and on account of the fewness of the bolts these pieces fell to the ground, leaving the targets uncovered. The whole of the chilled cast-iron shots were broken up on impact, and the penetration into the steel-faced plates was less than that in the steel plate. At this stage the comparative tests ended. A third round was fired, with the heavier charge and a steel projectile against the steel plate. The shot was stopped, the penetration was only 7 inches, but the plate was broken up, and the backing seriously splintered. A fourth round was fired at this target and completely wrecked it.

On a review of all the circumstances of the experiments, it must be admitted that the greatest success was attained by the steel plate, although this must be attributed rather to the number and excellence of its fastenings than to superiority in quality of the plate over the steel-faced plates. The latter proved themselves less penetrable than the steel plate, and had rather the advantage as regards fracture at the end of the first two series of rounds; but they were insufficiently secured. One definite lesson to be learned from these experiments is, therefore, that a larger number of bolts is needed for a given area of steel or steel-faced armour than has been commonly used. Another lesson taught by these trials is that the steel armour plates of Creusot manufacture in 1882 are far superior to those made six years earlier. It is not at all probable that light guns such as broke the 22-inch steel plate to pieces in 1876 would have been equally effective against the 19-inch plate recently tested. In both cases the plates were made specially for the firing-tests, and they may not have been "merchantable articles" in the sense of representing large quantities of steel armour. But nevertheless this 19-inch plate shows what can be done with steel, if cost is of secondary importance. Authoritative statements are wanting of the actual processes of manufacture, or of the cost of production. It is reported that the 19-inch plate was hammered down from an ingot three or four times as thick as the finished plate, and that the face was oil tempered. If this is correct the cost must be high, and probably as great as, if not greater than, that of steel-faced plates. Moreover if such an amount of "work" has to be put into steel plates in their conversion from ingots into the finished forms, then no great economy or advantage can result from the power which the maker has to cast steel ingots in special shapes or sectional forms. The Creusot Company use a soft steel containing perhaps three-tenths to four-tenths per cent. of carbon, give it toughness by means of a large amount of hammering, and harden the face by oil tempering. On the contrary, the Sheffield firms, as the result of numerous experiments, use a hard steel for the face, the percentage of carbon amounting to about twice that in the Creusot plate, and support this by a tough iron back. With this hard steel, oil tempering does not appear to be beneficial, although with softer steel it undoubtedly is an advantage. These steel-faced plates which were tested at Spezia were really samples of large quantities made at Sheffield in the same manner. Probably equally good results would have been obtained if any one of the batch of plates represented had been selected for test. In this respect, therefore, there is a marked difference between the test to which the two manufactures were subjected.

As between the steel and steel-faced plates tried at Spezia we may assume that there is no notable difference in resistance to perforation or to fracture. Possibly, with equally good and equally numerous fastenings the steel-faced plates would have had some slight advantage, and in other trials mentioned later on steel-faced plates have had a decided advantage. Supposing no important difference to exist, then the choice between the two kinds of armour will be governed by their relative prices; and how these compare, we have no means of judging, but it seems probable that the steel-faced plates would be at least as cheap as steel plates made in the manner described above for the steel test-plate.

It may be convenient in this connection to briefly describe the mode of manufacture of steel-faced plates. Messrs. Cammell prefer to pour the molten steel on to the face of a wrought-iron plate which has been brought to a good welding heat. The layer of molten steel is surrounded by a frame of wrought-iron which has previously been attached to the iron plate; and it is pressed against the surface of the iron plate by a cover carried by an hydraulic ram, until the welding is complete and the steel has solidified. Messrs. Brown prefer first to roll a steel face-plate as well as an iron back-plate, and then to raise both to a welding heat; the molten steel is afterwards poured into a space left between the two, and hydraulic pressure is applied until the solidification has taken place. The remaining processes are similar in the practice of both firms. After welding has been completed, the whole mass is reheated and rolled down to the finished thickness of the armour plate. The steel face is usually about one-half the thickness of the iron back, and it is a curious fact that the iron and steel maintain their relative thicknesses as the rolling proceeds, even when the reduction in thickness during rolling is very considerable. This reduction varies from one-half for thin armour-plates, up to 10 or 11 inches in finished thickness, to one-third with 18 to 20 inches of finished thickness. Some competent authorities consider that too little work is done in the rolls on the thicker plates, but there is a need for further experiment to show whether this view is correct. Whatever may be the cause, it would seem that the best results so far have been obtained with steel-faced plates below 12 inches in thickness.

Simultaneously with the Spezia experiments another competition was proceeding, near St. Petersburg, between steel-faced and steel armour. The plates tested were 12 inches thick, 8 feet long, and 7 feet wide. They were first fired at with the 11-inch breech-loading gun, throwing a 550-lb. chilled cast-iron projectile, with a powder charge of 132 lbs. The velocity of the shot was 1500 feet per second. Messrs. Schneider supplied the steel plate, which was fastened with twelve bolts. Messrs. Cammell made the steel-faced plate, which had only four bolts in it. The first blow on the steel plate broke it into five pieces; the projectile was destroyed, but it penetrated 13 inches into the target. A blow of equal energy on the steel-faced plate produced only a few unimportant cracks in the steel, and the penetration was about 5 inches only. Three out of the four bolts were, however, broken. A second shot was then fired at each plate with 81 lbs. charge. The steel plate was broken into nine pieces, and the penetration was 16 inches: whereas on the steel-faced plate the principal effect produced was to break the only remaining bolt and to let the plate fall to the ground, face downwards. The back of this plate was perfect, and the target behind the plate was uninjured. In this trial the steel-faced plate proved greatly superior to the steel, but had insufficient fastenings. It is proposed to increase the bolts in number, re-erect the plate, and continue the trial, of which the further results cannot fail to be interesting.

This contest between steel and steel-faced armour must not be allowed to withdraw attention from the great superiority of both, in certain respects, to iron armour. Even as matters stand, either of these modern defences is greatly to be preferred to their predecessor. Against this hard armour chilled cast-iron projectiles break up in a manner never seen with soft iron. Projectiles of this kind are virtually impotent, and must be replaced by more expensive, harder projectiles, if steel or steel-faced armour is to be attacked. Even with steel projectiles results cannot be obtained such as were possible with iron armour. Perforation of armour by shells carrying relatively large bursting charges is no longer a possibility: and the heaviest gun yet made cannot drive its projectiles through a thickness of hard armour only three-fourths as great as the thickness of iron which it it could perforate.

The use of steel and steel-faced armour will involve many experiments to determine not merely what descriptions of projectiles are best adapted to damage or penetrate it, but what are the laws of the resistance of such armour to penetration and disintegration. All the formulæ based on experiments with soft iron armour and chilled cast-iron projectiles are inapplicable under the new conditions. Perforation is no longer to be feared as the most serious damage likely to happen to armour plates: more moderate thicknesses of hard armour suffice to stop the projectiles from the heaviest guns than would have been considered possible a short time ago. Instead of perforating 19 inches of steel or steel-faced armour, the projectile of the 100-ton gun with a given velocity only penetrates 8 inches into the plates. But, on the other hand, the possible disintegration and fracture of the armour plates are becoming important matters. Makers of armour plates have to endeavour to produce materials which shall resist fracture as well as penetration, and the only proof of their success or failure is to be found in the results of actual trials. Experiments are equally essential to progress in the manufacture of guns and projectiles. The example set by Italy must be followed; the necessary experiments must be on a large and costly scale, and they may lead to many departures from former practice. But if real progress is to be made in the armour and armament of ships, it must be prefaced by experiments beside which those of the former Iron Plate Committee will appear insignificant.

In conclusion it may be stated that although iron armour has been practically superseded for the sides and batteries of war ships, it is still preferred for decks. Experiments have shown that for angles of incidence below 20 degrees, and for such thicknesses—not exceeding 3 or 4 inches—as are used on decks, good wrought-iron is superior to both steel and steel-faced plating. The explanation of this departure from the laws which hold good for thicker plates and greater angles of incidence cannot be given here, but the fact has been established by elaborate trials made in this country and abroad.

SMOKE ABATEMENT

Report of the Committee of the Smoke Abatement Exhibition. (London: Smith & Elder, 1883.)

THIS volume, which has just been issued, presents many points of interest, as it is the outcome of the labours of a Committee formed in 1881 with a view to ascertain what means could be adopted to check the growing evils arising from the evolution of smoke which attends the combustion of bituminous coal. It may be said to be the continuation of work undertaken by the several Parliamentary Committees which met in 1819, 1843, and in 1845. In the previous efforts attention appears to have been mainly directed to lessening the nuisance arising from smoke from factory and other furnaces, but in the present movement it is evident that the importance of the domestic fireplace as a foe, if not the chief one, to the purity of the air of cities, has been generally recognised and has been the main object of attack.

It is not a little remarkable that, although elaborate experiments have been made from time to time with a view to ascertain the nature and composition of the gases generated in furnaces, but little attention has been devoted to the gases given off from stoves and grates. On the Committee of the recent Smoke Abatement Exhibition chemists were well represented, and this brief notice will mainly refer to the general chemical results that have been obtained.

The examination of the gases withdrawn from flues to which stoves and grates were attached, was intrusted to Prof. Chandler Roberts, who at first considered that the analysis of representative samples might best be made by the aid of the rapid methods of gas analysis arranged by Orsat. In view, however, of the peculiar conditions under which the tests had to be made, and bearing in mind that more than one hundred appliances were submitted for testing in the limited time during which the Exhibition was open, Prof. Roberts submitted a plan to the Committee which received its approval.

He points out in his report that the first researches on chimney gases are due to Péclet, who published some results of analysis in 1828, but Péclet's results and those of different experimenters who followed him were open to the objection that the samples submitted to analysis were only small fractions of the total gases in the flues, and as the samples were not taken with sufficient frequency they could not represent the mean composition of the gaseous mixture passing up the chimney. This grave defect was, however, remedied by Scheurer-Kestner in an elaborate research on the composition of the flue-gases of boiler furnaces, which will always be the basis of future experiments in this direction, and to which frequent allusion is made in the Report. The details of the method adopted are given in the Report itself; it will be sufficient to say here that the gases were withdrawn through a fine slit in a tube extending across the flue, an arrangement which rendered it possible to draw the gases uniformly from the entire diameter of the ascending current of gas in the flues. The effluent gases were withdrawn by aspiration through a tube loosely filled with asbestos to retain the solid particles of carbon and soot; they then passed through a U-tube filled with chloride of calcium to absorb water, and thence through three U-tubes filled with soda-lime to absorb carbonic anhydride; the gases were then led to a tube of porcelain filled with cupric oxide and heated to redness by means of a small furnace. The complete combustion of the remaining gases was thus effected, the carbonic oxide being burnt to carbonic anhydride, and the hydrocarbons and free hydrogen to aqueous vapour and carbonic anhydride; the water was retained in a U-tube filled with chloride of calcium, and the carbonic anhydride in two other soda-lime tubes; the residual gases (unconsumed oxygen and nitrogen) then passed to the aspirator, a chloride of calcium tube being interposed to prevent any moisture from the aspirator from penetrating the system of tubes.

It will be evident that this plan renders it possible to compare the relative proportion of the completely burnt products of combustion with those in which combustion has been imperfect. With regard to the proportion of carbon lost as soot, the evidence, afforded by the results of the tests made at the Exhibition, although they do not unfortunately render it possible to give a clear and precise answer to the question, are sufficiently definite to show that the amount probably does not exceed 1 per cent. of the total carbon in the fuel, and is in many cases far less.

The coal used in testing the grates and stoves was either 'Wallsend,' which yielded 67·1 per cent. of coke, or Anthracite, giving 94 per cent. on distillation in a closed vessel.

With regard to the completeness of the combustion, the carbon present in the form of carbonic anhydride varied in relation to that present as carbonic oxide and as hydrocarbons, C_xH_y, within the limits of 1,000 to 4 and 1,000 to 375, but of the whole eighty-six tests in only three was the number indicating imperfect combustion below 10, and in only nine cases was it above 200, and six of these nine cases (three grates and three stoves) were worked purposely for "slow combustion."

The total amount of carbon present in the gases ascending the flue (either in the free state or combined with carbon) bore a relation to the hydrogen present which varied between the limits of 1,000 to 8 and 1,000 to 259, the latter probably being due to the fact that the grates and stoves were tested whilst the mortar in which they were set was still wet.

The mean of the results of the tests of the seventeen best grates shows that the loss of carbon in the form of carbonic anhydride and hydrocarbons is about 3·4 per cent. of the carbon in the fuel used (in the case both of Anthracite and Wallsend), the mean for the whole of the grates being about 9 per cent. of the total carbon.

The comparative imperfection of the combustion shown in some of the tests is hardly to be wondered at when it is remembered that the bituminous coal employed yielded on distillation no less than 32 per cent. of volatile matter, and that in the case of many of the appliances the cold fuel was simply charged on to the top of a mass of coal already in the state of incandescence.

Professor Roberts cautiously points out that all that has hitherto been done in this series of tests "merely renders it possible to select certain typical appliances which deserve more detailed examination." He appears, however, to have spared no pains to render this very laborious investigation as complete as the circumstances allowed, and the Chemical section of this Report is certainly one of the most important contributions ever made to our knowledge of the combustion of fuel.

E. FRANKLAND

NORTH AFRICAN ETHNOLOGY

Sahara und Sudan: Ergebnisse Sechsjahriger Reisen in Afrika. Von Dr. Gustav Nachtigal. Part II. (Berlin: 1881.)

NEARLY a decade has elapsed since Dr. Nachtigal's return to Europe after his travels in East Sahara and Central Sudan during the years 1869-74. Most of the geographical and ethnological results of his researches in that region have already appeared at various times in the memoirs of the *Gesellschaft für Erdkunde* and of other learned societies. But the issue of the monumental work embodying all the details in a permanent form is proceeding at a very slow rate. The first part, covering the years 1869-70, did not appear till 1879, and an interval of two years elapsed before the publication of this second instalment, which, although forming a bulky volume of 790 pages, gets no further than the first days of September, 1872. In the preface the delay is attributed mainly to the time occupied in the tedious process of sifting the ethnological and especially the linguistic materials brought home by the traveller. The help afforded by Rudolf Prietze in arranging these materials is handsomely acknowledged in the preface, where occasion is also taken to express regret for omitting to give the source of the familiar mailclad, mounted Bornu warrior borrowed in Part I. from Denham and Clapperton's work, attention to which oversight had been called in our review of that volume (see NATURE, vol. xxi. p. 198).

The three books forming the present volume embrace the trips made to Kanem and Borku north of, to Baghirmi south of, Lake Chad, and to the islands in the lake itself. Separate chapters of great permanent value are devoted to the main geographical features of these regions, and to the history and complex ethnical relations of their inhabitants. Here special attention is naturally claimed by the mysterious Tubu people of the East Sahara, and a serious attempt is made to explain their relations on the one hand to the Hamitic Tuariks (Berbers) of the West Sahara, on the other to the Negro races of Sudan.

The Tubu, that is, "people of Tu" or Tibesti,[1] are by Lepsius[2] with great probability identified with the Garamantes of Herodotus (iv. 183), whose capital was Garama (Edrisi's Germa) in Phazania (Fezzan). Ptolemy, who places the Garamantes in the same region, that is, in the Libyan Desert (Sahara) south of the Syrtis Major (Gulf of Sidra), already speaks doubtfully of their ethnical affinities, and seems disposed to affiliate them rather to the Ethiopian (Negro) stock.[3] Later on this position is disturbed by Leo Africanus, whose fifth great division of the Berbers are the Gumeri (Garamantes?), whom he elsewhere calls Bardæi (Bardoa). These Bardæi, whose name appears to survive in the *Bardai* oasis of Tibesti, are accordingly identified by Vater with the Tubu, and by him grouped with the Berbers.[4] Now comes Lepsius, who again removes the Tubu from the Libyan (Berber) connection, and with Ptolemy transfers them to the Negro group. He admits a strong modification of the original dark, and a corresponding assimilation to the Libyan, type. But this is attributed to their position along the great historical trade route across the Sahara between North Africa and the Chad basin, while their language is regarded as decisive proof of their Negro relationship.[5]

And thus this interesting, if somewhat troublesome, nomad race has continued throughout the historic period to occupy a dubious ethnological position between the surrounding Hamitic and Negro peoples. That Dr. Nachtigal should attempt to grapple with the problem was inevitable, and although his own inferences are vague and hesitating, he at all events supplies ample material for a satisfactory solution. As in so many other anthropological fields, the difficulty turns, so to say, mainly on the collision between ethnical and philological interests. The present physical resemblance of the Tubu to their western neighbours, the Berber Tuariks, admits of no doubt, and this resemblance increases as we proceed from the Dasa, or southern, to the Teda,[1] or northern, division of the race. In fact there is here the same gradual transition between the Hamites of the Sahara and the Negroes of Sudan, which is found further west all along the borderlands from Bornu to the Atlantic, and which is conspicuous especially in the more or less mixed Sonrhay, Pul, Hausa, and Toucouleur nations of the Chad, Niger, and Senegal basins. To these correspond in Central Sudan the Negroid Kanuri, Kanembu, Baele, and Zoghawa peoples,[2] while the same complexity is presented in the Nilotic regions, where the Nuba family merges imperceptibly north and south in the Egyptians and Negroes of the White Nile.

But Lepsius (*op. cit.*) now holds that the two elements have become interpenetrated throughout the whole of the Sudan, which he consequently regards as an intermediate zone of transition between the intruding Hamites from Asia and the autochthonous Negroes, whose original domain is relegated to the southern half of the continent. In this scheme, which thus recognises in Africa only two fundamental racial and linguistic types, what place can be assigned to the Tubu? We have seen that Lepsius himself disposes of the question by regarding them as originally Negroes assimilated physically to the Berbers, while retaining their primitive Negro speech. If this view could be accepted, we should have an instance of the linguistic surviving the ethnical type, a theory in which anthropologists would in any case be slow to acquiesce. But Dr. Nachtigal's researches, while confirming Barth and Koelle's conclusions regarding the intimate relation of Tubu to Kanem and Kanuri, also show that this group is fundamentally distinct from the Bantu, that is, from the typical Negro linguistic stock of Lepsius. If it could be affiliated to the Hamitic family, there would be no further difficulty as to the ethnical position of Tubu. But Nachtigal also shows that it differs quite as radically from Hamitic as it does from Bantu. His inquiries have in fact resulted in the discovery of an independent and widespread linguistic family corresponding in the East Sahara and Central Sudan to the northern Hamitic and southern Bantu groups. The source, or at least the most archaic known form, of this family is the Teda, or northern Tubu, whose direct offshoots are the more highly developed Dasa, or southern Tubu, the Kanem north of Lake Chad, the Kanuri of Bornu, the Baele of Ennedi and Wanyanga, and the Zoghawa of North Dar-Fur. More distant members appear to be the Hausa, Fulu, and Sonrhay of

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[1] Cf. *Kanem-bu* = people of Kanem, where *bu* is the pl of the personal postfix *ma*, answering to the personal prefix *m*, pl *ba*, *wa*, cf the Bantu races, as in M'Ganda, Waganda; and to the *be* of *Ful-be* = "Pul people."

[2] *Nubische Grammatik*, *Einleitung*.

[3] Οὔτων δὲ καὶ αὐτῶν ἤδη μᾶλλον Αἰθιόπων, i. 8.

[4] "Mithridates II," p. 45 of Berlin ed. 1812

[5] "Ursprünglich ein Negervolk," *op cit.*, *Einleitung*, xlviii.

[1] In this word *Teda* we have apparently the root of the *Tedamensii*, a branch of the Garamantes placed by Ptolemy south of the Samamycii in Tripolitana If my identification is correct, it gives us a fresh proof of the identity of the Garamantes with the Tubu

[2] See my paper "On the Races and Tribes of the Chad Basin," NATURE vol xv p. 550.

West Sudan, the Logon, Bagrimma (Baghirmi), and Mandara (Wandela) of the Shary basin, and the Maba of Wadai. But the actual relationship of these and other outlying branches to the main trunk can only be determined by future research. Meantime, Dr. Nachtigal rests satisfied with having demonstrated the existence of this widely-ramifying family and its radical difference both from the Hamitic and Bantu groups. "How far the relationship may extend will be made more and more evident by a further study of the Sudan languages, especially the Hausa, Masa, Bagrimma, Maba. For the present it is enough for me to have established the relations of the Tubu dialects to each other, and of both to the Kanuri and Baele" (p. 209).

But when he comes to the consequences of his premisses he speaks with singular hesitation, as if overweighted or hampered by the brilliant generalisations of Lepsius. The fact is, this theory of the three zones leaves no room in Africa for the great linguistic family which Nachtigal has nevertheless discovered there. But instead of boldly giving up the theory, he timidly suggests alternatives, in order somehow to reconcile it with the actual conditions. After clearly showing the independent position of Tubu, he leaves the reader to choose between a possible "extremely remote connection with the Negro languages, or, if it be preferred, to regard it as a distinct species, which has held its ground between the Negro and Hamitic linguistic types" (p. 201). Most ethnologists will probably be prepared to accept the latter alternative, even at the risk of adding one more to the two "linguistic types" which alone Lepsius will tolerate. The only point of contact between Tubu and Bantu seems to be the absence of grammatical gender, a negative feature which both share with a thousand other languages in the Old and New Worlds. Yet apparently in order to save Lepsius's scheme, Nachtigal is content on this weak ground to allow a connection between Teda and Negro, adding, still more inconsequently, "in which case, considering the vagueness of the concept 'Negro' (bei der Unbestimmtheit des Begriffes 'Negro'), there can certainly be no objection to group the Tubu themselves with the Negroes, although, taking the word in its ordinary sense, in other respects they essentially differ from them" (p. 209). So the Negro—that is, the most marked of all human varieties—is frittered away to a "vague concept," because Tubu is a no-gender language, or because Lepsius will allow only two linguistic types in Africa.

But by getting rid of this theory, an easier exploit than getting rid of the Negro, everything will fall into its place. The consideration that the centre of evolution of the Tubu group lies, not in Sudan, but in the Sahara, far north of the original Negro domain, placed by Lepsius south of the equator, would almost alone suffice to separate it from that connection. Dr. Nachtigal himself shows that Teda, or northern Tubu, represents the germ, of which the southern Dasa, Kanuri, Baele, &c., are later developments. He also shows that the migrations, as was natural to expect, were always from the arid plains and uplands of the Sahara to the fertile region of Sudan. Except under the lash of the slave-driver, the Blacks seem never to have moved northwards. But we have seen that the roving nomads, Tuariks in the west, Tubu in the east, have everywhere, all along the line, penetrated from their desert homes into the "Black Zone." The inference seems obvious. Nachtigal himself regards the Tubu as "a thoroughly pure, homogeneous people (*eine durchaus reine, homogene Bevölkerung*), unmodified by any changes from remote times" (p. 190). He also shows their close physical resemblance to the Tuariks (Berbers) of the western Sahara, and their essential difference from the Negro type. The anthropologist will not hesitate to remove them from the latter and group them with the former race. The Tubu and Berbers are thus ethnically two slightly differentiated branches of the Hamitic section of the great Mediterranean (Caucasic) division of mankind.

From this standpoint the Tubu speech, although as radically distinct from the Hamitic as it is from the Bantu, will no longer present any difficulty. In Europe the Mediterannean races have developed at least one radical form of speech, the Basque; in Asia several, the Aryan, Semitic, Georgian, and others in Caucasia. Why should they not have developed two in Africa, the Hamitic and Tubu? Elsewhere I have endeavoured to account for this remarkable phenomenon of specific diversity of speech within the same ethnical group.[1] Here it will suffice to note the fact, and if the no-gender character of Tubu be urged as a difficulty, the reply is twofold. First, no-gender languages occur also in other Caucasic groups, as in Basque, Georgian, Lesghian; secondly, although gender has not been developed in Tubu, nevertheless it contains the raw material, so to say, which has been elaborated into a system by the more cultured Hamitic peoples. After admitting that, but for the absence of this feature, there would be no scruple (*Bedenken*) in affiliating Tubu to the Hamitic order, Dr. Nachtigal adds: "Tubu also certainly seems to possess the elements by which gender is indicated in the Hamitic—*o* and *p* for the masculine, *t* for the feminine, as in *o-mri*, man; *mi*, son, by the side of *âdi*, woman; *dô*, daughter; *dê*, mother; *edī*, female" (p. 200). Here *d* of course answers to *t*, the universal mark of the feminine gender in Hamitic, and in the Berber group often both prefixed and postfixed, as in *akli*, negro; *taklit*, negress.

Room must therefore be made in Lepsius's scheme for a third linguistic family, the honour of having determined which belongs to Dr. Nachtigal. This Tubu family must be assumed to have been independently evolved in remote ages by the Garamantes, ancestors of the Tubu nomads, during long isolation in Kafara, Kawár, Tibesti, and the other oases of the eastern Sahara and Fezzan. Lastly, the Tubu themselves must be absolutely separated from the Negro ethnical connection, and grouped with the Hamites in the Mediterranean division of mankind

A. H. KEANE

OUR BOOK SHELF

The Electric Lighting Act, 1882, *the Acts incorporated therewith, the Board of Trade Rules, together with numerous Notes and Cases.* By Clement Higgins, M.A., Recorder of Birkenhead, and E. W. W. Edwards, B.A., Barrister-at Law. (London: W. Clowes and Sons, 1883.)

PRACTICAL electricians unversed in law, and lawyers unversed in the practical applications of electricity, will find much useful matter in this volume. The authors are thoroughly competent to deal with the legal aspect of the case, whilst their judicious comments show that they appreciate at least many of the technical difficulties necessarily presented by the subject. The contents deal with the various sections of the Electric Lighting Act, adding copious notes and comments, and references to legal precedents and decisions. Quotations are given from the evidence collected by the Select Committee on Electric Lighting, and from the Rules and Regulations recommended by the Society of Telegraph Engineers and Electricians concerning the prevention of fire-risks. One or two minor slips in the science are to be regretted, as for example where the authors state that a current of unit strength will decompose ·09378 grammes of water per second. It is a pity, moreover, that they have departed from customary usage in speaking of the "strength" of a current as its "intensity." That term has been and is still so much abused, that so long as it is liable to mislead its use should be avoided. One of the authors describes himself as "Fellow of the Physical Society of London." We were not aware that the Physical Society of London recognised any such grade amongst its members.

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[1] See Appendix to my "Asia" (Stanford Series), p. 695.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Ben Nevis Observatory

IN NATURE, vol. xxvii. p. 399, there is among its notes of scientific intelligence, a paragraph mentioning that at a public meeting in Glasgow last week, called at the suggestion of Sir William Thomson and Mr. John Burns of Castle Wemyss, it was agreed to collect money for a permanent observatory on Ben Nevis.

As NATURE has always kindly encouraged this project of the Scotch Meteorological Society, perhaps you will permit me, as Chairman of the Society's Council, to add a little to this brief notice.

A requisition was presented to the Lord Provost of Glasgow, which was signed, not only by the eminent physicist and the extensive ship-owner mentioned in your notice, but also by Dr. Grant of the Glasgow Observatory, suggesting that a meeting of the merchants and ship-owners should be called to aid the Society in raising the necessary funds.

The Lord Provost in compliance called a meeting for the the 14th inst., which was well attended, and at which very able speeches were made, not only by the three requisitionists, but by the Lord Provost and by other influential citizens. The result of the meeting was a resolution expressing approval of the Society's proposal, and appointing a Committee to obtain subscriptions in aid of it.

It is expected that the amount of the funds required will be obtained from a community so wealthy and so public-spirited as that of Glasgow. But if we are mistaken in this, the Society's Council intend to appeal to other communities also for help, being resolved to resort to every legitimate means of attaining an object allowed on all hands to be of national importance.

The Council began with Glasgow, not only because it is the richest community in Scotland, but because the Scotch Meteorological Society *originated* there. The late Sir John G. Forbes of Pitsligo, and I, being both of us interested in meteorology, applied to the British Association for the Advancement of Science, when it met in Glasgow in September, 1855, under the presidency of the Duke of Argyll, to see whether it would approve of the formation of a Meteorological Society for Scotland. The result of our application was the following resolution by the General Council:—

"*Resolved, that the British Association express their satisfaction at the proposed establishment of a Scotch Meteorological Society, and their willingness to afford the Society any assistance which can be yielded by the establishment of the Association at Kew.*

"*That a letter to this effect be addressed to the Meteorological Society by the General Secretary.*"

On the basis of this testimonial by so influential a body, Sir John Forbes and I proceeded at once with the organisation of a Society, the Duke of Argyll being our first President, and assisting us greatly by his patronage.

When the Society resolved on attempting the formidable undertaking of establishing an observatory on Ben Nevis, at a cost of at least 5000*l*., the first movement for funds was made among its own members and friends, the result of which was a promise of 1400*l*. provided the full sum of 5000*l*. was raised. In order to be enabled to fulfil this condition, the Society's Council not unnaturally went first to the town where it originated, and which more than any other town would be supposed to take an interest in the Society and its operations.

There was this further reason: that the Observatory being intended to be on the west coast, its proximity to Glasgow would add to that interest, and the more so as, on account of the vast shipping and commerce of the Firth of Clyde, no district of Scotland could be so deeply concerned in obtaining additional data for storm warnings.

The British Association, by way of encouraging the formation of the Meteorological Society, expressed in the resolution before quoted a willingness to afford to it assistance from its establishment at Kew.

This promise, unfortunately, the Association was unable to fulfil. But this disappointment to our Society has now been so far compensated by a handsome donation of 100*l*. towards the Ben Nevis fund from Dr. Siemens, the present President of the Association.

The Scotch Meteorological Society is one out of many proofs of the usefulness of the British Association in encouraging researches in particular branches of science, and the recent recognition of the Society's work in this Ben Nevis enterprise by so eminent a man as the present President of the Association is very gratifying to the Council. DAVID MILNE HOME

Milne Graden, Coldstream, February 26

Indian Archegosaurus

THE skull and part of the vertebral column of a large labyrinthodont, allied to *Archegosaurus*, was obtained in 1864 from the Bijori-group of the trias-jura of India, and presented to the Asiatic Society of Bengal. It was soon after sent to England for determination. All traces of this unique and important specimen, which should now belong to the Government of India, are now lost, and I write in the hope that some of your readers may be able to afford us a clue to its present position. The specimen can hardly have been mislaid, as it is some two feet in length. RICHARD LYDEKKER

The Lodge, Harpenden, Herts, February 21

The "Vampire Bat"

KINDLY permit me to ask for a further explanation from Mr. Geo. J. Romanes about the vampire bat, in regard to which he says in his criticism of "Zoological Sketches" (Oswald): "Mr. Bates says (I presume it is a clerical error giving Mr. Belt as the authority) the vampire, however, is the most harmless of all bats." Yet he, Mr. Bates, would lead us to believe that a species of the same genus, Phyllostoma, is a blood-sucker, and had even attacked himself (see p. 91 of the fifth edition of his "Naturalist on the Amazon").

Is there a species of Phyllostoma that lives on fruits, the vampire, and another species of the same genus that Mr. Bates calls "the little grey blood-sucking Phyllostoma," that may possibly attack human beings?

The late Chas. Waterton seems to have had no doubt that the vampire attacks persons asleep, and gives an instance.

The common name vampire may ~~not~~ be in South America confined to the species *Phyllostoma spectrum*. Mr. Romanes' remarks would lead one to believe that he considered there was no species of bat that attacked human beings.

THOS. WORKMAN.

4, Bedford Street, Belfast, February 15

DR. ROMANES, in criticising a book ("Zoological Sketches"), in NATURE, vol. xxvii. p. 333, says: "The writer speaks of vampire bats as those which suck the blood of sleeping persons; whereas the truth is, as Belt has remarked, 'the vampire is the most harmless of bats.'"

In Charles Darwin's "Voyage of the *Beagle*," we find an account of a vampire bat (*Desmodus d'orbignyi*) sucking the withers of horses during repose. We also have Charles Waterton's most circumstantial account of the sucking of the blood of human sleepers. Waterton says there are two species, only one of which attacks man. The Rev. J. G. Wood tells us in his notes to "Waterton's Wanderings" that the bat is *Vampirus spectrum*, on what authority he does not say, but quotes C. Kingsley in confirmation of the blood-sucking habit. Again, Prof. Mivart has an article in the *Popular Science Review* for July, 1876, on bats, in which he not only quotes Darwin's account, but speaks of the modification of the teeth and stomach of *Desmodus* as specially suited to this habit. What I wish to ask in all humility, as a mere onlooker, is, How are we to reconcile the above statement with all this authority?

94, Jacob Street, Liverpool, February 12. A. W. AUDEN

I INADVERTENTLY wrote the name of Belt while quoting from the work of Bates. The answer to the question which your correspondents ask is sufficiently simple, and has, in fact, been furnished by one of them, viz., that while the vampire bat itself does not suck blood, the name is popularly extended to other kinds of bats which do. These other kinds—or at any rate some of them—belong indeed to the same sub-family as the vampire (viz., genera *Phyllostoma* and *Desmodus*); but that the large and repulsive-looking vampire is innocent of the habit in question may briefly be made evident by citing again, and a little more fully, the authority of Mr. Bates, who writes: "The vampire was here by far the most abundant of the family of leaf-nosed bats. . . . . No wonder that imaginative people have inferred diabolical instincts on the part of so ugly an animal. The vampire, however, is the most harmless of bats, and its inoffensive character is well known to residents on the banks of the Amazons" ("Naturalist on the Amazon," p. 337). Again, Mr. G. E. Dobson writes: "This species (*Vampirus spectrum*), believed by the older naturalists to be thoroughly sanguivorous in its habits, and named accordingly by Geoffroy, has been shown by the observations of modern travellers to be mainly frugivorous, and is considered by the inhabitants of the countries in which it is found perfectly harmless" ("Catalogue of the Chiroptera, &c." p. 471).

In conclusion, I cannot quite understand why my remarks should have led any one to believe, as one of your correspondents says, that I consider there is no species of bat which attacks human beings. I stated that the author whom I was reviewing was wrong in speaking "of vampire bats as those which suck the blood of sleeping persons," a statement which appears to me plainly enough to imply that there are certain other bats which do suck the blood of sleeping persons.

GEORGE J. ROMANES

Hovering (? Poising) of Birds

LET me entreat the Duke of Argyll not to confuse the issue between us. I made bold to ask his Grace to draw a diagram showing by what balance of forces he thought a bird could be sustained in mid-air, motionless on motionless wings, in a perfectly horizontal wind; and he refers me to a beautiful drawing of a kestrel hovering, *with fluttering wings, in still air*. (See note at foot of page 161 of the "Reign of Law," 5th edition, 1868: "Mr. Wolf's illustration of a kestrel hovering shows accurately the position of the bird when the action is performed in still air.")

This is quite beside the mark. The problem to be solved is not, How does a bird remain at rest in mid-air on fluttering wings? That question is admirably answered in the "Reign of Law" (p. 160). But the problem before us—the same that was discussed in NATURE in 1873-74—is simply this, How does a bird remain at rest in mid-air on perfectly motionless wings?

Does the Duke deny that this ever takes place? Has he forgotten the letters of Prof. Guthrie and Major Herschel (NATURE, vol. viii. pp. 86 and 324) in which the phenomenon was so graphically described? The Duke himself says (NATURE, vol. x. p. 262), "that under certain conditions of strength of air-current a kestrel can maintain the hovering position with no visible muscular motion whatever;" and compares the action to that of a rope-dancer "standing still in some tiptoe attitude." At that time he appears to have recognised the peculiar features of motionless hovering; but now he denies that he has ever "seen a kestrel's wings motionless when hovering," except for a moment or two, and even then he "could detect the quivering of the quills."

I am really at a loss to know whether the Duke maintains his former position; or whether by shifting his ground he admits that it is untenable; or, lastly, whether he has not partly misapprehended the problem under discussion.

In instancing the "hovering of a boy's kite" the Duke curiously parodies the mistake which he made in his last letter, which required for its correction the tilting of gravity through a certain angle. So here, when he says, "the element of weight is here represented by the string, held at the surface of the ground," he forgets the all-important angle between the direction of gravity and the direction of the string at its point of attachment to the kite.

HUBERT AIRY

February 26

HAVING all my life given some attention to the flight of birds, I may mention that I have frequently noticed both hawks and gulls stationary in the air, without flapping, for five or six seconds over the Cornish cliffs when the wind has been blowing off the sea, but never under the circumstances mentioned by Dr. Rae. I totally fail to see why Mr. Airy should be, as the Duke of Argyll states (NATURE, vol. xxvii. p. 387), "mistaken in his description of the facts," it having been plain throughout that Mr. Airy employs the term "hovering" as equivalent to "hanging in motionless poise." Mr. Wolf's kestrel in the "Reign of Law," p. 160, is shown as moving its wings through an angle of about 30°.

Although I believe there is nothing in the etymology of the word "hover" which implies movement, yet its similarity to such words as "quiver," "shiver," &c., may have caused the idea of movement to be associated with it; but whether this be a "disease of language" or not, Mr. Airy seems to have most accurately described what is surely not an uncommon fact of observation.

W. CLEMENT LEY

The Auroral "Meteoric Phenomena" of November 17, 1882

IF Dr. Groneman has established the fact that the spindle-shaped beam from every point of observation appeared moving in a straight line, that is an important point gained; but I fail to gather from his letter on p. 388 that there is clear evidence of this. He cites S. H. Saxby as one observer in favour of this, but his description appears to me very ambiguous. When he says, "Its trajectory was much flatter than that of the stars," what stars does he mean? If he means the stars at the same declination as that of the beam, viz. about 10° S., then a great circle undoubtedly would be flatter, but still more would a small circle having its centre at the magnetic pole. On the other hand, H. D. Taylor writing from near York describes the path of the beam as from south-east to south-west, thus making it a small circle curved in the wrong direction for an auroral arch.

It must be remembered that it is very difficult to judge whether a trajectory is a straight line when it covers a great extent in azimuth.

T. W. BACKHOUSE

Sunderland, February 26

IT is much to be desired that the increasing interest concerning this great phenomenon should supply the only way of obviating the paucity and incompleteness of observations, by having a meeting of observers and advanced nature-students either at London or Bristol. The Utrecht observation says: "When this arch had obtained the length of 90° (which lasted only a few seconds), a separation was made in the middle of its length," &c. I think this accounts for many of the discrepancies.

M. Groneman writes: "The Dutch observations confirm the English, only the phenomenon seems to have been of greater apparent size and therefore nearer." I used to think this for the same reason he gives, but I now think it probable that it was further from the earth when it first approached.

From Bordeaux I learn the sky was cloudy, but the aurora was well seen from Rome, Spezia, and Florence, and I have hopes of observations from the north of Italy.

The logical position is that we must lay aside all preconceived opinions; that we must be prepared to receive fresh ideas from our new views of the action of intense heat on gases and meteorites.

I have only one point to add to my own observation (at two, not ten, minutes past six, as misprinted), that the object, when nearest, presented through its length (but rather below than above) a remarkable "*boiling*" appearance (as seeds in a capsule), while the edges appeared smooth and quiet.

The Rookery, Ramsbury, February 20 ALFRED BATSON

Aurora

A NEWSPAPER paragraph that has come under my notice describes "a strange phenomenon" seen at Brixham on Thursday morning at 1.30—the 15th instant is to be inferred from the date of the paper. It would seem to have been an aurora—yet another example of exceptional auroral activity attendant on the passage of large sun-spots, as there was a spot of importance approaching the sun's central meridian at the time. Any definite information concerning this particular manifestation, or indeed aurora generally near the date in question, appears worthy of a place in your journal. The sun-spot maximum is passing—perhaps past—and such opportunities should not be lost.

February 24 F. B. E.

DIURNAL VARIATION OF THE VELOCITY OF THE WIND ON THE OPEN SEA, AND NEAR AND ON LAND[1]

DURING the three-and-a-half years' cruise of the *Challenger*, ending with May, 1876, observations of the force and direction of the wind were made on 1202 days, at least twelve times each day, of which 650 days were on the open sea, and 552 days near land. The observations of force were made on Beaufort's Scale, (0—12) being the scale of wind-force observed at sea. The five oceans have been examined separately, viz., the North and South Atlantic, the North and South Pacific, and the Southern Ocean, and thereafter the results grouped together. The mean diurnal periodicity in the force of the wind on the open sea and near land respectively is shown on Fig. 1, where the figures on the left are Beaufort's Scale, and those on the right their equivalents in miles per hour. The solid line represents the mean force on the open sea, and the dotted line the mean force near land.

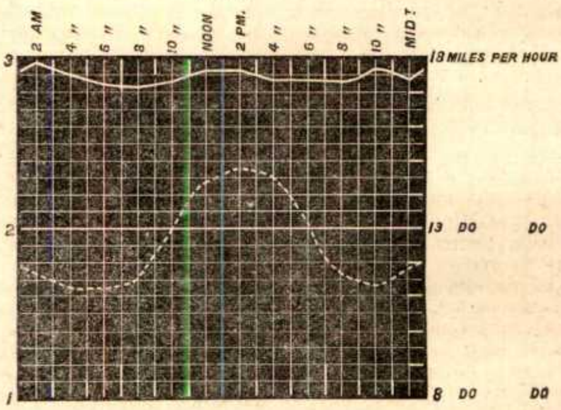
As regards the open sea, it is seen that the diurnal variation is exceedingly small, showing only two faintly-marked maxima about midday and 2 a.m. respectively. On examining, however, the separate means for the five oceans, no uniform agreement whatever is observable among their curves. The slight variations which are met with are different in each case, not one of the maxima or minima being repeated at the same hours in more than two of the five oceans. It follows, therefore, that the force of the winds on the open sea is subject to no distinct and uniform diurnal variation. The difference between the hour of least and that of greatest mean force is less than a mile per hour.

Quite different is it with the winds encountered by the *Challenger* near land, where the observations of the force of the wind give a curve as pronouncedly marked as the ordinary diurnal curve of temperature. The minimum occurs at 2 to 4 a.m., and the maximum from noon to 4 p.m., the absolute highest being at 2 p.m. The curve constructed for each of the five oceans, from the observations near land, gives one and the same result, viz., a curve closely agreeing with the curve of diurnal temperature.

The 650 daily observations on the open sea give a mean velocity of 17½ miles per hour, but the 552 near land give a velocity of only 12½ miles per hour. The difference is greatest at 4 a.m., when it amounts to upwards of 6 miles an hour, but is diminished as the temperature rises, till at 2 p.m. it is less than 3 miles an hour.

At Mauritius, which is situated within the south-east trades, the minimum velocity of the wind is 9·7 miles per hour, occurring from 2 to 3 a.m., from which hour it rises to the maximum 18·5 miles from 1 to 2 p.m., the influence of the sun being thus to double the wind's velocity. At Batavia, situated in a region where the mean barometric gradient is much smaller, the differences are still more decided. From 1 to 6 a.m., 85 per cent. of the whole of the observations are calms, whereas from noon to 2 p.m. only 1 per cent. are calms. In all months the minimum velocity occurs in the early morning, when the temperature is lowest, and the maximum from 1 to 3 p.m., when the temperature is highest. At Coimbra, the mean maximum hourly velocity in summer is five times greater than the minimum velocity, whereas in winter it is only about a half more. At Valencia, in the south-west of Ireland, one of the stormiest situations in western Europe, the three summer months of 1878 gave a mean hourly velocity of 13·3 miles per hour, the minimum oscillating from 10 to 11 miles an hour from 9 p.m. to 6 a.m., and the maximum exceeding 16 miles an hour from 11 a.m. to 5 p.m. The absolutely lowest hourly mean was 10 miles at 11 p.m., and the highest 18 miles at 1 p.m., the velocity about midday being thus nearly double that of the night. The results of observations at many other places might be added to these, including those published by Wild, Hann, Köppen, Hamberg, and others, which go to establish the fact that the curves of the diurnal variation of the velocity of the wind generally conform to the diurnal curves of temperature. The curves of the diurnal variation are most strongly marked during the hottest months. The maximum velocity occurs at 1 p.m., or shortly thereafter, being thus before the maximum temperature of the day (occurring therefore at the time when insolation is strongest); and the minimum in the early morning, when the temperature falls to the lowest, or when the effects of terrestrial radiation are at the maximum. The rule appears to hold good with all winds, whatever be their direction, as shown by Hamberg. The exceptions to this rule are so few, and of such a nature, that they are in all probability attributable to causes more or less strictly local.

FIG 1.



With respect to cloud, Hann has pointed out that for a number of places the mean maximum hourly velocity is 102 per cent. above that of the minimum with clear skies; 77 per cent. with skies half covered with clouds; and 50 per cent. with skies wholly covered. At Vienna, however, these rates of increase are, for clear skies, 101, and half-covered skies, 66 per cent., whereas when the sky is overcast the variation becomes irregular and but faintly marked. Hann has also examined the Vienna observations of the wind on those days when the velocity did not exceed 30 kilometres per hour and on the days when this rate was exceeded, and finds the diurnal periodicity well marked with light and moderate winds, but irregularly and only slightly marked with strong winds and stormy weather.

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[1] Part of this article is abridged from a forthcoming volume of the "Reports" of H.M.S. *Challenger*, by permission of the Lords Commissioners of H.M. Treasury.

In inquiring into the remarkable facts regarding the variation in the diurnal velocity of the wind observed in all climates, attention is first drawn to the two curves of Fig. 1, showing the observations of wind-force made on board the *Challenger* during the cruise. As regards the open sea, the diurnal curve shows practically no variation. The whole of the observations of the surface temperature of the North Atlantic made by the *Challenger* have been discussed, with the result that the daily range is only 0°·7. Hence the statement may be regarded as substantially correct, that over the ocean the atmosphere rests on a floor the temperature of which is all but constant day and night; and, so far as concerns the generation of ascending aërial currents from a heated surface, practically constant.

On approaching the land, however, the daily range of the temperature of the air over the sea becomes materially augmented, the daily range being 4°·3, and, as all observation shows, the temperature over land still more so. Now, bearing in mind that the temperature has risen above its daily mean at 10 a.m., and fallen below it at 10 p.m., an examination of the curve of velocity near land in Fig. 1 reveals the fact that the increase in the diurnal velocity of the wind is entirely restricted to those hours of the day when the temperature is above the daily mean, and the maximum velocity is reached at the hour when insolation, or the sun's heating power, is strongest. The phenomenon of the diurnal variation in the wind's velocity is thus associated in the closest manner with the temperature of the surface on which the air rests. Where there is practically no variation, as in the temperature of the surface of the sea, there is no variation in the velocity; but where, as on land, the temperature of the air has a strongly-marked daily period, the wind-force also is strongly marked, and the increase rises and falls with the degree of insolation on the surface. Further, the velocity increases, not with the increase in the temperature of the air, but with the heating of the surface; in other words, with the conditions on which ascending aërial currents depend.

It is also to be observed, as regards the curves of the five oceans, that they show in each case and at all hours of the day a greater velocity of the wind on the open sea than near land.

The following are the mean and extreme hourly velocities, in miles per hour, for the five oceans:—

	North Atlantic.	South Atlantic.	North Pacific.	South Pacific.	Southern Ocean.
	Miles.	Miles.	Miles.	Miles.	Miles.
Mean hourly velocity on open sea	18·0	18·1	14·5	16·2	23·5
Mean hourly velocity near land	15·0	14·7	9·6	11·0	17·6
Difference	3·0	3·4	4·9	5·2	5·9
Highest mean hourly velocity near land	17·0	16·4	11·6	13·7	20·8
Lowest mean hourly velocity near land	13·1	13·0	10·0	9·3	14·3
Diurnal variation near land	3·9	3·4	1·6	4·4	6·5

Thus the winds are lightest on the North Pacific, and strongest on the Southern Ocean, and these oceans show respectively the least and the greatest diurnal variation in the force of the wind on nearing land.

From the number and character of the two sets of observations, it may be assumed, without risk of error, that the open-sea and the near-land winds, summarised and represented in Fig. 1, were atmospheric movements resulting from mean barometric gradients substantially equal to each other. From the above table it is seen that in each of the oceans the mean velocity near land is less than that on the open sea, the two extremes being the North Atlantic, with a difference of 3·0 miles, and the Southern Ocean with a difference of 5·9 miles; and that even the maximum velocity during the day is always less than the velocity on the open sea. The slight rise in the near-land curve during night is probably wholly caused by the land-breezes felt on board the *Challenger* when near land. In strictly inland places, tolerably well situated for making observations of the wind, this feature does not appear in the curve, and there the velocity falls to the diurnal minimum during the period of lowest temperature, or when the effects of terrestrial radiation are most felt on the surface of the ground.

From these results it follows that, so far as concerns any direct influence on the air itself, considered apart from the floor or surface on which it rests, solar and terrestrial radiation do not exercise any influence in causing the diurnal increase of the velocity of the wind with the increase of the temperature of the air; or if there be any influence at all, such influence is altogether insignificant, as the observations of the *Challenger* on the five great oceans of the globe conclusively prove. The same observations show that on nearing land the wind is everywhere greatly reduced in force, the retardation being due chiefly to friction, and to the viscosity and inertia of the air in relation to the obstructions offered by the land to the onward course of the wind. The retardation is greatest when the daily temperature is at the minimum, and it is particularly to be noted that though the temperature rises considerably, yet no marked increase in the velocity sets in till about 9 a.m., when the temperature has begun to rise above the daily mean. From this time the increase is rapid. The maximum velocity is reached immediately after the time of strongest insolation, and falls a little, but only a little, during the next three to five hours, according to season, latitude, and position. The velocity is low during the hours when the temperature is lower than the daily mean, and the least velocity occurs early in the morning. Even the maximum near land falls considerably short of the velocity which is steadily maintained over the open sea by night as well as by day.

The period of the day when the wind's velocity is increased is thus practically limited to the time when the temperature is above the daily mean, and the surface superheated, and the influence of this higher temperature is to counteract to some extent the retardation of the wind's velocity resulting from the causes already stated. The results show that the increase in the diurnal velocity of the wind is due to the superheating of the surface of the ground, and to the ascensional movement of the air consequent thereon, which tend to reduce the effects of friction and viscosity of the air. It is of importance in this connection to keep in view the fact, shown by hourly observations made at the instance of the Marquis of Tweeddale in 1867 on the temperature of the soil and air, that in cloudy weather a temperature much higher than that of the air near the ground was radiated from the clouds down upon the earth's surface (*Journal Scottish Meteorological Society*, vol. ii. p. 280). Hence in cloudy weather the superheating of the surface-layer of the ground will often take place, the greatest degree of heating being under an overcast sky, where the cloud-covering is of no great thickness, and the temperature of the clouds themselves is much higher than that of the surface of the earth. On the other hand, little or rather no heating will take place, when the cloud-screen which overspreads the sky is of great thickness, and the temperature of the clouds is not greater than that of the surface; and when the temperature of the cloud-screen is lower than that of the surface, the temperature of the latter will fall. It is scarcely necessary to remark that in discussing the influence of cloud on the diurnal periodicity of the wind's velocity, only such means are of real value as are calculated from a very large number of observations.

During the night, when terrestrial radiation is proceeding, the temperature of the surface falls greatly, and instead of an ascensional movement in the lowermost stratum of the air, there is, on the contrary, a tendency towards, and, if the wind be light, an actual descensional movement down the slopes of the land. The effects of friction being thus intensified, the velocity of the wind falls to the daily minimum during these hours.

ALEXANDER BUCHAN

EPHEMERIS OF THE GREAT COMET, b 1882

(Communicated by Vice-Admiral Rowan, Superintendent U.S. Naval Observatory)[1]

GREENWICH MEAN NOON

	R.A.			Decl.			Log. r.	Log Δ.
1883	h.	m	s.	°	′	″		
Feb. 10·0,	6	0	37·8 ...	−19	41	17 ...	0·48137 ...	0·38891
14·0,	5	57	40·4 ...	18	40	13 ...	0·48909 ...	0·40520
18·0,	5	55	19·7 ...	17	41	17 ...	0·49669 ...	0·42132
22·0,	5	53	32·7 ...	16	44	35 ...	0·50413 ...	0·43723
26·0,	5	52	14·7 ...	15	50	14 ...	0·51133 ...	0·45282
March 2·0,	5	51	24·4 ...	14	58	16 ...	0·51841 ...	0·46817
6·0,	5	50	58·7 ...	14	8	43 ...	0·52532 ...	0·48322
10·0,	5	50	54·8 ...	13	21	37 ...	0·53200 ...	0·49790
14·0,	5	51	12·3 ...	12	37	0 ...	0·53861 ...	0·51231
18·0,	5	51	47·9 ...	11	54	52 ...	0·54508 ...	0·52635
22·0,	5	52	39·5 ...	11	15	10 ...	0·55135 ...	0·53995
26·0,	5	53	46·1 ...	10	37	56 ...	0·55751 ...	0·55316
30·0,	5	55	6·1 ...	10	3	6 ...	0·56354 ...	0·56594
April 3·0,	5	56	38·1 ...	9	30	34 ...	0·56944 ...	0·57828
7·0,	5	58	20·9 ...	9	0	19 ...	0·57520 ...	0·59015
11·0,	6	0	13·9 ...	−8	32	21 ...	0·58090 ...	0·60158

Note.—In the published elements ϕ should be 89° 13′ 42″·70 instead of 89° 7′ 42″·70.

Washington, February 10

E. FRISBY,
Prof. Math., U.S.N.

ILLUSTRATIONS OF NEW OR RARE ANIMALS IN THE ZOOLOGICAL SOCIETY'S LIVING COLLECTION[2]

XI.

29. THE CAPE SEA-LION (*Otaria pusilla*).—It is a singular and as yet unexplained fact in geographical distribution, that while the Sea-lions amongst Mammals and the Albatrosses amongst Birds are confined to the South Atlantic Ocean, both these groups reach up to high northern latitudes in the Pacific. In the Atlantic, no Albatross is seen "north of the line," whereas these birds are familiar objects on the coasts of both California and Japan. No Sea-lion is met with in the Atlantic until we get to the Cape on one side and the La Plata on the other, but these animals are well-known objects at San Francisco, and the great supply of their much-valued furs comes from the far northern territory of Alaska.

The Sea-lion first became an inhabitant of our Zoological Gardens, and thus known to Europe in a living state, in 1866, when a French seaman, François Lecomte, brought to this country an example of the Patagonian species (*Otaria jubata*), and exhibited it to the public. The remarkable form of this animal, its extreme docility, and its agile movements attracted great attention, and led to its acquisition by the Zoological Society, in whose Gardens it quickly became an established favourite. Upon the death of this individual in the autumn of the same year, the Council of the Society determined to send out Lecomte, who had entered their service in charge of it, to the Falkland Islands, in order to obtain other specimens. Lecomte returned to this country in August, 1867, but owing to various unforeseen circumstances only succeeded in landing alive one of the four Sea-lions with which he had started from Port Stanley. This animal, young and small on its arrival, throve well under Lecomte's careful management, and soon supplied the void occasioned by the death of the original specimen. Like its predecessor, it exhibits extraordinary agility in the water, and catches the fishes thrown to it for food both above and below the surface with unerring aim.

Four years subsequently, in 1871, the Society received from Sir Henry Barkly, then Governor of the Cape Colony, a present of a young specimen of the Cape Sea-lion, of which we now give an illustration (Fig. 29). Like its Patagonian relative, the Cape Sea-lion is a female, and although quite adult, does not attain the dimensions of the male sex of these animals In general appearance, shape, and form, the two species are very similar, and present little obvious differences to the casual observer, except that the ear-lobe is longer in the Cape animal. To the two females has recently been added a young male of the Patagonian form, and the three individuals now live together in the narrow limits of their basin in the greatest harmony, forming one of the most attractive groups in the Regent's Park Gardens. Little has been recorded of the mode of life of the Sea-lion in a state of nature, but Mr. E. L. Layard in his "Catalogue of the South African Museum," tells us that it "is abundant along the whole of the coasts of the colony, and has given its name to numerous bays, islands, and capes, of which 'Robben' Islands near Cape Town is perhaps the best known.

"It resorts to these places in great numbers for breeding purposes, and is sought for and slain for the sake of its fur and oil. The male is said to be maned, and to much exceed the female in size, but though double the market value of the skin has been offered by the Museum for a skin of the male of this common animal, as it is not *the custom* of the sealers to take the skin off, leaving in the head and feet, we have been unable to procure one."

As regards the habits of some of the other members of this genus, which are of the most extraordinary character, we have now ample details concerning the North Pacific species in a very interesting and well illustrated work prepared by Mr. Henry W. Elliott on the Seal Islands of Alaska and their productions.[1]

Soon after the Sea-lions were established in the Zoological Gardens in this country, specimens of these animals were obtained by the principal Gardens on the Continent, and basins built for the exhibition of their aquatic evolutions. But the examples on the Continent, as well as those in the Aquarium at Brighton, all belong to one of the North Pacific species of Sea-lion (*Otaria californiana*), which is found in enormous multitudes upon the Pacific coast. Of the South African species now figured, the example in our Zoological Society's Gardens is the only one yet brought alive to Europe.

30. BLANFORD'S SHEEP (*Ovis blanfordi*).—Every high mountain-tract in Northern and Central Asia appears to be occupied by a distinct form of Wild Sheep (*Ovis*), while single outliers of the same genus are found far to the west in Sardinia and to the east in North America. Some of these animals, such as the celebrated "Ammon" of Ladakh (*Ovis hodgsoni*) and the Snow-sheep of Kamschatka (*O. nivicola*), attain a magnificent size and

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[1] Computed from elements (NATURE, vol. xxvii. p. 225) and reduced to the mean equinox 1883·0.

[2] Continued from p. 154.

[1] A Monograph of the Seal Islands of Alaska. By Henry W. Elliot Reprinted, with additions, from the Report of the Fishery Industries of th Tenth Census. 4to. Washington, 1882.

development, whilst others, like the Sardinian species (*Ovis musimon*), are more nearly of the dimensions of the ordinary domestic animal.

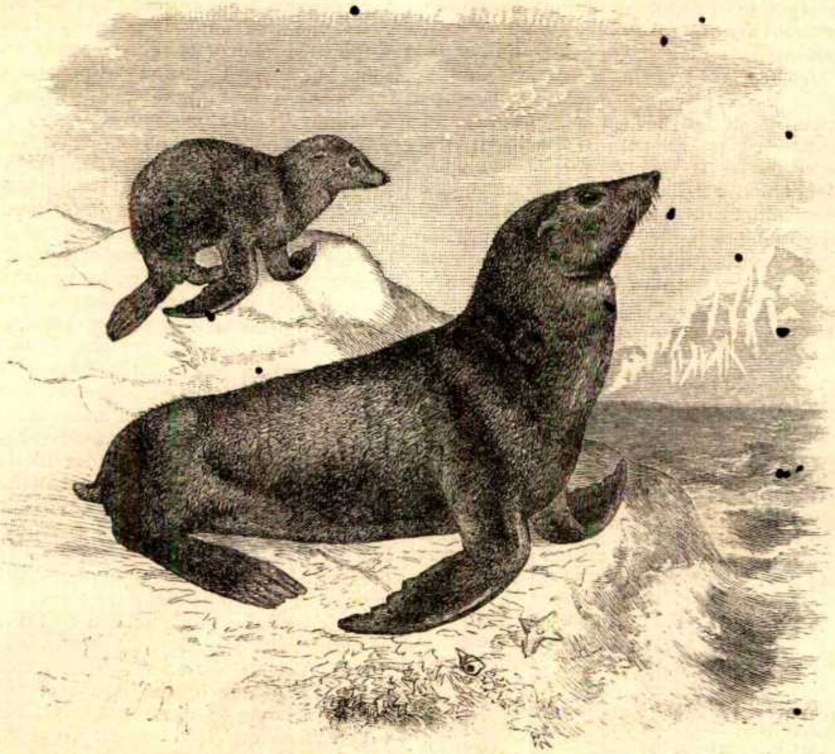

FIG. 29.—The Cape Sea-lion

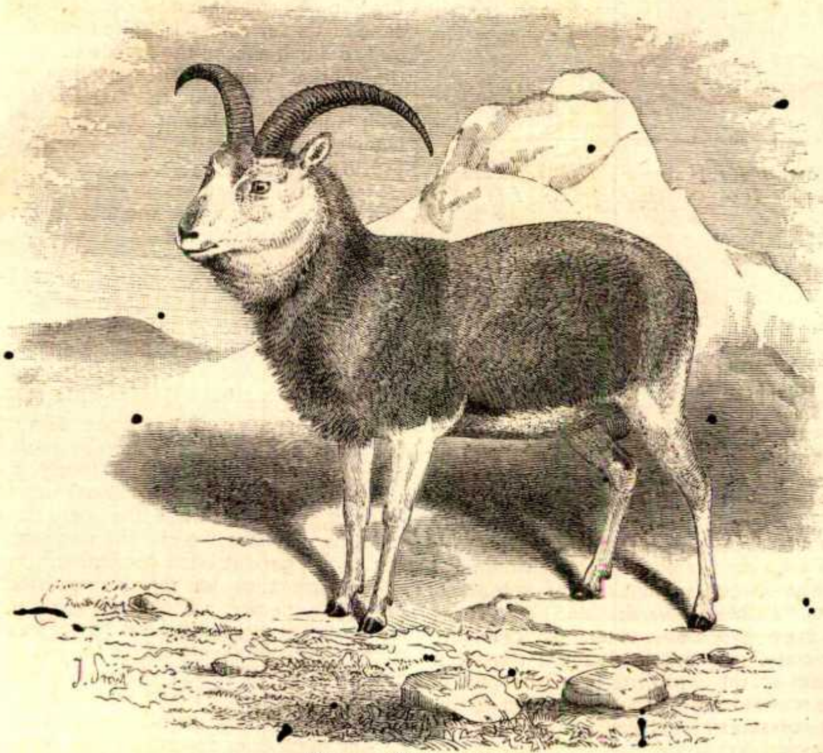

FIG. 30.—Blanford's Sheep.

The Wild Sheep best known to the sportsmen of British India is the "Koch" or "Gudd" of the Punjaub, also called the "Oorial." The Oorial frequents the rocky and stony hills of the Punjaub, as also the Sulimani range on the other side of the Indus and the hills of Hazara and Peshawur. In these districts, Dr. Jerdon tells us, it occurs at very low elevations—from 800 to

2000 feet above the sea-level—and is capable of enduring great heat.

The Punjaub Sheep was introduced into the Zoological Society's series nearly thirty years ago, and has frequently bred in their menagerie. The adults of both sexes and the lambs, of which two are generally produced at a birth, are correctly figured in Wolf and Sclater's "Zoological Sketches" from specimens living in the Society's Gardens.

The recent extension of British influence into Khelat and Afghanistan has led to our acquaintance with the Wild Sheep of the higher ranges of these territories, which, although closely allied to that of the Punjaub, is perhaps distinct and entitled to specific separation. Such at least is the opinion of the Indian naturalist, Mr. A. O. Hume, who in 1877 described and figured the horns of this form, from a specimen received from Major Sandiman, under the name *Ovis blanfordi* in honour of Mr. W. T. Blanford, a well-known Indian zoologist and geologist. Mr. Hume's example of Blanford's Sheep was obtained in the hills above the Bolan Pass. The specimen from which our drawing has been prepared (Fig. 30) was captured in Afghanistan during the recent campaign in that country, and presented to the Zoological Society by Capt. W. Cotton in February, 1881. It is a young male animal with the horns not yet fully developed, and has been placed in company with a female of the better-known Sheep of the Punjaub hills, with which, there is no doubt, it will readily cross.

31. THE UVÆAN PARRAKEET (*Nymphicus uvæensis*).—In the second volume of Capt. Cook's "Voyage towards the South Pole and round the World" will be found (at p. 110) a large, double, copper-plate engraving entitled a "View in the Island of New Caledonia," taken from a sketch prepared by Hodges, the artist of the expedition. The left-hand corner of this engraving contains a rude figure of a parrot with two feathers projecting from the summit of its head. This is doubtless the first representation ever given of the celebrated "Horned Parrot" of New Caledonia, of which a single specimen was obtained by the great circumnavigator when the island itself was first discovered (in September, 1774), and brought home for the collection of Sir Joseph Banks.[1]

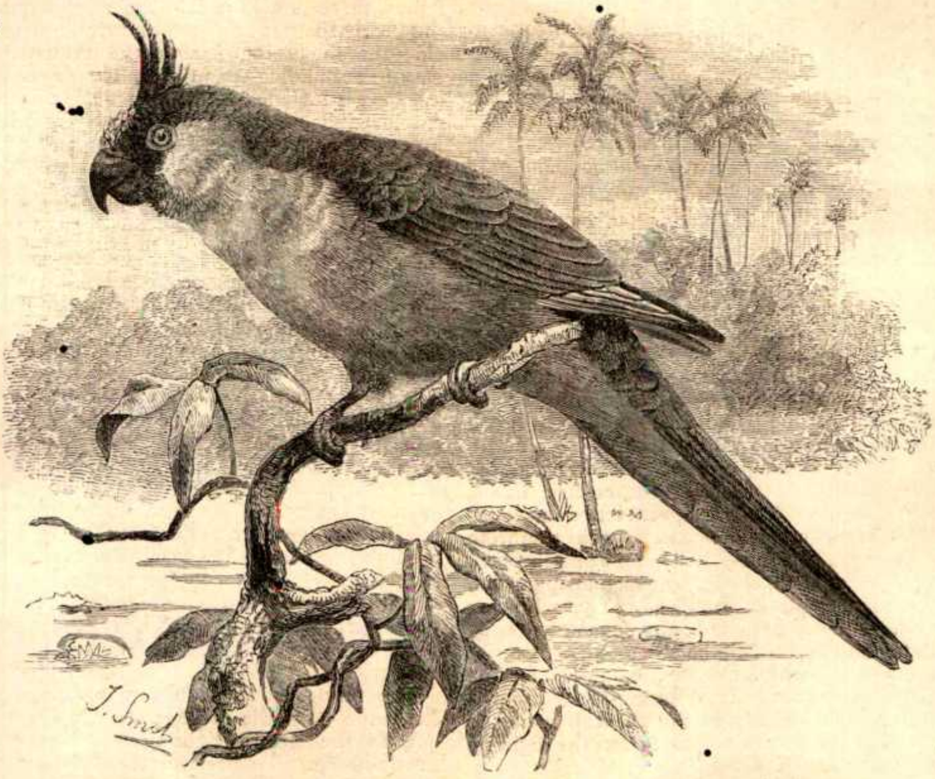

FIG. 31.—The Uvæan Parrakeet.

Since the time of Cook until recent days little more has been known of this singular parrot. In the posthumous works of Forster, who accompanied Cook as naturalist, it was described as "*Psittacus bisetis*," and Wagler, in 1832, made it the type of a new genus "*Nymphicus*." But specimens remained very scarce, and Dr. Finsch in his excellent history of the Parrot-tribe, published in 1868, tells us that it was then still one of the rarest of the whole family in the museums of Europe.

In 1879 however two living examples of this parrot were brought to London in a vessel coming from Sydney, and secured by the Zoological Society for its parrot-house. There was thus for the first time for those who do not wish to make a voyage to New Caledonia, an opportunity of seeing this lovely species in its full beauty. What, however, followed was still more singular. In April, last year, while the two Horned Parrots were still alive, there arrived in the London market a second pair of birds of the same peculiar structure, but presenting ample distinctions for their recognition by naturalists as a distinct species. These birds, it need hardly be said, were quickly secured for the Zoological Society's Aviary, so that the two forms might be exhibited side by side. At the same time was received a communication from Mr. E. L. Layard, H.B.M. Consul at Nouméa, in New Caledonia, a Fellow of the Zoological Society, and a well-known naturalist, describing the new species under the name *Nymphicus uvæensis* (see *P. Z. S.*, 1882, p. 408, Pl. XXVI.).

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[1] See Latham's "Synopsis of Birds," vol. i. p. 248.

It would appear that the Uvæan Parrakeet, of which we now give an illustration (Fig. 31), taken from one of the Zoological Society's living specimens, is a kind of satellite of the New Caledonian Parrakeet, as is the small island of Uvéa, in which it is found, of the larger island of New Caledonia. Mr. Layard had a living pair of the Uvean bird for some time in his possession before he noticed their difference from the New Caledonian bird, of which he had regarded them as the immature form. But in the first place the crest of the two birds is totally different. In *Nymphicus cornutus* the crest is composed of two elongated feathers, which are black, faintly tinged with green, and broadly tipped with red. In *N. uvæensis*, as will be seen in our figure, the crest consists of a bunch of about six short, upturned, entirely green feathers, springing from the end of a small spot of red which occupies the centre of the forehead. In *N. cornutus* the two long crest-feathers rise from the centre of the broad red cap which covers the whole top of the head. Besides this difference the former bird does not present the broad orange nuchal collar which ornaments *N. cornutus*, and exhibits only the faintest trace of orange on the rump.

The small island of Uvéa, one of the Loyalty group, to which the new species is confined, consists, as Mr. Layard tells us, of a series of small islets joined together by a connecting reef with a lagoon in the centre. It is very singular that this distinct form should be found only in so restricted a locality, while its near relative, the "Horned Parrot" of Cook, appears to be distributed all over the large island of New Caledonia.

THE ELECTRIC LIGHT AT THE SAVOY THEATRE[1]

MR. D'OYLY CARTE, having determined to light the Savoy Theatre by the Swan incandescence electric light, intrusted the work of installation to Messrs. Siemens Brothers and Co. The theatre is lighted by no less than 1194 Swan lights of the improved form introduced by Mr. C. H. Gimingham, of the Swan United Electric Light Company. Of these 1194 electric lights, the auditorium is lighted by 150 lamps attached in groups of three, supported on threefold brackets projecting from the different tiers and balconies, each lamp being inclosed within a ground, or opaloid, shade, by which arrangement a soft and pleasant light is produced. These bracket lamp-holders have been designed and constructed by Messrs. Faraday and Son, of Berners Street, London.

Two hundred and twenty lamps are employed for the illumination of the numerous dressing-rooms, corridors, and passages belonging to the theatre, while no less than 824 Swan lamps are employed for the lighting of the stage.

The stage lights are distributed as follows :—

6	rows of	100	lamps each	above the stage ...	600
1	,,	60	,,	,, ,, ...	60
4	,,	14	,,	fixed upright	56
2	,,	18	,,	,, ...	36
5	,,	10	,,	ground lights ...	50
2	,,	11	,,	,, ...	22
					824

In addition to the above-mentioned lights within the theatre, there are eight pilot lights in the engine-room, which, being in the same circuit with some of the lights in the theatre, serve the purpose not only of illuminating the machinery, but also of indicating to the engineer in charge of the machines, by the changing of their illuminating power, when the lights in the building are turned up or down.

The lamps are at present worked in parallel circuit in six groups, five of which comprise 200 lamps each, and the sixth 202 lamps. The current of each group is produced by one of Messrs. Siemens Brothers and Co.'s W_1 alternate current machines, the field magnets of which are excited by a separate dynamo electric machine of the Siemens type, known as D_7. The machines and engines are fixed in a shed erected on a piece of waste land adjacent to the Victoria Embankment, the current being conveyed to the theatre by means of insulated cables laid underground.

The six alternate or W_1 machines are driven at a speed of 700 revolutions per minute, and the six exciting or D_7 machines at 1150 revolutions, by three steam-engines, that is to say, a portable 20-horse engine by Garrett, a 12-horse power portable by Marshall, and a 20-horse semi-portable engine by Robey, but the power actually utilised, as measured by a "von Hefner Alteneck" dynanometer, is between 120 and 130 horse-power. We must not, however, omit to state that, in addition to the six pairs of machines for working the 1202 incandescent lamps, there is also a D_2 Siemens dynamo machine for producing the powerful arc electric light suspended outside the theatre, and over the principal entrance in Beaufort Buildings, and that the power to drive this machine is included in the above-mentioned horse-power employed, as well as that necessary for driving the shunt machine used to charge the secondary batteries for the fairy lamps.

The most interesting feature, however, from a scientific point of view, of this most interesting installation, is the method by which the lights in all parts of the establishment are under control, for any of the series of lights can in an instant be turned up to their full power or gradually lowered to a dull red heat as easily as if they were gas lamps, by the simple turning of a small handle. There are six of these regulating handles, corresponding to the number of the machines and circuits—arranged side by side against the wall of a small room on the left of the stage, and each handle being a six-way switch, can, by throwing into its corresponding magnet-circuit greater or less resistance (according to its six stages), lessen or increase the strength of the current passing through the lamps by as many grades. The special interest of this part of the installation, however, is the fact that the turning down of the lights is accompanied by a corresponding saving of motive power in the engine, for the variable resistance which is controlled by the regulators is not thrown into the external or lamp circuit of the alternate current machines, but into the circuit by which their field magnets are excited.

The fittings of the lamps in the passages, staircases, &c., have, up to the present, been of a temporary nature, but, as the electric lighting has worked to the entire satisfaction of all concerned, these temporary fittings will now be replaced by permanent brackets, quite independent of the gas brackets.

All risk of fire is avoided by the leading wires being thoroughly insulated, and small pieces of lead wire being inserted into the circuit wherever a branch wire leaves the mains. These "safety-pieces" of lead are chosen of such size that they will melt before the conductors themselves become sufficiently heated to cause any danger, and by their melting the current is at once interrupted.

The small lamps worn by the fairies, and which have been specially made by the Swan United Electric Light Company, are rendered incandescent by the current produced from a small "secondary" battery, which is carried on the back like a small knapsack. These secondary batteries have been made by Messrs. Siemens Brothers and Co. on a new plan, and are charged by a shunt-wound Siemens' dynamo in the engine-shed. Each battery is provided with a switch, by means of which the light can be turned on or off by the wearer at pleasure.

The system of electric lighting has now been working at this theatre for about a year and a half without any accidents, and has proved to possess many advantages over gas as applied to the illumination of buildings of this description. Not the least amongst these are the total absence of heat and vitiated air in the house, and the length of time during which the decorations will retain their freshness and colour instead of becoming quickly faded and tarnished, as would be the case were the old system of gas adopted.

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[1] Communicated.

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT

By inhibition we mean the arrest of the functions of a structure or organ, by the action upon it of another, while its power to execute those functions is still retained, and can be manifested as soon as the restraining power is removed.

It is thus distinguished from paralysis, in which the function is abolished, and not merely restrained.

Inhibition is one of the most perplexing problems in physiology, and we have at present no satisfactory hypothesis regarding it. It plays, however, such a very important part in pharmacology, that we cannot pass it over; and as it is through the action of drugs upon the various functions of the body that we have already arrived at a knowledge of inhibitory actions, which would otherwise have been impossible—as, in fact, pharmacology has here quite outstripped physiology—we are obliged to enter into some hypothetical considerations, in order to be able to form some kind of idea regarding the mode of action of many drugs.

Hypotheses serve as "pegs on which to hang facts," and by their aid the isolated facts which few memories could carry may be arranged, and their relation to each more readily perceived. A hypothesis serves also as a guide for further experiments, by which it may be either disproved or supported. Should facts be against it, so much the worse for the hypothesis; it must be discarded, and another tried in its place; but if facts agree with it we obtain a means of predicting phenomena, and make another step in knowledge. Like other useful things, hypotheses are not without danger, and sometimes do harm by satisfying people and stopping further inquiry. Thus Sultzer noticed the peculiar taste produced by the contact of two dissimilar metals with each other and with the tongue forty years before Galvani; but at that time the doctrine of vibrations was employed to explain all natural phenomena, and he concluded that some peculiar vibration occurred from the contact of the metals, which produced the peculiar sensation on the tongue. All the world were satisfied with the explanation, and thus a prominent fact slept in obscurity from the time of Sultzer to that of Galvani, no further attempts being made to determine the nature of the vibrations or the laws which governed them.[1] Yet in their proper place hypotheses are most useful, and but for the hypothesis that light, heat, and sound are due to waves, our knowledge of their phenomena would be much less than it is.

The cases of inhibition, as we may term them, which we meet with in the study of physics, are the production of complete silence by the interference of two sounds, and of darkness by the interference of two rays of light.

When two sounds or two rays of light are combined, so that the crests of the waves of which they consist coincide, the sound becomes louder and the light brighter. If they are thrown together, so that the crests of the waves in the one sound or ray coincide with the sinuses or hollows of the other, they completely counteract each other, and silence or darkness is produced.

When the waves are of different rhythms, the crests and hollows of the two sounds or rays, which at one time coincide, will gradually interfere, and again gradually coincide, so that rhythmical alternations of loud sound and silence, of bright light and darkness, are produced.

A good example of interference or physical inhibition, and one that affords an illustration well suited to our purpose, is that of Newton's rings. When a lens of small curvature is placed on a plane surface of glass, a series of rings is observed, starting from the centre of the lens and passing concentrically outwards. If monochromatic light is used, such as pure yellow light, pure red light, &c., these rings are alternately bright and dark; but if white light is used, they appear as a number of circular bands of different rainbow colours. The cause of these rings is, that though the surface of the lens appears to the eye to be in contact with the plate of glass over a considerable area, it is not really so; a very fine film of air of varying thickness being interposed between them.

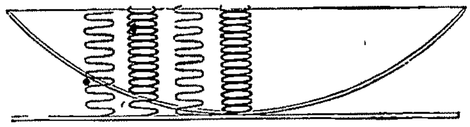

Fig. 1—A very diagrammatic representation of interference in Newton's rings.

When a ray of light passes through the lens on to the glass, part of it is reflected back from the lower surface of the lens, and part of it from the upper surface of the glass plate. Between those two points there is a very minute film of air: *one ray has therefore to travel somewhat further than the other*. The distance which it has to travel is only through the extremely thin layer of air lying between the surface of the lens and the glass and back again; but this distance at some places is just sufficient to throw the waves in the one beam *half a wave-length* behind those in the other, and to produce darkness by their interference.

As we recede from the point of most complete contact between the lens and the glass, the thickness of air increases, the ray has somewhat further to travel, and the distance is then just sufficient to throw it *a whole wave-length* behind the other ray; no interference is produced, and we get a ring of bright light.

Further outwards the increased thickness of the film of air is again sufficient to throw one ray *a wave-length and a half* behind its fellow; interference is again produced, and darkness is the result.

With rays, then, of one colour, or of one wave-length, we get alternately light and darkness by interference.

But it is evident that the extra distance which the waves have to travel in order to produce interference will not be the same for long and short waves; and thus it is found that when white light, which contains rays of different wave-lengths, is used, the rings, instead of being alternately light and dark, are coloured.

The very distance which was sufficient to throw the red rays half a wave-length behind the other, and to produce interference, will throw, let us say, the violet rays a whole wave-length behind, and thus there will be no interference and *vice versâ;* the distance which causes interference of the violet rays does not cause interference of the red, and so on with other colours.

Thus the spaces which would have been perfectly dark when rays of pure red or pure violet, or more correctly ultra-violet, were used, would be filled up by the other if used together, and when white light is used, the various waves interfere at different places, and so we get a series of rainbow colours.

The extra distance which one beam has to travel in order to produce interference with another is *not absolute*, but relative to the wave-length. This relation differs for different wave-lengths, and therefore if the relative distances remain constant, the effect of the beams on each other will vary if their wave-lengths be changed.

It is obvious that if both the wave-lengths and the distances they have to travel remain the same, the effect of the beams on each other will be altered by any change in their rate of travel such as would be effected by altering the media through which they pass.

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[1] Ree's Cyclopedia. Article "Galvanism."

This is a most important point in regard to the hypothesis of the causation of inhibition by interference of vibrations in the nervous system. It may therefore be useful to illustrate this further, and probably it could not be done better than by using, with a little modification, the example given by Sir J. Herschel in his article on Light in the Encyclopædia Metropolitana. "Let R be a reservoir of water, from which the channels A and B proceed, to join each other at P; they are supposed to be equal in every respect except that B is longer than A. If a wave from the reservoir enters the openings of A and B at the same time and travels at the same rate along them, the wave which passes through A will reach P sooner than the one which passes through B, so that the water at that point will be agitated by two waves in succession. But let the original cause of undulation be continually repeated so as to produce an indefinite series of equal and similar waves. Then if the difference of lengths of the two canals A and B be just equal to half the interval between the summits of two consecutive waves, it is evident that when the summit of any wave propagated along A has reached the point of intersection P, the depression between two consecutive summits (viz., that corresponding to the wave propagated along A and that of the wave immediately preceding it) will arrive at the intersection P by the course B. Thus in virtue of the wave along A the water will be raised as much above its natural level as it will be depressed below it by that along B. Its level will therefore be unchanged. Now as the wave propagated along A passes the intersection P, it subsides from its maximum by precisely the same gradations as that along B, passing it with equal velocity, rises from its minimum, so that the level will be preserved at the point of intersection P undisturbed so long as the original cause of undulation continues to act regularly.[1] So soon as it ceases, however, the last half-wave which runs along B will have no corresponding portion of a wave along A to interfere with, and will therefore create a single fluctuation at the point of concourse P."

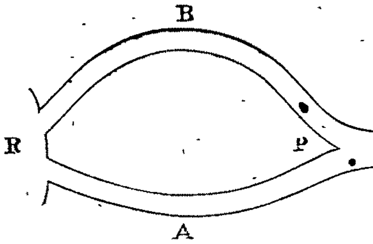

FIG. 2.—Diagram to illustrate Sir J. Herschel's observations on interference. Adapted from his article on "Absorption of Light," Phil. Mag. 1883, p. 405.

It is obvious that if everything else remains the same, the effect which the waves have upon each other at P will be altered if the rate at which they travel is increased or diminished.

The more the speed is increased the less effect comparatively will the greater length of B have in retarding the wave which flows along it, so that its crest will no longer coincide with the trough or sinus of the waves in A, but will, on the contrary, coincide more nearly with the crest of one of the waves in A.

The more the speed is diminished, the more will the wave in B lag behind that in A, so that its crest, instead of coinciding with the trough between two crests of the waves from A will gradually come to coincide with the crest succeeding the trough, and thus double its magnitude instead of destroying it.

We see, then, that under the conditions we have supposed either increase or diminution in the rapidity of their transmission may convert the interference of waves into more or less complete coincidence, and the effect of the two waves may thus be doubled instead of neutralised by their superposition.

The alteration which is produced in the mutual effect of two waves by increase or diminution of their rate of transmission along channels of constant length supplies us I think with a test by which we may ascertain the truth of the hypothesis that inhibitory phenomena in the animal body are due to interference. For if it be true we ought to find that a nerve which produces inhibitory phenomena when excited under normal conditions will gradually lose this power when the rate of transmission along it is increased or diminished, as, for example, by the influence of heat or cold, and will gradually acquire an exactly contrary or stimulating action. This, I think, is shown to be the case by our experimental data so far as they go.

Several authors have pointed out the analogy between inhibitory phenomena in the animal body and the effects of interference of waves of light or sound. This has been done with special precision by Bernard[1] and Romanes.[2] The tendency to do away with the idea of distinct inhibitory centres is gradually spreading, but hitherto no attempt has been made to bring all the phenomena of inhibition under one general rule or to explain the mode in which they are affected by the action of drugs. The object of the present paper is to gather together some instances of inhibition which we find in the body, and to see whether by the theory of interference it is not possible to explain both the curiously perplexing exceptions which we meet with in physiological experiments, and the still more perplexing action of drugs on inhibitory phenomena.

One of the most striking examples of reflex action and of inhibition, is the effect of a slight touch or touches, and of firm pressure upon the palms of the hands, the soles of the feet, or the axillæ, and in some persons also the knees. In many persons a very slight touch or succession of touches upon these parts is sufficient to throw first the respiratory muscles, and then the whole body into violent convulsions. Indeed, it is stated that during the persecution of the Albigenses by Simon de Montfort, several people were tortured to death by tickling the soles of their feet with a feather. The stimulus here applied, and the consequences it produces, appear to be out of all proportion to one another; the stimulus being almost infinitesimal, and the consequences enormous.

In the case of Newton's rings it might be possible with much trouble to throw a different beam into such a condition that it would interfere with one of the beams in the rings and produce darkness, but in the rings a similar effect is produced in a very much simpler way by alteration of part of the same beam. A similar occurrence is to be observed in the inhibition of the reflex action on tickling.

By a very powerful effort of the will we may completely arrest the reflex movement which would otherwise occur, and allow the limb to remain perfectly passive. But the same effect is produced in a much simpler way by applying a firm pressure instead of a slight touch. The firm pressure neutralises the effect of the touch in regard to motion, and not only are no reflex convulsive actions produced, but no tendency whatever to them is felt.

But while the pressure has neutralised the tendency to motion, and has altered the character of the sensation, it has not neutralised sensation. On the contrary, it has rendered it more definite, so that one can distinguish with much greater certainty the particular point of the surface which has been touched. Increased pressure has thus inhibited motion but increased sensation.

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[1] This actually happens in the harbour of Batsha, into which the waves pass from the open sea through two channels of unequal length.

[1] Bernard, La Chaleur Animale, Paris, 1876, p. 371.

[2] Romanes, Phil. Trans. 1877, p. 730.

In a paper on "Inhibition, peripheral and central," which I wrote in the West Riding Asylum Reports in 1874, I tried to explain these phenomena in the following manner: "It appears to me to be in all probability due to there being two sets of ganglia in the cord itself, one motor and one inhibitory. The motor is more readily excited than the inhibitory, and causes violent movements, which the inhibitory centres of the brain cannot restrain without the greatest difficulty, though they are readily controlled by the inhibitory ganglia in the spinal cord. A slight titillation excites the motor, but not the inhibitory spinal ganglia; a stronger pressure stimulates the inhibitory centres also, and thus arrests the movements without any action being required on the part of the inhibitory centres in the brain. We may try to explain this, by supposing that there are two distinct sets of nerves proceeding from the skin to the cord, one of them having the power to excite inhibitory, and the other to excite motor centres. Further, we must suppose that these sets of fibres are endued with different degrees of excitability, the motorial ones being stimulated by a slight touch, but the inhibitory ones only by a stronger impression.

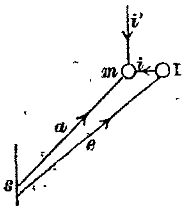

FIG. 3.

"This is represented in Fig. 3, where *s* is the skin; *a*, the fibres proceeding from it to the motor ganglion, *m* and *e*, those going to the inhibitory ganglion, I; *i* is the fibre by which I arrests the action of *m*, and *i'* that by which the brain exerts a similar action. The different fibres by which *m* acts on the muscles have not been introduced into the diagram.

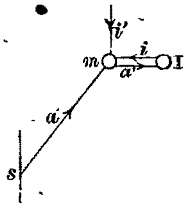

FIG. 4.

"This hypothesis, however, is a very clumsy one, and we explain the facts quite as well by supposing that there is only one set of afferent nerves (*a*, Fig. 4) from the skin to the cord, which transmit a slight impression only to the motor ganglia, *m*, but convey a stronger one along *a'* to the inhibitory ganglia I, also, which then react through *i* upon the motor ones. This latter supposition renders intelligible the fact that it is only when something is drawn quickly and lightly across the skin, so as to make a slight and transient impression on the ends of many sensory nerves, that tickling is felt. If the pressure on the skin is heavier, or if the motion over it is slow, the effect is quite different, and this is just what we might expect if a short and slight impression travels only to the motor ganglia, and a stronger or more lasting one goes to the inhibitory beyond them."

These diagrams themselves are suggestive of interference; but I did not in that paper say anything regarding it, contenting myself only with the term inhibition. One reason that prevented me from considering inhibition in animals as corresponding closely to the interference of light, was that the rapidity of transmission of nervous impulses was differently given by different observers, and indeed, according to Munk, it varies along the course of the same nerve.[1]

Unless the rate of transmission of impulses is constant, one cannot expect interference to produce inhibition. But in his observations on Medusæ, Mr. Romanes found that when the circumference of the bell in a medusa was cut into a long spiral strip, leaving only the centre of the bell uninjured, stimuli applied to the extreme end of the strip passed along it, and were delivered to the centre of the bell, just as if they had been applied to the central part itself—all passing at the same rate they did not interfere with one another. But when the strip was pressed upon or stretched, the passage of impulses was interfered with.

This seems to show that the rate of transmission of a stimulus along a conducting structure is a definite one, provided the structure remain under the same conditions. But still more instructive on this point are the experiments with the Ton-inductorium, invented by my friend Prof. Hugo Kronecker. Other observers have found that when a muscle is irritated by an interrupted current applied to its nerve, the tetanic contraction into which it would be thrown by twenty interruptions per second ceased when the interruptions became as frequent as 250 per second. By using an interrupted current induced by the vibrations of a magnetic rod, which gave out a definite tone, Kronecker and Stirling were able to throw the muscle into tetanus with no less than 22,000 interruptions per second. This success is probably to be attributed to the regularity and equality of the stimuli applied by Kronecker's method, while the fact that their predecessors got no tetanus with more than 250 interruptions per second is probably due to interference of the stimuli they applied.[2] Kronecker's observations show, I think, how definite must be the rate of transmission of stimuli along a nerve so long as it remains under the same conditions and give us a basis for extending the theory of interference from waves of light and sound to vibrations in nervous and muscular tissues.[3]

We are justified, I think, by these experiments in considering that interference may occur in the nervous system, and that one part may exercise an interfering or inhibitory effect upon the other, which is constant under normal conditions, but will be modified when these conditions are altered.

Let us now try to apply this hypothesis to the reflex action which we have just been discussing.

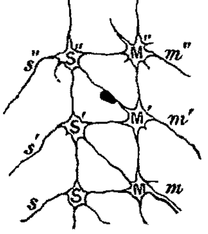

FIG. 5.

Let S, S' and S'' be three sensory cells in the spinal cord, M, M' and M'' motor cells, *s* and *s'* sensory nerves, and *m m'* motor nerves. S B is a sensory and M B a motor cell in the brain. When *s* is stimulated by a slight touch, the stimulus is transmitted up to S, thence to M, and down *m* to muscles, thus causing reflex contraction. This is increased when a number of slight touches are made over a limited surface, as in tickling, because then S and S′ are both stimulated, and more motor impulses are produced. But when harder pressure is made on *s* the stimulus, instead of being confined to S, is transmitted to S′ and thence to M, as well as direct from *s* to M. Thus two impulses are sent to M, which, starting at the same time from *s*, have had a different length to travel round. This different length, we suppose, is just sufficient to allow the impulses to interfere with one another in M and thus destroy each other's action in regard to motion. When *s′* is also irritated at the same time as *s*, the same interference is produced by a stimulus passing from S′ to S″, and then to M′. But at the same time that the relation of S′ S″ to M and M′ is such as to produce interference and inhibition in regard to motor impulses, the relation to each other is such that the impulses mutually strengthen one another on their way up to the brain, and thus the sensation which we perceive on firm pressure is more definite and better localised.

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[1] Archiv. f. Anat. u. Physiol. 1860, p. 798.

[2] It must be borne in mind, however, that the overtones of such a vibrating rod are in the ratio of *n*, 3 *n*, 5 *n*, &c., and not in that of *n*, 2 *n*, 4 *n*, like those of a vibrating string or pipe. Quincke (Poggendorff's Annalen, 1866, vol. viii. p. 182) failed to silence the sounds of such a rod by means of an interference apparatus.

[3] *Vide* Hermann's Handbuch d. Physiol. Bd. i. Th. i. p. 44, and Bd. ii. Th. i. p. 39.

On this hypothesis each successive layer of sensory and motor cells in the spinal cord may have several different functions: (1) Each cell may exercise its own sensory or motor functions in relation to the sensory or motor nerves connected with it; (2) it may exercise an inhibitory function on the sensory and motor cells above or below it, and also on other sensory or motor cells on the same plane with itself; (3) it may have a stimulating function on other cells above, below, or on the same plane as itself, increasing instead of abolishing their action.

The effect that any sensory or motor cell produces when stimulated is not determined then simply by the *properties* of the cell itself, but by its *relations* to other cells or fibres.

Motion, sensation, inhibition, or stimulation are not positive, but simply relative terms, and stimulating or inhibitory functions may be exercised by the same cell according to the relation which subsists between the wave-lengths of the impulses travelling to or from it, the distance over which they travel, and the rapidity with which they are propagated.

T. LAUDER BRUNTON

(*To be continued.*)

NOTES

M. JANSSEN was present at the sitting of the Academy of Sciences on Monday, for the last time before his departure from Paris. He is very busy preparing his apparatus.

BARON NORDENSKJÖLD so very carefully considers every step he takes that we may be sure he has satisfactory reasons for claiming the reward of 25,000 guilders (about 2000*l.*) offered by the Dutch three centuries ago to the discoverer of the North-east Passage. Some surprise is expressed at the Baron's claiming a reward which lapse of time may be considered as having rendered obsolete. At the time it was offered the North-east Passage was regarded as a sea-route of the highest commercial importance, though this idea has been long exploded. Still to some extent Baron Nordenskjöld has shown that the old conception was not without justification, and although the passage is now of no value as a route to China and India, still the Swedish explorer has proved that as a trade-route it may be rendered of considerable value. Moreover as he is so disinterested, ardent, and successful a pioneer of science, we should be glad if the Dutch Government cheerfully admitted the claim. It may be remembered that the much larger reward offered by our own Government to the discoverer of the North Pole was withdrawn many years ago.

AT the last meeting of the Royal Swedish Geographical Society, on the proposal of Baron Nordenskjold, the greatest honour at the disposal of the Society, the *Vega* gold medal, was conferred on Mr. Stanley. The medal, struck *in memoriam* of the *Vega* expedition "for geographical discovery," has only been twice before conferred—viz. in 1881, on Baron Nordenskjold, and in 1882, on Capt. Palander.

IN 1880 the Belgian Academy proposed, as a prize-subject, the relations between physical and chemical properties of simple and compound bodies (completion of the knowledge of these by new experiments). The prize (a gold medal valued at 1000 francs) has been awarded to M. De Heen, engineer at Louvain. His memoir is an extension of one previously sent in, which gained high approbation for original work and results, but was thought badly-proportioned, so that the subject was re-proposed. The work is in five sections, dealing successively with specific heats, dilatability of solids and liquids by heat, changes of state in relation to chemical composition, capillarity, and (here without original researches) molecular volumes, refraction, spectral analysis, and absorbent power of bodies for heat. The ample *résumé* M. Spring gives of this memoir (*Bull. Belg. Acad.* No. 12) indicates matter that must be of much interest and value to the physicist and the chemist.

PERHAPS never in the history of science, the *Lancet* says, has a distinguished career equalled in its length that of M. Chevreul, whose name is best known in this country in connection with his investigations on colour; and it is probably altogether unique for a *savant* to be able, at one of the most distinguished scientific societies in the world, to refer to remarks which he made before the same society more than seventy years previously. A few days ago M. Chevreul made a communication to the Académie des Sciences, and at its close he observed: "Moreover, gentlemen, the observation is not a new one to me. I had the honour to mention it here, at the meeting of the Académie des Sciences, on the 10th of May, 1812"!

THE death is announced of the Silesian botanist, Herr Johann Spatzier, aged seventy-seven; also of Herr Josef Knörlein, the entomologist, at Linz, on February 12, aged seventy-seven.

MOUNT ETNA is very active and ejects red-hot lava. At night the glare is constantly visible. A violent shock occurred on February 15.

WE find in the last number of the *Izvestia* of the Russian Geographical Society a note, by Prof. Lenz, on the cosmical dust collected by M. Marx at the meteorological station of Yeniseisk. After having vainly searched for traces of cosmical matter, as he was advised to do by Baron Nordenskjöld, he discovered it finally on October 31, 1881. The wind was blowing in the evening with great force from the west, and during the night it turned into a strong gale, with some snow and rain. When M. Marx measured next morning the amount of water in his pluviometer, he remarked that it had a considerable quantity of suspended matter of a brick-red colour. After careful analysis this matter proved to consist of iron, nickel, and cobalt. Prof. Lenz does not doubt that the red dust found by M. Marx had a cosmical origin, and points out that it was observed on a day very near to the appearance of the November meteors.

AT Monday's meeting of the Paris Academy of Sciences, M. Tresca read a paper full of facts on the experiments tried at the Gare du Nord. Deducting certain work for the mechanical transmission to the generator, the result was 42 per cent. of energy conveyed instead of 35 per cent. with a smaller velocity, but without deducting this work the alteration was very slight, 33 per cent. instead of 32 per cent.

THE second annual general meeting of the members of the London Sanitary Protection Association was held on Saturday at the rooms of the Society of Arts, under the presidency of Prof. Huxley. From the report of the council, presented to the meeting, it appeared that 368 new members had joined the Association during the year, and there was a total of 533 members. The total number of houses inspected was 362, and in the greater number of these serious errors in the sanitary arrangements of the houses were found and corrected. Twenty-one of them, or 6 per cent., were found to have the drains choked up, and no communication whatever with the sewer; all the foul matter sent down the sinks and soil-pipes simply soaking into the ground under the basement of the houses. In 117 houses, or 32 per cent., the soil-pipes were found to be leaky, allowing sewer-gas, and in many cases liquid sewage, to escape into the house. In 137, or 37 per cent., the overflow pipes from the cisterns were led direct into the drains or soil-pipes, allowing sewer gas to pass up them, and contaminate the water in the cisterns, and in most cases to pass freely into the house. In 263, or nearly three-fourths of the houses inspected, the waste-pipes from baths and sinks were found to be led direct into the drain or soil-pipes, thus allowing the possibility of sewer gas passing passing up them instead of being led outside the house, and made to discharge over trapped gullies in the open air as they should be. Prof. Huxley moved the adoption of the report, and stated that he had found himself unable longer to act as president of the association, owing to the increasing demands upon his time and energies. He was glad however to say that the Duke of Argyll had consented to succeed him in that post. The second annual meeting of the Sanitary Assurance Association was held at the office, Argyll Place, Regent Street, W., on Thursday. In the absence of Sir Joseph Fayrer, Prof. T. Hayter Lewis, F.R.I.B.A., was elected to preside. The secretary read the report of the council for the year 1882, from which it appeared that the inspection of houses, supervision of work, and issue of certificates had been continued on the plan initiated by the association in 1881. The financial statement showed that considerable progress had been made since the issue of the first report. The increase during 1882 had been nearly double that of 1881.

PART 2 of vol. ii. of "The Encyclopædic Dictionary," published by Messrs. Cassell, extends to the word Destructionist. The present instalment seems quite up to the standard of those already published, though for a work of such extent we think the account of the corona of the sun inadequate. On the other hand, to illustrate the term Darwinism, we have half a column biography of Charles Darwin.

THE last number of the *Izvestia* of the East Siberian Geographical Society, which has just reached us, contains a letter from M. Yurgens, chief of the meteorological station at the mouth of the Lena. When leaving Yakutsk with his companions, Dr. Bunge and M. Eigner, he took with him, besides provisions for eighteen months, a wooden house 42 feet long and 21 feet wide, 40 cwts. of petroleum, two cows with a calf, plenty of hay, bricks, lime, and even moss and clay, as there is no clay in the delta of the Lena. As is unfortunately too often the case with such expeditions, the barometers went out of order, and the observers found great difficulty in filling and boiling them again, so that the new meteorological station at Olekminsk has remained without a barometer. On this subject a correspondent writes:—"A new portable barometer would be really an immense benefit for countries like Siberia, but in the meantime would it not be advisable for second-rank meteorological stations to make use of the aneroid? Of course the correction of each aneroid changes slowly but continuously, so that an uncontrolled aneroid has no value at all; but would it not be possible to control it, say every fortnight, by means of a hypsothermometer—a most reliable instrument if the observer follows the advice of Dr. Wild—and, after having boiled the water, leave the thermometer to cool, and make use only of a second reading, which is made when boiling the water for a second time. The observations of Dr. Wild, repeated by M. Krapotkin at the St. Petersburg Physical Observatory on five hypsothermometers taken from an optician's shop, proved that they were most reliable if the above-mentioned precaution were used. Might it not be useful to repeat these observations on hypsothermometers on a larger scale, in order to ascertain the degree of accuracy that might be expected from these instruments, which highly recommend themselves to travellers, and especially for small meteorological stations, by their portability?"

AN avalanche, or rather a landslip, took place at Gudvangsören, in the remote and narrow Næro valley, in Norway, at the end of January last. The quantities of earth and stone precipitated into the valley destroyed several farms, and killed two women. Landslips have previously occurred in this valley.

IT is remarkable that a disease like leprosy should flourish in Norway. From the returns just published this appears however to be the case, although we are happy to say that the number of afflicted is decreasing. At the end of 1875 there were 2008 patients reported in the country. At the end of 1880 the number had fallen to 1582. The disease is stated to be due to the consumption of food in an unwholesome condition, particularly fish, and also to uncleanliness.

ON February 5, at 6.45 p.m., a meteor of unusual size and appearance was observed near Arvika, in Sweden. An observer who happened at the time to be passing a lake—Glasfjorden—states that he first observed the meteor high on the horizon, going from south-east to north-west, when, after about eighteen seconds, it suddenly changed its course to south-east. During its progress to north-west, calculated at eighteen seconds, the meteor made several digressions from its plane, while its size varied from that of an ordinary star to that of the sun, sometimes emitting a white, at others a yellow light, and at times discharging showers of sparks. At the point of changing its direction, when it was so near the surface of the lake that its path was reflected therein, it possessed a distinct tail, and with this adjunct it passed out of the range of sight in a south-easterly direction, after being observed for nearly fifty seconds.

AT Iserlohn (Rhenish Prussia) the fall of a meteorite was observed by several persons on the evening of February 1. Next morning the meteorite was found, having penetrated deeply into the hard-frozen soil of a neighbouring garden. Its weight is 165 grammes, its size that of a goose's egg. The surface is of a glistening black, and the point seems broken off.

A NEW substance, remarkable for its intense sweetness, being much sweeter than cane-sugar, has been lately found by Dr. Fahlberg in the course of some investigations on coal-tar derivatives (*Journ. Frank. Inst.*). He designates it *benzoic sulphinide*, or *anhydrosulphamine benzoic acid*.

MR. H. HEATHCOTE STATHAM will give the first of two lectures, at the Royal Institution, on "Music as a Form of Artistic Expression," on Saturday, March 10. The subject of Prof. Tyndall's discourse on Friday evening, March 16, is "Thoughts on Radiation, Theoretical and Practical."

ON February 11, at 9.50 a.m., an earthquake was noticed at Szigeth (Hungary). It lasted four seconds. It was also felt in the Bosnian village of Looskrupa and its neighbourhood.

An Electro-technical Exhibition will be opened at Konigsberg on April 15 next.

The additions to the Zoological Society's Gardens during the past week include two Common Marmosets (*Hapale jacchus*) from Brazil; two Brazilian Caracaras (*Polyborus brasiliensis*) from Uruguay, presented by Mr. Donald F. Mackenzie; a Rook (*Corvus frugilegus*), a Common Magpie (*Pica caudata*), British, presented by Mr. C. L. Sutherland; a Lump Fish (*Cyclopterus lumpus*), British Seas, presented by Mr. W. K. Stanley; a Bonnet Monkey (*Macacus radiatus*) from India, deposited; a Humboldt's Saki (*Pithecia monachus*) from the Amazons, a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, two Red-vented Bulbuls (*Pycnonotus hæmorrhous*) from India, a Crested Black Eagle (*Lophoaetus occipitalis*) from West Africa, a Cirl Bunting (*Emberiza cirlus*), British, purchased; a Zebu (*Bos indicus* ♂), five Brown-tailed Gerbilles (*Gerbillus erythrurus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

Ceraski's Variable Star, U Cephei.—On comparing Dr. Julius Schmidt's observations of this star in 1882, with minima determined by the same observer in the autumn of 1880, there results a period of 2·49289 days, or 2d. 11h. 49m. 46s., on the assumption that it is regular or equable. Dr. Schmidt suspected a marked variation in the period, each successive period being 5·25 seconds longer than the preceding one. The following are the times of minima in March, which will be observable here:—

	h.	m.		h.	m.		h.	m.
March 2 ...	12	43	March 12 ...	12	2	March 22 ...	11	21
7 ...	12	22	17 ...	11	41	27 ...	11	0

The Total Solar Eclipse of 1901, May 17.—The ensuing return of the solar eclipse in May next, for the observation of which this country with France and the United States have despatched observers to the Pacific, will take place on May 17, 1901, when the duration of totality will be even longer than in the present year, and the part of our globe where observations will be most advantageously made will be rather more accessible than in the approaching eclipse. The following are approximate elements of the phenomenon:—

G.M.T. of conjunction in R.A. 1901, May 17, 17h. 28m. 14s.

	°	′	″	
Right ascension	54	15	36	
Moon's hourly motion in R.A.		39	30	
Sun's ,, ,, ...		2	29	
Moon's declination	19	1	34	N.
Sun's ,, ...	19	23	49	N.
Moon's hourly motion in decl.		5	14	N.
Sun's ,, ,, ...		0	34	N.
Moon's horizontal parallax		60	57	
Sun's ,, ,, ...		0	9	
Moon's true semi-diameter		16	36·5	
Sun's ,, ,, ...		15	48·9	

Hence the middle of the general eclipse occurs at 17h. 33m. 25s. G.M.T. The central phase commences in longitude 39° 57′ E., latitude 27° 21′ S; the eclipse is central and total with the sun on the meridian in longitude 97° 0′ E., latitude 2° 7′ S., and the central phase ends in longitude 157° 8′ E., latitude 13° 0′ S. If we calculate directly for a point in 100° 59′ E. and 1° 14′ S., which is close upon the central line and to the west coast of Sumatra, we find—

	h.	m.	s.	
Beginning of totality, May 18, at	0	22	11	Local mean time.
Ending ,, ,,	0	28	35	

Hence the duration of total phase is 6m. 24s. The sun's altitude is 68°.

The Variable Star, S Virginis.—This object, which varies between 5·7m. and 12·5m., appears to have escaped observation during the last few years. Prof. Schönfeld assigns a period of 374 days, according to which, reckoning from his tabular maximum, the last would have occurred on October 25, 1882, and if the minimum falls about 119 days before maximum, as stated by the Bonn astronomer, one will be due about July 8. This star has an intense reddish-yellow light: its position for 1883 is in R.A. 13h. 26m. 54s., N.P.D. 96° 36′.

There is a suspicion that another star in the vicinity varies through about two magnitudes, 8–10. Its place for 1883 is in R.A. 13h. 24m. 26s., N.P.D. 98° 58′.

The Binary Star, ζ Cancri.—Among several orbits recently calculated for this star by Dr. H. Seeliger, in an interesting memoir communicated to the Academy of Sciences at Vienna, the following is perhaps the most satisfactory:—

Passage of Peri-astron	1870·393
	° ′
Node	71 32
Node to Peri-astron on orbit	113 52
Inclination	10 53
Eccentricity	0·34327
Mean motion	−5°·8867
Semi-axis major	0″·8515
Period of revolution	61·154

This orbit gives for 1883·0, position 71°·1, distance 0″·88; and for 1885·0, position 61°·4, distance 0″·93.

GEOGRAPHICAL NOTES

As there seems to be some misunderstanding as to the route to be followed by Baron Nordenskjold in his Greenland Expedition, we may say that we have good reason to believe that there is no intention to proceed along the west coast to Cape York. An attempt will certainly be made to add to our knowledge of the old Danish settlements on the south and south-east coast, but the chief purpose of the expedition is to further explore the east coast, and to penetrate the interior. Baron Nordenskjold will be accompanied by a complete scientific staff; but we believe he does not intend to divulge the details of his plan till after the expedition sails. He has made a thorough study of all that is known of Greenland; among other things he has published an elaborate investigation of the voyage of the Zeni. A Danish expedition, under Lieut. Holm, will also be sent to Greenland this year; it will be away two years.

The Russian Geographical Society announces the early publication of the following works which it has already received:—A large work, by M. Mayeff, being a statistical and economical description of the Khanate of Bokhara; the report of M. Polakoff on his explorations in Sakhalin, with several maps, including the eastern coast; and a work, by M. Adrianoff, on the antiquities of the Altay and Sayan, with numerous drawings. These works will be published in the *Memoirs* of the Society, but each of them will appear separately, as soon as printed, without awaiting the completion of a volume, as was formerly the case, which caused great delay in the appearance of interesting papers.

At the last meeting of the Caucasian Geographical Society, General Stebnitsky exhibited his new orographical map of Asia Minor and adjacent countries. The map is based on measurements of heights of about 1500 places. Dr. Radde made a further communication on his great work, "Ornis Caucasica," which is the result of his many years' travels in the Caucasus, and of the description of the collection of the Tiflis Museum, which contains no less than 4000 specimens of birds.

M. Polakoff, who was sent by the Academy of Sciences for the exploration of Sakhalin Island and of the coasts of the Pacific, spent last winter and spring at Taranka, in the Gulf of Patience, and has returned to Korsakovo with rich scientific collections. He will now begin the exploration of the coasts of Russian Manchuria. One part of his report has already reached the Geographical Society.

News has been received at the Paris Geographical Society that the French had reached the banks of the Niger, Colonel Desborde having been obliged to cut his way through the Beledegou region. He fought a battle with the chief of Daba, after having crossed a stream called Baoulè. The victory was won by artillery, and the chief of Daba was killed, as well as a large number of his followers.

The Danish Ministry received on February 24 a despatch from their representative in St. Petersburg, to the effect that the Samoyedes sent out to look for the *Dijmphna* and the *Varna*, had returned on January 6 to Liapine in the Obi basin, and reported that "neither had they seen any vessel at sea, nor heard of any shipwrecked crew."

In the last part of the *Bulletin* of the Paris Geographical Society for 1882, Dr. J. Montano describes his excursion into the interior and along the coast of Mindanao; Commander Gallieni gives a detailed narration of his mission to the Upper Niger and Segou; M. Aymonier describes the result of his excursion to Central Cambodia; a paper by the late Dr. Crevaux gives the leading results of his exploration of the Yary, Paron, Iça, and Yapura; and M. Dutreuil de Rhins has a paper on the observations of the transits of Venus.

In the new number (102) of the *Zeitschrift* of the Berlin Geographical Society we have the usual annual systematic list of new works, papers, and maps in all departments of geography published during the past year, a list indispensable to geographers, and which will be found useful by students of the many departments of science related to geography. In the *Verhandlungen* (No. 1, for 1883) Prof. Foerster has a paper on the expeditions for the observation of the recent transit of Venus, and Prof. Brauns a paper on the Island of Yezo. Interesting news from the various German expeditions in Africa will be found in Heft 4 of Band iii. of the *Mittheilungen* of the German African Society, including a detailed account of Dr. Wissmann's journey across the continent, to which we referred last week. There are four letters from Herr Flegel on the progress of his Niger explorations, and several communications of great importance from the party stationed at Gonda, in East Africa, who are accumulating material of great value. They were arranging for a visit to Lake Moero according to the latest intelligence.

In a paper on the Gulf Stream in the *Bulletin* of the American Geographical Society (No. ii. 1882), Commander Bartlett gives some of the results of the examination of that current by the party in the *Blake* in the summer of 1881.

The principal paper in the February number of the *Bollettino* of the Italian Geographical Society is a narrative, with illustrations, by Lieut. Bove, of his mission to South America.

THE OPENING OF THE FINSBURY TECHNICAL COLLEGE

We have already given in our issue of February 1 (p. 318) a brief outline of the curriculum of study to be pursued at the Finsbury Technical College, in our review of the programme of instruction recently published. The new college was opened on Monday, February 19, with an address by Mr. Philip Magnus, the Principal of the College, and Director of the Institute. The address was delivered in the hall of the Cowper Street School, none of the lecture-rooms of the new college being large enough for the purpose. There were present about 1200 persons, chiefly artisans. Sir Frederick Bramwell occupied the chair, and among those on the platform were Sir Sydney Waterlow, Dr. Siemens, Professors Roscoe, Abel, Carey Foster, Adams, Ayrton, Huntington, Armstrong, and Perry, Dr. Gladstone, Mr. H. T. Wood, Mr. J. G. Fitch, Mr. Swire Smith, Mr. Matthey, Mr. Owen Roberts, Mr. John Watney.

Mr. Magnus commenced by indicating some of the incorrect ideas still prevalent on the subject of technical education. He considered that any definition ought to be expressed in very wide terms, so as to be referable to the different kinds of training to which the term technical education applies. He himself proposed to call that education, training, or instruction technical which had a direct reference to the career of the student who received it. Thus considered, technical education was no new thing, except in its reference to careers called into existence by recent developments of science. It was because the system of education to which we had been accustomed was no longer the best preparation for actual work, and not because no relation hitherto existed between the boy's training and the man's career that such colleges were needed. The necessity of technical education he attributed to the invention of the steam-engine and the breaking-up of the apprenticeship system, and the tide which was pushing it forward would not subside until it had influenced the educational institutions of the country from the primary school to the university. The Council had been guided by the desire to supplement, and not to duplicate, existing educational machinery. The college consisted really of a day school for pupils entering between the ages of fourteen and seventeen, and an evening school for apprentices, workmen, &c. The former would give preparatory training to students for practical work in the factory or engineer's shop, and the evening department was intended to help those already at work to understand the principles underlying processes they saw exemplified in their daily work. The college was therefore a technical school of the third grade, and whilst the majority of the pupils would complete within it their instruction, some would proceed to the technical high school or central institution in course of erection at South Kensington. The college might claim to represent a new grade of school. It was not an institution in which any particular trade would be taught, except it were some art industry, nor would it teach the excellence, precision, and rapidity of execution which could only be acquired in the workshop or factory, where, under the severe strain of competition, salable goods were being manufactured. Proceeding to indicate the course of instruction to be given, Mr. Magnus explained that on entering the institution, the student would generally declare whether he wished to be trained as a mechanical engineer, an electrical engineer, or with a view to some branch of chemical industry, or whether he wished to study applied art, and the subjects would be taught with special reference to the career of the student. The teacher would keep steadily in view the purpose to which the student would apply his knowledge. The work would be essentially practical, and more would be done in the laboratory than in the lecture-room, the lectures forming rather a commentary on the practical work than the practical work an illustration of the teaching of the lecture-room. The main purpose was not to turn out scientists, but to explain to those preparing for industrial work the principles that had a direct bearing on their occupation, so that they might be able to trace back the principles they saw to their causes, and thus substitute scientific method for mere rule of thumb. Of the four departments of the College—electrical engineering, mechanical engineering, chemistry, and applied art—that of electrical engineering promised to be the most attractive to students. But there was an intimate connection between the different branches of science not to be lost sight of in the training of a student in any one department. In the course of his remarks on the evening school and the curricula arranged for artisans engaged in various industries, Mr. Magnus referred very pointedly to the narrow view which adult workmen generally take of their own educational requirements. He impressed upon this class of students the necessity of acquainting themselves with branches of industry cognate to their own, and suggested that one of the objects of technical education was to correct the cramping and narrowing influences of extreme division of labour. He referred to a fact told him by a medical friend, that a student refused to dissect the abdominal cavity because, as a surgeon, he intended to occupy himself exclusively with diseases of the eye, and stated that this view of technical instruction needed to be strenuously resisted. He also insisted very strongly upon the importance of artisan students gaining a knowledge of the principles of science, as helping them to deal with unexpected and exceptional cases of difficulty certain to arise in their ordinary work. Mr. Magnus referred at some length to the methods of teaching to be adopted in the college, showing that there was no real opposition, as sometimes stated, between technical instruction, properly understood, and mental culture—that science might be so taught as to yield mental discipline, and yet at the same time have a direct reference to the career or occupation of the student. Mr. Magnus further explained the exact position which the Finsbury Technical College is intended to occupy in the Institute's general scheme of technical education. He illustrated this part of his address by a diagram showing the Bavarian school system, which he said was pronounced by many educational authorities to be the best in Germany, and the technical part of which was in many respects similar to the series of schools which the Institute is engaged in establishing. Mr. Magnus attached great importance to the Central Institution, now being erected in South Kensington, as crowning the educational ladder which pupils from the primary schools should have the opportunity of ascending, and as influencing, in the same way as the Universities at present influence, the entire system of education pursued in the series of schools leading up to them. The speaker did not omit to refer to the Applied Art Department which has recently been added to the College, and in which the instruction he said would be specialised according to the particular occupation of the student. In conclusion Mr. Magnus hoped the college would do much to wipe away the reproach of the neglect of technical education under which the country had hitherto lain compared with other countries. On the motion of Dr. Siémens, seconded by Prof. Abel, Mr. Magnus was thanked for his address. In seconding a vote of thanks to the Chairman, Alderman Sir Sydney Waterlow said their success was attributable to the generous aid of the Livery Companies, and he appealed to them to render permanent those grants hitherto given at their pleasure.

SCIENTIFIC SERIALS

The Journal of Anatomy and Physiology, vol. xvii. Part 2, January, 1883, contains:—On a method for the estimation of urea in the blood, Part 1, by Dr. J. B. Haycraft.—On the homologies of the long flexor-muscles of the feet of Mammalia, with remarks on the value of their leading modifications in classification, by Dr. G. E. Dobson (Plates 4-6).—On obliterative endarteritis and the inflammatory changes in the coats of the small vessels, by Dr. R. Saundby (Pl. 7).—The presence of a tympanum in the genus Raia, by G. B. Howes (Pl. 8).—The ligamentum teres, by J. B. Sutton (Pl. 8).—Fibrinous coagula in the left ventricle, by Dr. A. M'Aldowie (Pl. 9).—A simple method of demonstrating the nerves of the epiglottis; the trachealis muscle of man and animals; the sulphocyanides of ammonium and potassium as histological reagents, by Dr. Wm. Stirling.—A new theory as to the functions of the semicircular canals, by Dr. P. M'Bride.—Some points on the myology of the common pigeon, by W. A. Haswell, M.A.—The action of saline cathartics, by Dr. M. Hay (Pl. 10).—Some variations in the bones of the human carpus; a first dorsal vertebra with a foramen at the root of the transverse process, by Prof. W. Turner, M.B.—Multiple renal arteries, by Dr. Macalister.—Division of the scaphoid bone of the carpus, with notes on other varieties of the carpal bones, by Dr. R. J. Anderson.

Journal of the Royal Microscopical Society, December, 1882, contains:—On some organisms found in the excrements of the domestic goat and the goose, by Dr. R. L. Maddox (Pl. 7).—On a further improvement in the Groves-Williams ether-freezing microtome, by J. W. Groves.—Summary of current researches relating to zoology and botany (principally Invertebrata and Cryptogamia), microscopy, &c., including original communications from Fellows and others.—The proceedings of the Society.

February, 1883, contains:—Observations on the anatomy of the Oribatidæ, by Dr. A. D. Michel (plates 1 and 2).—On a minute form of parasitical Protophyte, by G. F. Dowdeswell, M.A.—On the use of incandescence lamps, as accessories to the microscope, by H. C. Stearn, with figure—and the usual summary of current researches relating to botany and zoology.

Revue internationale des Sciences, December 15, 1882, contains:—On the Nofoures of New Guinea, by Élie Reclus.—On movements and sensibility in plants (finis), by J. L. de Lanessan.—Reviews.—Notices of learned Societies: the Academy of Sciences, Paris; the Academy of Sciences, Amsterdam.

January 15, 1883, contains:—On the localisation of the cerebral functions in the cerebral hemispheres in man and animals, by Julius Nathan.—On the development of colours in flowers, by H. Müller.—On cell-division or cytodieresis, by L. F. Henneguy.—On the vaginal stopper in rodents, by Dr. Lataste.—On the adulterations in provisions in Paris, by M. Egasse.

Zeitschrift fur wissenschaftliche Zoologie, Bd. 37, Heft 4, December 22, 1882, contains:—On the Coelenterata of the South Sea, No. 1.—On *Cyanea aunaskala*, nov. sp., by Dr. R. v. Lendenfeld, of Melbourne (Plates 27 to 33; Pl. 27, a coloured representation of the new species).—Contribution to the anatomy, developmental history, and general biology of *Trombidium fuliginosum*, Herm., by H. Henking (Plates 34-36).—On some facts in the life-history of freshwater polyps, and on a new form of *Hydra viridis*, by Wm. Marshall, of Leipsig (Pl. 37).—Supplementary remarks on Dinophilus, by Dr. E. Korschelt.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 22.—"On the Effects of Temperature on the Electromotive Force and Resistance of Batteries," by W. H. Preece, F.R.S.

That heat has considerable influence on the condition of galvanic elements is well known, and it has been investigated by De la Rive, Faraday, Daniell, and many others. Some attribute the result to increased chemical affinity, and others to increased conductivity of the liquid, but no one has eliminated the effect on electromotive force from that on internal resistance with the view of expressing each in definite measurement. This the author has done. Special apparatus was made, so as to vary the temperature, and a very careful series of experiments were made upon Daniell, Leclanché, and bichromate of potash cells, measuring the electromotive force and resistance at each change of temperature in rising and falling between 0° and 100° C. The results are tabulated and plotted out as diagrams.

The conclusions are (1) that the E.M.F. is not materially affected by changes of temperature; (2) that the internal resistance is affected very materially according to a fixed law that apparently varies with every cell. A Daniell's cell at 100° C. has only one-third the resistance it has at 0° C. Between 10° and 20° C. it falls one half. Bichromate and Leclanché cells, though much reduced, are not reduced to the same extent; (3) when a liquid is warmed up, its resistance at the same temperature in cooling is greater than when it was being warmed up, and it takes a very long time (fifty hours) to recover its normal condition.

Chemical Society, February 15.—Dr. Gilbert, president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting (March 1).—The following proposed changes in the list of officers were also announced:—Prof. G. D. Liveing and Dr. A. Voelcker as vice-presidents instead of Professors J. Dewar and A. V. Harcourt; Prof. Dittmar, Dr. W. R. E. Hodgkinson, Messrs. P. D. Howard, and R. Meldola as members of Council instead of Dr. T. E. Thorpe, and Messrs. F. D. Brown, J. M. Thomson, and W. Thorp.—The following papers were read:—On some derivatives of diphenylene ketone oxide, by A. G. Perkin. During the preparation of this substance from salicylic acid and acetic anhydride, a body was noticed which was separated out as transparent, satiny plates containing 75·2 per cent. carbon, and 4 per cent. hydrogen. The author has also investigated the action of nitric acid, of bromine, and of sulphuric acid on the above substance.—On α-ethyl valerolacton, α-ethyl β-methyl valerolacton, and on a remarkable decomposition of β-ethyl aceto-succinic ether, by S. Young.

Anthropological Institute, February 13.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. Colquhoun read a paper on the aboriginal and other tribes of Yünnan and the Shan country. Mr. Colquhoun first dwelt upon the races of the South China borderlands. Between Canton and Nan-ning (one of the important towns on the Si-Kiang in Kwang-si), the inhabitants met with were pure Chinese. West of that to the Yünnan frontier, a mixed population on the river and aboriginal tribes in the interior were found. Throughout Yünnan the chief population consisted of Shans disguised under a great variety of tribal names. Lo-lo and Miao-tzŭ aborigines were met with, as well as Thibetans under the name of Kutsung. On the west side of Yünnan Mahomedans are numerous, presumably the remains of the armies of Ginghis Khan. The costumes are most varied and picturesque, and the Shans and all the aboriginal people were kind, frank, and hospitable, and in these respects and in their feet being uncrushed offer a great contrast to the Chinese. Besides the tribes met with, Mr. Colquhoun pointed out that there were in the north and north-west Yünnan, as well as in Ssŭ-chuan, four divisions, namely Li-ssŭ, Moro, Sifan, and Mantzŭ. A great similarity of language exists between the Lo-lo, Li-ssŭ, Sifan, and Burmese. The large area over which the Shan population is distributed was pointed out, and the habitat of the Karens and Lawas. The paper was illustrated by part of a collection of admirable photographs and sketches made during Mr. Colquhoun's late exploration, exhibited by means of the oxhydrogen light. These form a portion of the illustrations which will appear in Mr. Colquhoun's forthcoming account of his late journey.

Geological Society, February 16th, Annual General Meeting.—J. W. Hulke, F.R.S., president, in the Chair.—The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1882. The Council expressed their regret that, owing probably to the same causes as last year, they could announce no material advance in the prosperity of the Society, although its financial position was well maintained, the balance at the close of 1882 showing an increase over that of the previous year, notwithstanding a large expenditure upon the *Quarterly Journal*. The total number of Fellows was diminished by one, but there was an increase of nine in the number of contributing

Fellows. The Council stated that Mr. Ormerod had furnished a second Supplement to his Classified Index to the publications of the Society, bringing that work down to the end of 1882. The Council's Report further announced the awards of the various Medals and of the proceeds of the Donation Funds in the gift of the Society.

In presenting the Wollaston Gold Medal to Mr. W. T. Blanford, F.R.S., F.G.S., the President addressed him as follows: "Mr. Blanford,—The Council has awarded you its highest distinction, the Wollaston Medal, in recognition of your services to geology in Abyssinia, in Persia, and on the Geological Survey of the Indian Empire. They are so well and so generally known that it is not necessary for me to enlarge upon them here. Your writings, which treat of a not inconsiderable portion of the Eastern Hemisphere, comprise, in addition to geology, much information respecting zoology and the climates of the countries in which you served. Stamped with thoroughness and comprehensiveness, they constitute important additions to our knowledge of those regions. In conferring upon you this distinction, the Council of the Geological Society desires to mark its sense of their great value."

The President then handed the balance of the proceeds of the Wollaston Donation Fund to Prof. J. W. Judd, F.R.S., for transmission to Prof. John Milne, F.G.S., of Tokio, Japan, and addressed him as follows: "Prof. Judd,—The Council, in bestowing upon Mr. Milne the balance of the proceeds of the Wollaston Fund, wishes to mark its appreciation of the importance of his investigations into the phenomena of earthquakes, to which he has devoted so much time and attention during his residence in Japan. In handing to you this cheque for transmission to him, I would ask you to convey to him the hopes of the Council that this award may assist him in continuing those inquiries in Seismology which he has proved himself so well able to undertake."

In handing the Murchison Medal to Mr. Warington W. Smyth, F.R.S., for transmission to Prof. Heinrich Robert Goppert, F.M.G.S., of Breslau, the President said: "Mr. Warington Smyth,—The Council of the Geological Society has awarded one of its high distinctions, the Murchison Medal and a part of the proceeds of the Murchison Fund, to Prof. H. R. Goppert of Breslau, one of our Foreign Members, in recognition of his labours in fossil botany. The very large number of papers, 245, recorded in the Scientific List of the Royal Society under Prof. Göppert's name, testifies to the zeal and success with which he has cultivated this branch of biology during half a century. In asking you to transmit to him this Medal, I would desire you to express to him the high estimation in which this Society holds his work."

The President then handed to Prof. Morris, F.G.S., for transmission to Mr. John Young, F.G.S., the balance of the proceeds of the Murchison Donation Fund, and said: "Professor Morris,—The Council of the Geological Society, in awarding to Mr. John Young, of the Hunterian Museum, Glasgow, the balance of the proceeds of the Murchison Donation Fund, wishes to mark its appreciation of the value of his long-continued researches on the fossil polyzoa, especially those of the western part of Scotland, and of his investigations into the structure of the shells of the Carboniferous Brachiopoda. In his absence, I have much pleasure in placing the amount in your hands for transmission to him."

The President next presented the Lyell Medal to Dr. W. B. Carpenter, F.R.S., and addressed him in the following words: "Dr. Carpenter,—The Council of the Geological Society has awarded to you the Lyell Medal with (in compliance with the terms of the bequest) a portion of the proceeds of the Lyell Fund, in recognition of the great value of your investigations into the minute structure of invertebrate fossils and your deep-sea researches. Your contributions 'On the Structure and Affinities of the Eozoon Canadense,' 'On the Microscopic Structure of Nummulina, Orbitolites, and Orbitoides,' published in our *Journal*, your numerous papers on the intimate structure of shells, communicated to the Royal Society, and others published in the 'Annals and Magazine of Natural History,' your long-continued work on Foraminifera, your communications on Oceanic Circulation and on Abyssal Life-forms, all testify to a life-long devotion to branches of natural knowledge bearing on that department of science, the cultivation of which is the *raison d'être* of this Society. I count it a pleasure, Dr. Carpenter, that it has devolved upon me to hand you this Medal."

In presenting one moiety of balance of the Lyell Donation Fund to Mr. P. Herbert Carpenter, the President addressed him as follows: "Mr. P. Herbert Carpenter,—The Council of the Geological Society, in awarding to you a portion of the balance of the proceeds of the Lyell Donation Fund, desires to express its sense of the great value of your researches into the structure and relationship of several families of fossil Echinodermata. Your papers 'On some little-known Jurassic Crinoids,' 'On the Cretaceous Comatulæ,' 'On the Crinoids from the Upper Chalk,' and that read last session, 'On Hybocrinus, Baerocrinus, and Hybocystites,' are models of clearness and an excellent earnest of future work. The Council hopes that this award may aid you in continuing those lines of research in which you have already achieved signal success."

The President then handed the second moiety of the balance of the Lyell Donation Fund to Prof. Seeley, F.R.S., for transmission to M. E. Rigaux of Boulogne, and said: "Professor Seeley,—In conferring upon M. Rigaux a portion of the balance of the proceeds of the Lyell Donation Fund, the Council of the Geological Society desires to signify its estimation of the value it places on his researches in the Jurassic formations of the Boulonnais and their contained fossils. In asking you to transmit to him this cheque, I would desire you to convey to him with it our hopes that he may continue those lines of inquiry in prosecuting which he has attained so great success."

The President finally presented the Bigsby Gold Medal to Dr. Henry Hicks, F.G.S., and addressed him in the following words: "Dr. Hicks,—The Council, in conferring on you the Bigsby Medal as a mark of their appreciation of your labours amongst the oldest fossiliferous and the Archæan rocks of Great Britain and Ireland, feels, in your community of interests, a peculiar fitness in associating you with the memory of the founder of this distinction. Your numerous communications, beginning with one 'On the genus *Anopolenus*,' written in 1865, and culminating in that which you read at our last meeting, show to what good purpose you have employed the *horæ subsecivæ* of a busy professional life in prosecuting those researches which have had a distinct effect on geological thought. In handing to you this Medal, I would express the wish that you will continue to prosecute the line of inquiry to which you have so long and so successfully devoted your leisure hours."

The President then read his Anniversary Address, in which he passed in review the work done by the Geological Society during the past year, and discussed at considerable length a question arising out of this review, namely, the structural characters presented by the sternal framework and the limbs of Enaliosaurians, and the classificational value which they possess. He also referred to the discoveries which have been lately made in America of numerous remains of Pterosaurians, often of gigantic size; adverted to the proceedings of the International Geological Congress, held in 1881, at Bologna, and noticed, as one gratifying result of the latter, the establishment of an Italian Geological Society.

The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President: J. W. Hulke, F.R.S. Vice-Presidents: Prof. P. M. Duncan, F.R.S.; R. Etheridge, F.R.S.; J. Gwyn Jeffreys, F.R.S.; Prof. J. Prestwich, F.R.S. Secretaries: Prof. T. G. Bonney, F.R.S.; Prof. J. W. Judd, F.R.S. Foreign Secretary: Warington W. Smyth, F.R.S. Treasurer: Prof. T. Wiltshire, F.L.S. Council: H. Bauerman; W. T. Blandford, F.R.S.; Prof. T. G. Bonney, F.R.S.; W. Carruthers, F.R.S.; Prof. P. M. Duncan, F.R.S.; R. Etheridge, F.R.S.; John Evans, F.R.S.; A. Geikie, F.R.S.; Rev. Edwin Hill, M.A.; G. J. Hinde, Ph.D.; Prof. T. M'Kenny Hughes, M.A.; J. W. Hulke, F.R.S.; J. Gwyn Jeffreys, F.R.S.; Prof. T. Rupert Jones, F.R.S.; Prof. J. W. Judd, F.R.S.; S. R. Pattison; J. A. Phillips, F.R.S.; Prof. J. Prestwich, F.R.S.; F. W. Rudler; Prof. H. G. Seeley, F.R.S.; Warington W. Smyth, F.R.S.; W. Topley; Prof. T. Wiltshire, F.L.S.

Physical Society, February 10.—Prof. Fuller in the chair.—Annual general meeting.—New officers elected for the year:—President: Prof. R. B. Clifton, M.A., F.R.S. Vice-presidents: Sir W. Thomson, Prof. G. C. Foster, F.R.S., Dr. T. Hopkinson, F.R.S., Lord Rayleigh, F.R.S., Prof. W. C. Roberts, F.R.S. Secretaries: Prof. A. W. Reinold, M.A., Mr. Walter Baily, M.A. Treasurer: Dr. E. Atkinson. Demonstrator: Prof. F. Guthrie, F.R.S. Other Members of Council: Prof. W. G. Adams, M.A., F.R.S., Prof. W. E. Ayrton, F.R.S., Mr. Shelford Bidwell, M.A., LL.B., Mr. W. H. M. Christie, M.A., F.R.S., Prof. F. Fuller, M.A., Mr. R. T. Glazebrook, M.A., F.R.S., Mr. R. J. Lecky, F.R.A.S., Prof. O. J. Lodge, D.Sc., Mr. Hugo Müller, Ph.D., F.R.S., Prof. J. Perry. New Member: Prof. Blyth of Anderson College, Glasgow.—Prof. Sylvanus P. Thomson explained his new graphical method of showing Jacobi's law of maximum rate of working, and Siemens's law of efficiency for dynamo-electric machines. This has been fully explained in the *Philosophical Magazine* and in the Cantor lectures on Dynamo-electric Machinery, delivered by Prof. Thomson. Prof. W. G. Adams pointed out the advantages of a graphic system of the kind.

The Institution of Civil Engineers.—February 20, Mr. Brunlees, president, in the chair. The paper read was on "Covered Service-Reservoirs," by Mr. William Morris, M. Inst. C.E. (of Deptford).

EDINBURGH

Royal Society, February 5.—Prof. Jenkin, F.R.S., vice-president, in the chair.—Emeritus Professor Blackie, in a paper on scientific method in the study of language, maintained that the true way to learn a foreign language was to learn it in the way a child learns its native language—conversationally; and that this method should be adopted for the teaching of the dead languages as well as for modern ones. Simple sentences expressing facts with which the pupil is in direct contact, the grammatical rule for construction being given after the construction is practically mastered by repetition, should lead by insensible gradations to more complicated sentences and ideas. The paper finished with some characteristic remarks about quantity and accent in Latin and Greek, which called forth criticism from Prof. Butcher and Mr. Marshall, Rector of the High School.—Prof. Tait, in a short note on the mirage problem, mentioned that he had come across a paper in Gorgonne's *Annalen* criticising Biot's great paper upon the subject. Thinking that possibly he might have been forestalled in some of his theorems, he had looked into the paper, the author of which, however, in attacking Biot, had given a construction which, if applied to the case of ordinary desert-mirage, would give a direct instead of an inverted image. Mr. Sang, in his criticisms on the paper, maintained that such mirage as was said to have been observed by Vince was impossible.

PARIS

Academy of Sciences, February 19.—M. Blanchard in the chair.—The following papers were read:—Observations of small planets, made with the large meridian instrument of the Paris Observatory, during the fourth quarter of 1882, by M. Mouchez.—Results of experiments made in the workshops of the Chemin de fer du Nord, on M. Deprez's electric transport of work to a great distance, by M. Tresca (See p. 399).—Note on the theorem of Legendre cited in a note inserted in *Comptes rendus*, by Prof. Sylvester.—Report on a memoir of M. Rosenstiehl, entitled "Researches on the Colouring-matters of Madder," by M. Wurtz. *Inter alia*, M. Rosenstiehl has found a new mode of formation of purpurine (decomposition of pseudopurpurine by heating with alcohol at 40°), and his discovery of the composition of pseudopurpurine (which is really a trioxy-carboxyl-anthraquinone) throws much light on several facts that were obscure. Madder contains only three glucosides, giving respectively pseudopurpurine, carboxyl-alizaric acid, and munjistine, or carboxyl-xanthopurpuric acid.—M. Hospitalier presented a note on the influence of the mode of coupling of dynamo-electric machines in experiments on transport of force to a distance.—Observations of the new planet (232) Palisa, made at the Paris Observatory, by M. Bigourdan.—Observations of the great comet *b* 1882, made with the Brunner equatorial of Toulouse Observatory, by M. Baillaud.—On a curious modification of the nucleus of the great comet, by M. de Oliveira Lacaille. On the evening of January 8 the nucleus was seen to be much elongated and subdivided into four small nebulosities in a line, with centres like stars of the 12th magnitude. At 9.30 a.m. next day there was a change in the relative position: the first nebulosity being more separated, and the second having taken its place, &c.—On the observation of the transit of Venus of 1882 at the Lick Observatory on Mount Hamilton, California, by Mr. Todd. He got 147 photographs, of which 125 are well fitted for micrometric measurement.—On the uniform functions of a variable connected by an algebraic relation, by M. Picard.—On the relations between co-variants, &c. (continued), by M. Perrin.—On the functions of several imaginary variables (continued), by M. Combescure.—On a question of divisibility, by M. de Polignac.—On the equilibrium of the elastic cylinder, by M. Schiff.—On crystals observed in the interior of a bar of cemented Swedish iron, by M. Stoltzer. These crystals of steel are not regular octahedra like those of pig-iron and iron.—On the immediate analysis of pozzuolanas, and on a rapid process for testing their hydraulic properties, by M. Landrin. The rapid process is attack with hydrochloric acid, and trial of the insolubles with lime-water. There is no possible comparison between the action of pozzuolanas and of their insolubles on lime-water.—On sulphocyanopropionine, by MM. Tcherniac and Norton.—On allotropic arsenic, by M. Engel. When arsenic is isolated by the wet or dry way under about 360°, it is amorphous, dark grey, brown, or black, and unalterable in moist air; and its density is between 4·6 and 4·7. Heated to 360°, it changes into arsenic with a density of 5·7,—the steel-grey arsenic of laboratories, which crystallises when formed from condensation of arsenic vapour about 360° or more.—On benzoyl-mesitylene, by M. Louise.—Researches on mesitylene, by M. Robinet.—Toxical power of quinine and of cinchonine, by M. Bochefontaine. The former has more active physiological properties than the latter. Both are convulsive, cinchonine more than quinine, and quinine is distinguished by its vomitive effects and depressing action on the central nervous system.—On the value of intercrossing of the movements of cerebral origin, by M. Couty. This intercrossing is not constant, and has not the value that has been attributed to it.—Vision of ultra-violet radiations, by M. de Chardonnet. The spectrum of the crystalline lens corresponds exactly to that of the visible spectrum. From observations of persons with the lens removed, the author concludes that the retina is sensitive to ultra-violet radiations that come to it (as well as visible radiations), at least to about the line S. Thus the crystalline lens alone limits the visible spectrum. The absorption of the long ultra-solar spectrum of the electric arc probably fatigues the eye.—Researches on the production of monstrosities by shocks imparted to hens' eggs, by M. Dareste. He produced tremors, and so monstrosities, by means of a beating apparatus used by chocolate-makers.—On the generation of cells of renewal of the epidermis and of epithelial products, by M. Retterer.—On M. Merejkowski's Suctociliates (second note), by M. Maupas.—On the structure of simple subterranean branches of adult Psilotum, by M. Bertrand.—On the conservation of solar energy, by M. Duponchel. He infers from peculiar circumstances of our epoch that the sun-spot period which has varied in the neighbourhood of ten years for 130 years, will be extended to fourteen for the present and the two following periods. The first maximum will be in 1885.—Imitation of diffraction-spectra by dispersion, by M. Zenger.—The second part of M. Grüner's geological description of the coal basin of the Loire was presented (with analysis) by M. Daubrée.

CONTENTS

PAGE
RECENT ARMOUR-PLATE EXPERIMENTS 405
SMOKE ABATEMENT. By Dr. E. FRANKLAND, F.R.S. 407
NORTH AFRICAN ETHNOLOGY. By A. H. KEANE 408
OUR BOOK SHELF:—
  "The Electric Lighting Act, 1882" 410
LETTERS TO THE EDITOR:—
  Ben Nevis Observatory.—DAVID MILNE HOME 411
  Indian Archegosaurus.—RICHARD LYDEKKER 411
  The "Vampire Bat."—THOS WORKMAN; A. W. AUDEN; GEORGE J. ROMANES, F.R.S. 411
  Hovering (? Poising) of Birds.—Dr. HUBERT AIRY; Rev. W. CLEMENT LEY 412
  The Auroral "Meteoric Phenomena" of November 17, 1882.—T. W. BACKHOUSE; ALFRED BATSON 412
  Aurora.—F. B. E. 413
DIURNAL VARIATION OF THE VELOCITY OF THE WIND ON THE OPEN SEA, AND NEAR AND ON LAND. By ALEXANDER BUCHAN (*With Diagram*) 413
EPHEMERIS OF THE GREAT COMET, *b* 1882. By Prof. E. FRISBY . . 415
ILLUSTRATIONS OF NEW OR RARE ANIMALS IN THE ZOOLOGICAL SOCIETY'S LIVING COLLECTION, XI. 415
THE ELECTRIC LIGHT AT THE SAVOY THEATRE 418
ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT. By Dr. T. LAUDER BRUNTON, F.R.S. (*With Diagrams*) . . 419
NOTES 422
OUR ASTRONOMICAL COLUMN:—
  Ceraski's Variable Star, U Cephei 424
  The Total Solar Eclipse of 1901, May 17 424
  The Variable Star, S Virginis 424
  The Binary Star, ζ Cancri 424
GEOGRAPHICAL NOTES 424
THE OPENING OF THE FINSBURY TECHNICAL COLLEGE . . . 425
SCIENTIFIC SERIALS 426
SOCIETIES AND ACADEMIES 426

THURSDAY, MARCH 8, 1883

THE ORIGIN OF CULTIVATED PLANTS

Origine des Plantes cultivées. Par Alph. de Candolle (Paris: Germer, Baillière et Cie., 1883.)

Les Plantes potagères, Description et Culture des principaux Légumes des Climats tempérés. Par Vilmorin-Andrieux et Cie. (Paris: Vilmorin-Andrieux et Cie., 1883.)

ALPHONSE DE CANDOLLE occupies a position in the botanical world which in its way is unique. He is in a manner the *doyen* amongst the heads of the botanical establishments of different countries which have for their especial object the study of the earth's vegetation in its taxonomic aspect. There is a special appropriateness in his being so; the Geneva botanical school, though in filiation related to the French, has always seemed to belong more to Europe than to Switzerland. The effect of this circumstance has doubtless operated indirectly on a mind naturally inclined to wide and general views. Accordingly as the invaluable "Prodromus"—the only modern work which has attempted to describe all known species of flowering plants—drew near the point at which it was decided to conclude it after occupying two generations of botanists, we find De Candolle himself more and more engaged with works dealing with general questions—works which both temperament and point of view peculiarly fitted him to undertake. Such were his "Histoire des Sciences et des Savants depuis deux Siècles," published in 1873, and his "Phytographie; ou, L'Art de décrire les Végétaux," published more recently (1880).

Long ago however, in 1855, he had published his classical "Géographie botanique raisonnée," and in this he had stated the theory, sufficiently novel then, though now a commonplace, that the present distribution of the earth's flora cannot be accounted for by any possibility as the result of the existing configuration of its surface, but is the gradual result of long antecedent geological changes. De Candolle's conclusions are now seen to form a particular case in the general theory of evolution. But we must not forget that they were the independent result of a long and laborious induction.

The study of geographical distribution requires the elimination from the facts of all disturbing elements. It is necessary to ascertain the precise nature of the flora of any given district undisturbed by artificial modifications. The slow action of natural forces is one thing: the changes brought about by man are another. De Candolle was therefore obliged to devote no small attention to the question of introduced and cultivated plants. They must clearly be eliminated from the enumeration of feral productions. But the question then arises, What is to be done with them, and to the flora of what country are they to be relegated? The result is not merely one of disembarrassment to the botanist; it has its interest no less for anthropology in the widest sense. Different races have taken advantage of plants susceptible of cultivation in the places where nature had originally planted them; and as these races have migrated they have taken their cultures with them. If the botanist then does his work properly in tracking them back to their original cradle, he is tracking back the migratory race as well, and doing the work of the anthropologist. Plants often take their old names with them, and these may and frequently have persisted when the race that brought them has passed away, been dispersed, or changed its language. All the various names, for example, given to hemp by the descendants of the Aryan race go back to the same root.

These considerations will be sufficient to establish the utility of the study which De Candolle has had in hand for some thirty years, and with regard to which he has now given us, in a singularly succinct form, probably as much as we are ever likely to know. Hitherto we have been badly off for a handy synopsis of the subject. It is true we have the chapter in De Candolle's work already referred to, and Mr. Darwin brought together a considerable body of information in his "Variation of Animals and Plants under Domestication." The former book has, however, long been out of print, and Mr. Darwin's purpose only led him to deal with those species which have largely varied under cultivation. For my own part I have generally used for reference the two admirable articles in the ninth volume of the *Journal* of the Horticultural Society—a body which unhappily, while taking the title of Royal seems to have lost its taste for such studies. These articles—in form a review of a little work by Targioni-Tozzetti, of which I have never seen but a single copy—are really an extremely useful examination of the whole subject; and as it is an open secret that they are from the pen of Mr. Bentham, the critical opinions they contain as to the origin of all our more important cultivated plants may be relied upon with considerable confidence.

"An English vegetable garden," says Mr. Tylor, "is a curious study for the botanist, who assigns to each plant its proper home; and to the philologist, who traces its name." But De Candolle, not confining himself to our temperate pot-herbs, has included in his studies the cultivated plants of all countries. Accurate knowledge in this matter is a thing of comparatively recent growth. Linnæus bestowed no pains upon it. Humboldt in 1807 dismissed it as an impenetrable secret. De Candolle has now discussed no less than 247 species. It is curious—perhaps significant—to note that 199 of these trace back to the Old World; only 45 are American, and 3 doubtful. Neither the tropical nor the southern regions of either hemisphere have any of these species in common. The northern have five which are so, but it goes with the rest of the facts that the domestication of these belongs to the Old World, and to this De Candolle has accordingly credited them. Some things no doubt have escaped him, although the list is remarkably complete. Perhaps the most curious omission is rhubarb, the use of which for the table seems pretty much confined to England and Holland.

It is rather to be regretted that De Candolle has abandoned the attempt to indicate the points on the earth's surface from which the maximum number of cultivated plants appear to have sprung. He contents himself with saying that the original distribution of the stocks of cultivated plants is most irregular. "It had no relation with the needs of man supplied nor with the area of origin." I have a decided suspicion that the facts might be made to yield a different result. There does not seem any *a priori* reason why plants susceptible of useful development under cultivation should be so arbitrarily distributed. The number of species domesticated in a given area would, other things being equal, seem to be related to the intelligence of the races working on them. North America has only given us the vegetable marrow and the Jerusalem artichoke; and neither deserve more than a *succès d'estime*. But our best domesticated plants have developed their merits *pari passu* with the races that educated them. If we stumbled *now* against the primitive stocks they might seem as little susceptible of development as the plants of the United States, whose capabilities we rank so low. But had the Old World races been but early enough on the New World soil to work out their progress to civilisation, possibly the balance in the proportion of domesticated plants would have been redressed. If the gardens of the United States are filled with Old World vegetables, the houses are inhabited by an Old World stock. The two things seem to me to go together; the indigenous races could neither develop their latent vegetables nor hold their own against an Old World human invasion.

The circumstances of domestication, however, impose certain conditions which the flora drawn upon must fulfil The early stages of civilisation were probably unsuited to any fixity of abode. Tylor, it is true, remarks that "even very rude people mostly plant a little." But they will plant only what will give a quick return, and the qualities of foresight as well as a permanent social structure must be developed before men would have the disposition to plant fruit trees, which perhaps only their descendants would gather from. The first domesticated plants must have been those that were in themselves succulent, or would in the course of a single season yield some desired product. We find then that out of the 44 species, the cultivation of which in the Old World goes back to the dawn of civilisation, half are annuals; and these are just what the great temperate flora of the northern hemisphere would supply. On the other hand, Patagonia and South Africa have not yielded a single domesticated plant. Australia only contributes the overrated *Eucalyptus globulus*, and New Zealand a wretched spinach (*Tetragonia*). But then, as De Candolle remarks, their floras are destitute of the types of *Gramineæ*, *Leguminosæ*, and *Cruciferæ*, which were available in the northern hemisphere, and predominate in the list of the 44 most anciently cultivated plants. As between the north and the south I think this argument is valid. But as between the east and the west in the north hemisphere, since the main features of the flora are radically the same, any similar explanation does not hold.

With regard to such of these primitive cultures as belong to the temperate regions of the Old World, it will be interesting to give De Candolle's conclusions. The turnip and rapeseed (not however sustainable as distinct species) originated in Northern Europe. The cabbage was derived from the western coasts of Europe, where its wild stock may still be found; it was first gathered and then cultivated by pre-Aryan races. Purslane is wild from the Western Himalayas to Greece. The onion was brought from Western Asia. As to textiles, the origin of flax is somewhat complicated. The inhabitants of the Swiss lake-dwellings of the Stone Age did not use our present annual flax but a subperennial sort indigenous to Southern Europe (*Linum angustifolium*). This was displaced by *Linum usitatissimum*, a native of countries south of the Caspian, which was introduced into Europe and India by Aryan races. The knowledge of hemp seems to have been brought into Europe by the Scythians about 1500 B.C.; there is no trace of it in the Swiss lake-dwellings. The vine is indigenous in Western Asia, whence its use was carried to various countries by both Aryan and Semitic races; but it did not reach China before 122 B.C.

The almond, although so characteristic of Mediterranean countries, seems to be a native of Western Asia, and perhaps Greece. As late as the time of Pliny the fruits were known to the Romans as *Nuces græcæ*. The wild stocks of our pears and apples seem to have been indigenous to Southern Europe and Western Asia before the Aryan invasion; their remains abound in the Swiss lake-dwellings. The quince is a native of North Persia, but seems to have been introduced into Eastern Europe in pre-Hellenic times. Remains of a form of the pomegranate have been found in strata of the Pleiocene age in Southern France by Saporta; but it died out and was reintroduced from countries adjoining Persia in prehistoric times into the Mediterranean region of which it is now so characteristic a feature. The primitive home of the olive was apparently the eastern shores of the Mediterranean, where the Greeks discovered its useful qualities the Romans learning them later. The fig has left its remains in quaternary rocks in France along with the teeth of *Elephas primigenius*, but its prehistoric home must be sought in the Southern Mediterranean shores and lands, where it survived after probably perishing in France. The common bean (*Faba vulgaris*) seems to have become extinct in a wild state; it may have originated south of the Caspian, and was introduced into Europe by the Aryans. The remains of lentils have been found in lake-dwellings of the Bronze Age, and it was probably indigenous in Western Asia, Greece, and Italy before its cultivation in these countries; subsequently it was introduced into Egypt. The chick-pea was carried from the south of the Caucasus by the Aryans to India and Europe. The carob is indigenous to the Eastern Mediterranean, whence the Greeks introduced it into Italy and the Arabs into Western Europe. De Candolle regards all the various kinds of wheat as derivatives of the small-grained kind found in the most ancient lake-dwellings of Western Switzerland. He inclines to the belief that the wild stock of this originated in Mesopotamia, where it may still exist. The origin of spelt is very doubtful, and it may possibly be an ancient cultivated derivative from the wheat stock. As to barley, the inhabitants of the Swiss lake-dwellings cultivated both the two-rowed and the six-rowed kinds. The former is found spontaneously in the area between the Red Sea and the Caspian; but nothing is known of the spontaneous occurrence of the latter or of the four-rowed kind. Either then both were derivatives in prehistoric times of the two-rowed variety, or they are the cultivated representatives of species which have since become extinct. As to rye, probability points to an origin in South-Eastern Europe. The lake-dwellers even of the age of Bronze did not know it, but Pliny mentions its cultivation near Turin. De Candolle supposes that the Aryan migrations westward met with it in Europe and carried it onward. Oats seem also to have originated in Eastern Europe; they are found not earlier than the Bronze Age in Switzerland. From Pliny's mention that the Germans used oatmeal, it is concluded that it was not cultivated by the Romans.

Space will not allow of my giving an idea of the method by which these results are arrived at. But they seem to me to take advantage of every line of evidence and to be as near the truth as we are at present likely to approach. De Candolle sums up with great pains the philological evidence which he has collected from the best quarters, and though, as he is prepared to admit, a professed philologist might handle the evidence in a different way, he claims that the inferences he draws are such as are fairly within the competence of instructed common sense. And controlled as they are by other lines of inquiry they do not seem to me to be pushed to a point where, except in the hands of an expert, they would be likely to prove treacherous. It is obvious that the philological evidence alone might make the most careful go astray. The two instances given by Tylor are, in a way, a case in point. "Sometimes," he remarks, "this (the name) tells its story fairly, as where damson and peach describe these fruits as brought from Damascus and Persia." This is true perhaps as far as it goes. The cultivated plums of Damascus had a reputation in the time of Pliny; but the wild stock does not extend to the Lebanon, and its home was probably far to the north in Anatolia and Northern Persia. As to the peach, De Candolle points out that its having no Sanscrit or Hebrew name is against an origin in Western Asia, and he gives a considerable body of evidence pointing to China as its true native country.

It is in fact the indirect evidence given by such names through their origin and history which is of use, not the actual information they imply. The Jerusalem artichoke is a well-known instance. As De Candolle says, it is not an artichoke, and being a North American plant can have nothing to do with the Holy Land. The plant is technically a sun-flower (*Helianthus*), though in our climate it rarely betrays its affinity by flowering. And the ordinary explanation is that Jerusalem is a corruption of *Girasole*. But this seems to be a wanton piece of euhemerism; there is no evidence that the Italians ever used such a name for it, and the real explanation seems to be that Jerusalem was applied in a vague way, like Indian or Welsh, simply to indicate a foreign origin. Thus an old-fashioned garden plant (*Phlomis fruticosa*) is called sage of Jerusalem with about as little reason. And in France the term Jerusalem artichoke is applied to a species of gourd.

The second work which I have cited at the head of this article deserves a more extended notice than it is possible to give it. It is safe to say that only in France could such a book be produced, either as regards the share of authors or publishers. It is a sort of complement to De Candolle's treatise, describing, from the cultural point of view (but with botanical references apparently carefully accurate), the various esculent vegetables which are known, with all their typical varieties, including even such sorts as are rarely seen in the gardens of cool countries. It is a book which any botanist will find a useful addition to his library, if only for the delicate illustrations, which contrast so strikingly with the coarse ostentation of the ordinary trade catalogue.

W. T. T. D.

OUR BOOK SHELF

Useful Rules and Tables relating to Mensuration, Engineering, Structures, and Materials. By William John Macquorn Rankine, C.E., F.R.S., &c. Sixth Edition, thoroughly Revised by W. J. Millar, C.E. With Appendix, Tables, Tests, and Formulæ for the use of Electrical Engineers, by Andrew Jamieson, C.E., F.R.S.E. (London: Charles Griffin and Co., 1883.)

We learn from the title-page of this edition of Rankine's well-known book of Rules and Tables that it has been thoroughly revised, but we are sorry to find so little evidence of this in the work itself. Many of the rules given in this book are only applicable in particular cases, and generally no explanation as to this is given. Take for example Rule XXV., p. 211, which gives the collapsing pressure of tubes as $\frac{9672000 \times \text{thickness}^2}{\text{length} \times \text{diameter}}$.

This rule is evidently based on Fairbairn's experiments, which were made on tubes with closed ends and of lengths in no case exceeding ten times the diameter. The rule as it stands is simply absurd, for it gives zero collapsing pressure for infinite length. The addendum to Part II. p. 367, referring to springs, may be taken as another example. Straight springs cannot be treated by the formulæ given for beams unless they are only slightly bent, and again, the ratio of the force to the elongation produced by it in a spiral spring is that given by the formula only when the spires have small inclination to a plane at right angles to the axis. This formula is not even accurate, as the r^3 in the denominator should be r^4. Some estimate of the care bestowed on the work by the editor may be gathered from the following slovenly sentences taken from p. 374:—"From experiments by Major Morant, R.E., India, it appears that only one-half the quantity of dynamite and one-third the number of bore-holes is required to remove the same quantity of rock as gunpowder." "The area of the fire-grate being about 14½ square feet."

The Appendix on Tables, Tests, and Formulæ, for the use of electrical engineers, is a real addition to the book. Beginning with a table of the "Formulæ of the Absolute Units," Mr. Jamieson goes on to the definition of the different practical units now in use; and then gives a large amount of useful information in the form of tables and rules for making electrical tests. Much of the information here given is published for the first time, and on this account will be the more valuable to engineers.

Cable work has received special attention, and although not fully treated, absorbs, we think, more than its due share of the sixty-four pages devoted to the appendix. The space taken up by some of the less useful tables might, it seems to us, have been saved, and a fuller treatment of the different methods of testing electromotive force, battery- and other resistances, &c., given. Mr. Jamieson gives only a few rules for such tests, and refers to other books for more, but we think that a book of rules should be full if it be anything. In order to measure the resistance of thick wire, Mr. Jamieson suggests that a piece of it may be drawn down to a fine gauge, and then tested in the Wheatstone bridge. This is neither convenient nor satisfactory, and should be replaced by one of the well-known methods of testing such wires. Again on page 361, Mr. Jamieson proposes to measure the work done in charging a secondary battery by joining up a suitable voltameter as a shunt to it. This method is worse than useless.

Although we should like to see an absence of such faults as the above, and considerably more space devoted to the subject of general laboratory and engineering work, Mr. Jamieson has made good use of the space at his disposal, and we have much pleasure in recommending his appendix as likely to prove exceedingly useful to electricians.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Mr. Stevenson's Observations on the Increase of the Velocity of the Wind with the Altitude

A HEAVY pressure of professional work has prevented me till now from noticing Mr. Archibald's remarks on my paper on simultaneous observations of the wind at different elevations.

I fear I have not been sufficiently explicit as to the object of my observations. All of them have reference strictly to the retarding influence of the friction caused by the earth's surface, and are not so much of a meteorologic as an engineering character. This I thought would have been understood from my statement that I believed they were approximately correct *"for practical purposes."* The formulæ are intended to be applicable only to heights within the limits of my observations, and the idea of applying them to the higher regions of the atmosphere, such as from 3800 to 23,000 feet above the earth, never for one moment crossed my mind, and I think the following facts observed by Mr. Glaisher in his balloon ascents prove the futility of attempting to deduce from experiments made near the surface of the earth what the velocity of the wind may be at such great elevations.

"In almost all the ascents the balloon was under the influence of currents of air in different directions." "The direction of the wind on the earth was sometimes that of the whole mass of air up to 20,000 feet, whilst at other times the direction changed within 500 feet of the earth. Sometimes directly opposite currents were met with at different heights in the same ascent, and three or four streams of air were encountered moving in different directions."

"On January 12, 1862, the balloon left Woolwich at 2h. 8m. p.m., and descended at Lakenheath, seventy miles distant, at 4h. 19m. p.m. At Greenwich Observatory, by Robinson's anemometer, during this time the motion of air was six miles only."

On June 26, 1863, "at 9000 feet the sighing and moaning of the wind were heard, and Mr. Glaisher satisfied himself that this was due, not to the cordage of the balloon, but to opposing currents." On the descent, "a fall of rain was passed through, and then *below* it a snow storm, the flakes being entirely composed of spiculæ of ice and innumerable snow crystals."

On July, 1862, the temperature of the air at starting was 59° Fahr., at 4000, 45°, at 10,000, 26°, at 13,000, 26°, at 15,500, 31°, at 19,500, 42°, at 26,000, 16°. On descending, it was found to be 37°·8 at 10,000, while on the ascent at the same height, it was only 26°.

Mr. Buchan states (article "Atmosphere," "Ency. Britt.," 9th edition): "observations of the winds cannot be conducted, and the results discussed, on the supposition that the general movement of the winds felt on the earth's surface is horizontal, it being evident that the circulation of the atmosphere is affected largely through systems of ascending and descending currents." The observations in the higher regions of the atmosphere quoted by Mr. Archibald confirms this irregularity of the atmospheric currents; as, for example, the velocity at an elevation of 1600 feet is greater than at an elevation of 7200 feet, showing that no satisfactory results can be deduced from them.

My observations on the 50-feet pole are only applicable to "small heights" above the ground, and they have proved the absolute necessity of all anemometers being placed at one uniform height above the ground, and are mainly useful in enabling us to reduce anemometric observations obtained by instruments at different heights to the same standard level—a matter which, as a meteorologist, I deem of great importance.

I believe that the formula for small heights will be useful, because I consider it applicable to such engineering works, for example, as the Tay and Forth Bridges.

As regards the other observations with pressure anemometers at comparatively great heights, the highest observed being 1600 feet at the Pentland Hills, the simple formulæ which I proposed were made only to cover the observations which I actually obtained, and they do agree nearly with these results. As to the assertion that I supposed that the force of the wind ought to vary as its velocity, the contrary is the fact, as Mr. Archibald might have seen by my statements that the only hypothesis on which I could account for the paradoxical result of the same formula being *practically* applicable both to force and velocity was the decreased density of the air as we ascend. I am not prepared to admit that the velocity at 100 feet above the sea will, as Mr. Archibald supposes, be much greater than at sea-level, for my simultaneous observations of wind passing over the sea, over sand and over grass (*Min. Cir. Eng.*) render it doubtful. If for example a wind passes over the surface of the sea with a given velocity, which will depend to a certain extent on the comparatively small amount of friction due to passing over water, that velocity will be at once reduced when the current meets the shore and begins to pass over the more retarding surface of solid land. At a height of 100 feet above sea-level, it may not therefore have attained the initial velocity which it had at sea. But as to the whole subject, which

Observations on Velocity at Arthur's Seat

Velocity recorded at high elevation	Velocities computed for lower station			Velocity recorded at low elevation.
775 feet above sea-level.	By Mr. Stevenson's 1st formula. $v = V\sqrt{\frac{h}{H}}$	By Mr. Stevenson's 2nd formula. $v = \frac{Vh}{H}$	By Mr. Archibald's formula. $v = V\sqrt[4]{\frac{h}{H}}$	550 feet above sea-level.
885	703	592	766	720
1,698	1,430	1,205	1,558	1,364
2,620	2,206	1,859	2,405	2,133
3,416	2,876	2,424	3,132	2,718
4,328	3,646	3,071	3,973	3,465
5 575	4,697	3,957	5,117	4,592
6,763	5,698	4,800	6,208	5,640
8,035	6,765	5,702	7,376	6,782
9,368	7,893	6,648	8,600	7,862
10,820	9,115	7,679	9,933	8,765
12,410	10,455	8,807	11,392	9,789
13,700	11,542	9,722	12,576	10,639
15,058	12,687	10,686	13,833	11,680
÷ 13	79,713	67,152	86,869	76,149
Mean results	6,132	5,165	6,682	5,857

Height of stations above sea-level in feet.	Observations		Calculations for lower station		
	At higher station.	At lower station.	By Mr. Stevenson's 1st formula. $f = F\sqrt{\frac{h}{H}}$	By Mr. Stevenson's 2nd formula. $f = \frac{Fh}{H}$	By Mr. Archibald's formula. $f = F\sqrt[4]{\frac{h}{H}}$
1617	5·656	—	—	—	—
1500	—	5·250	5·424	5·246	5·491
1500	8·542	—	—	—	—
915	—	5·000	6·671	5·208	7·516
438	5·077	—	—	—	—
371	—	4·259	4·675	4·301	4·873
371	4·577	—	—	—	—
276	—	4·181	3·945	3·405	4·247
Average for lower stations		4·672	5·179	4·540	5·532

is one of great difficulty, I will only repeat what I formerly said, that "additional observations are much wanted at high levels," and I might have added, at small elevations also.

I have tabulated as above the results given by my two formulæ and by that of Mr. Archibald, from which it appears that my first formula, viz. $v=V\sqrt{\frac{h}{H}}$, agrees more nearly with the recorded results of velocity at Arthur's Seat than any of the others, while my second formula, $f=\frac{Fh}{H}$, agrees best with my first observations of pressure.

Edinburgh, February 17 THOMAS STEVENSON

The Supposed Coral-eating Habits of Holothurians

IN glancing through my back numbers of NATURE my attention has been drawn to a letter on the above subject by Mr. H. B. Guppy, published in the issue dated November 2, 1882. Quoting the late Mr. Charles Darwin's famous work on "Coral Reefs," where it is stated at p. 14, on the authority of Dr. J. Allan, that Holothuriæ subsist on living corals, he recounts the results of his investigations made on the reefs of Santa Anna and Cristoral, with the object of putting such statement to the test. As the upshot of his experiences he writes that he has by no means satisfied himself that Holothuriæ do subsist on living coral. In no instance did he meet with a single individual browsing on the patches of living zoophytes, the two species observed being indeed found living only in the plots of detritus or dead coral matter that flanked the growing masses. Mr. Guppy gives an approximate estimate of the amount of coral sand daily voided by an individual Holothurian, but adduces no evidence as to the manner in which such hard matter is taken into its body. This phenomenon indeed he apparently did not witness; nor, so far as I am able to ascertain, has any other investigator brought forward any positive testimony in this direction.

Through my cultivation of Holothuriæ in company with various other Echinoderms a few years since in the tanks of the Manchester Aquarium, and also more recently in the Channel Islands, I find myself in a position to supply this hiatus in our knowledge of their life economy. The two species that were more particularly the subject of my observations included the large, dark purple *Cucumaria communis*, derived from the Cornish coast, attaining, in its fully extended condition, a length of from eight to twelve inches, and the white or dirty yellow variety of *Cucumaria pentactes*, that rarely exceeds half these dimensions. The oral tentacula in both of these species are largely developed, taking the form of ten extensively ramifying pedunculate plumose or dendriform tufts stationed at equal distances around the oral opening. It is with these organs that the food substances are seized and conveyed to the alimentary system, though in a manner totally distinct from what obtains in other tentaculiferous animals, such as a sea-anemone, tubiculous annelid, or cuttle-fish. When on the full feed it was observed indeed that the tentacles of the Holothurian were in constant motion, each separate dendritic plume in turn, after a brief extension, being distally inverted and thrust bodily nearly to its base into the cavity of the pharynx, bearing along with it such fragments of sand and shelly matter as it had succeeded in laying hold of. No consecutive order was followed in the inversion of the separate tentacles, that which at the moment had secured the most appetising morsel gaining seemingly the earliest *entrée*. But little time was lost in this feeding process, for no sooner was one tentacle everted than another was thrust into the gullet, and so the meal continued, as not unfrequently observed, for several hours together. To furnish a fitting simile for this anomalous phenomenon of ingestion, one might imagine a child provided with ten arms, after the manner of ancient Buddha, grasping its food with every hand and thrusting it in a quick and continuous stream down its throat, the hands and arms with every successive mouthful, not stopping at the mouth, but disappearing up to or above the elbow within the visceral cavity. That the Holothuriæ are not devourers of living corals is shown not only in connection with the data just recorded, but from the fact also that several of these animals were kept in a tank containing sea-anemones and corals (*Balanophyllia verrucosa*) without their interfering with them in any way, or manifesting alimentative functions other than those just described. All that they require for their nutrition is evidently derived then from the coral or shell debris with which they are customarily associated. At first sight this material would appear to be in the last degree adapted for the sustenance of such highly-organised animals, but, as may be confirmed at any time by the investigation of like conditions in aquaria, it will be found that shell-sand, gravel, and all other debris forming the superficial layer at the bottom of the water, when exposed to the light, is more or less completely invested with a thin pellicle of Infusoria, Diatoms, and other microscopic animal and vegetable growths. It is upon these minute organisms that the Holothuriæ feed, swallowing both them and the shelly or other matter upon which they grow, much in the same way as we might subsist on cherries, swallowing stones and all—the nutritious matter in the case of the cherries being in much greater ratio—and the Echinodermata having the advantage over us that they have no vermiform appendage to their alimentary system to jeopardise their safe indulgence in such stone-swallowing propensities. Most probably, but this as a fact I did not at the time take steps to determine, the shell or coral debris, with its investing organisms conveyed to the mouth, is triturated by the characteristic teeth that arm the pharynx into one homogeneous mass, which, after the extraction of all nutritious substances, is discharged in the form of sandy pellets at the opposite extremity. At all events, the phenomenon of food ingestion as witnessed and here described amply accounts for the relatively prodigious quantities of shell- or coral-sand that the Holothuriæ have been observed to void by Mr. H. B. Guppy and other writers.

Data of interest concerning the feeding processes of various other Echinodermata were noted by me at the Manchester Aquarium. Two species of Echini—*E. miliaris* and *E. lividus*—throve well, and devoured large quantities of the seaweed, *Ulva latissima*, thus demonstrating their essentially herbivorous tastes, while the common Sand-star, *Ophiura texturata*, exhibited peculiarly interesting habits. These were kept in a shallow tank with a sandy bottom, and, except at feeding-time, were but rarely visible. No sooner, however, had a few small pieces of chopped fish been dropped into the water and settled to the bottom than their snake-like arms appeared above the sand in all directions, next their entire bodies, then a general scramble ensued for the provided food. This was conveyed to the creatures' mouths with the aid of the flexile arms, one of which was dexterously twisted round the selected fragment—as an elephant might use its proboscis—and the morsel then dragged beneath the body with its central oral aperture, or rather the body dragged on top of it. Phenomena of corresponding interest, did space permit, might be related of numerous other species, but the foregoing will suffice to illustrate the amount of knowledge that may be gained, and gained in no other way, concerning the habits and life-histories of marine organisms by their intelligent study in the tanks of an aquarium.

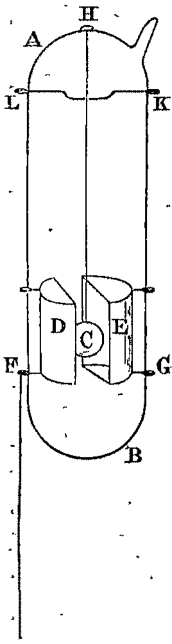
Apropos of this subject of aquaria, it will interest all biologists to know that the long-hoped for opportunity has at last arrived, or is about to arrive, of securing to the nation an aquarium suited in all ways, in both position and equipments, for the conduct of scientific pisciculture and biological research. Included in the buildings now in course of erection for the forthcoming International Fisheries Exhibition is a fine series of marine and freshwater tanks, having such substantial construction and perfect mechanical arrangements, that it is proposed to leave them standing after the close of the Exhibition, their final destiny having alone to be decided on. The close contiguity of these tanks in the Western Arcade of the Horticultural Gardens to the existing Museum of Economic Pisciculture immediately suggests the appropriateness with which they might be incorporated and more fully developed in conjunction with that Museum, or with the biological department of the Science Schools on the opposite side of the road. As an appanage of the last-named institution, it is indeed almost impossible to prognosticate the important *rôle* this aquarium might be adapted to fulfil. Leaving a sufficiency of tanks for public exhibition, the remainder might form an efficient depot or inland zoological station for both supplying the class-rooms and for placing at the disposal of original investigators facilities hitherto unprecedented in this country for making themselves acquainted with the structure, habits, and developmental histories of organisms pertaining to every branch of marine biology. It is at all events most earnestly to be desired that steps will be taken at the right time by the proper authorities to turn to good account so magnificent and rarely recurring an opportunity. W. SAVILLE KENT

Buckland Fish Museum, South Kensington, February 21

Influence of a Vacuum on Electricity

THE theory of Prof. Edlund that a perfect vacuum is a perfect conductor of electricity, but that a discharge across such a vacuum between two electrodes is prevented by an electromotive force at the surface of the electrode, involves our attributing to the vacuum the property of screening from electrical influence any body which it envelops. If the vacuum be a conductor, what we call induction cannot take place through it.

Not having been able to find any record of an experiment which conclusively proved that a vacuum so perfect as to offer considerable resistance to the passage of a current nevertheless permitted induction to take place through it, I have tested the matter by means of the apparatus shown in the figure.



AB is a glass tube about 15 cm. long; C is a light hollow platinum ball, 1 cm. in diameter, hung by a fine platinum wire from the top of the tube between D and E the two separated halves of a cylindrical platinum box, which are insulated from each other and held in position by platinum connections sealed into the sides of the tube, and projecting to the outside at F and G.

It is of importance to mention that the upper terminal H, from which the sphere hung, does not reach more than about 3 millimetres above the inner surface of the tube. The two halves of the cylindrical box are sufficiently near together to prevent the ball coming in contact with the sides of the glass.

This tube was exhausted until an induction current, would give a 12-millimetre spark in air, rather than pass between two terminals, K L, sealed in the upper part of the tube with their opposed ends about half a centimetre apart

A wire about 30 cm. long was then hung from F, and an electrified body presented to the lower end. On the approach of this body to the wire the sphere was at once attracted towards D, and when a discharge was permitted between the electrified object and the wire, the sphere was violently attracted, and a minute spark was seen when the wire holding it touched the cap of the box D. The sphere was then repelled by the similarly charged box.

It thus appears that the phenomena of electric induction take place across a discharge-resisting vacuum, and that the sphere hung in it is not screened from electrical influence as it would be if surrounded by a conductor. A. M. WORTHINGTON

Clifton College, Bristol, February 22

The Meteoroid of November 17, 1882

THERE has already been much discussion on this subject, but I do not think that such exceptional phenomena lose any of their interest by having happened a few months ago; and so I write partly to correct a misapprehension on the part of Mr. Backhouse and Mr. Groneman as to the bearings of the positions of appearance and disappearance of the meteoroid as seen by myself. It seemed to me to appear in the S.E.E. and disappear S.W. by S., but these are not the directions of those points where the trajectory and the horizon would intersect. By mentally continuing the apparent path down to the east and west horizons, points would be reached, nearly, but I think not quite, 180° apart, the former about 20° N. of E. and the latter nearly opposite, so that I scarcely think that it was a great circle, but it is very uncertain. Mr. Saxby states that a similar cloud was observed to cross the zenith of Brussels by M. Montigny. Now there are two accounts—one by M. Zeeman of Zierícksee and the other from near Rye (Sussex)—which seem to consistently apply to one and the same thing, for the latter place is W. by 20° S from Zierícksee, and from both places the same elevation of about 50° was reached. These accounts, if combined with that from Brussels, indicate a height of about 70 miles; but then how does such a height agree with some of the English observations? On the supposition of the above height the altitudes of culmination as seen from Woodbridge and Windsor would be about 29°, from Bristol 25°, and from York 10° only, which last angle is directly at variance with the actual one. For my part, I will give up the reconciling of such contradictory evidence to those who have an aptitude for conundrums. The evidence is in favour of this being an auroral manifestation, but the spectrum of the cloud does not prove this, for as yet it is not known whether the extremely rarefied upper atmosphere may not be excited to such incandescence as will yield the so-called "auroral" spectrum by other means than the electric discharge, as, for instance, by the passage of a cloud of meteorites. Mr. Petrie upholds the latter hypothesis, but I think that there is a simple but weighty objection to it; for it is difficult to see how a cloud of meteoric dust of such closeness and defined form as the appearance of this cloud would imply, could travel through space for any length of time without coalescing into one granular lump, owing to the mutual gravitation of its particles. Of course this objection will have the less weight the smaller we suppose the individual particles to be. This argument will scarcely apply to the well-known meteor streams, whose individual particles are really so very far apart. If this "flying arch" was subject to gravity, it certainly had more than sufficient velocity to prevent it being appropriated by our earth as a satellite, for the tangential speed necessary to a circular orbit of 4100 miles radius round our earth would only be about $4\frac{3}{4}$ miles per second, with a period of $1\frac{1}{2}$ hours. All interested in this phenomenon will no doubt pay more attention to the southern sky during future auroras, in hopes of noting something more of a similar nature, and they will also look forward to seeing a full account of Prof. Lemström's remarkable experiments on the nature of the aurora, which he has been conducting at Sodankyla with such unlooked-for results.

Heworth Green, York H. DENNIS TAYLOR

A Meteor

LAST evening at 9.35 p.m. a remarkably large and brilliant meteor was seen from here, appearing at a point about 10° east of η Canis Majoris, passing slowly over that star in a south-west direction, and vanishing a few degrees above the horizon; time about three seconds. Its light had a pale green tint, and in brightness and apparent diameter it far exceeded Sirius (which was particularly bright all the evening), so much so that my companions, though not looking in that direction, were instantly attracted by the light, and saw it in its splendour.

R. W. S. GRIFFITH

Eyeworth Lodge, Lyndhurst, Hants, March 3

Aurora

LAST night at about ten o'clock there were two beautiful white auroral streamers, like the tails of enormous comets, near the Pleiades. They were nearly vertical, and slowly moved, in a direction parallel to the horizon, towards Orion, after which they gradually vanished. There was little wind, and the night was bright starlight, after a cloudy day. There was an auroral glow like twilight over the northern horizon. The barometer rose during yesterday and last night, and stands high.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, February 28

Hovering of Birds

WITH regard to Mr. W. Clement Ley's remarks, I have already been permitted to explain in NATURE (vol. xxvii. p. 366) how I had accidentally misunderstood Mr. Airy's meaning. I do not believe that any bird having a greater specific gravity than the air can retain a perfectly fixed position in a calm without some wing-motion. Mr. Ley "believes that there is nothing in the etymology of the word 'hover' that implies movement." This has induced me to look up a somewhat voluminous and recent dictionary, in which I find "Hover, *v i.* (W. hoviaw, to hang over, to fluctuate, to hover). To flap the wings, fluttering or flapping the wings with short irregular flights"; and more to the same effect, all indicating movement. J. RAE

AMATEURS AND ASTRONOMICAL OBSERVATION

THE labour done by astronomical amateurs has had no little influence upon the progress of the science. The work achieved by them has indeed often been of the utmost value, and a long list of names might be adduced of those who in past years attained a most honourable position either as discoverers, as systematic observers, or as both. Seeing therefore that amateurs, whose efforts are purely disinterested and the natural outcome of a love for the subject, have contributed so largely to place our knowledge of astronomy in its present high place, their efforts should be encouraged and utilised by their contemporaries, who hold official positions, and who may find it convenient to assist them by some of that practical advice and instruction which they are eminently qualified to afford.

It seems a thing to be deplored that in this country there are no establishments where astronomy is made a special subject for teaching, and where those who early evince a taste in this direction may be educated in conformity with inclination. We think that an institution giving special facilities to astronomical students, and affording instruction both in observation and computation, must prove a most efficient means of advancing the interests of the science. It cannot be denied that the work of many amateurs is rendered far less valuable than it would otherwise be by its approximate character, that is to say, by its lack of critical exactness—both as regards practice and theory. This cannot be avoided under present circumstances. A man on first becoming imbued with the desire to study astronomy as a hobby is generally in a measure isolated; he has to rely entirely upon his own exertions and what he can get out of the popular treatises upon the subject. It must, however, be conceded that he has many difficulties to encounter, both imaginary and real, before he proceeds very far; and these impediments are of such force as either to deter him altogether from advancing further, or check him so effectually that more than ordinary enthusiasm is required to surmount them. Now this could be obviated by a little timely assistance from some practical astronomer. Treatises, however exhaustive and felicitous in explanation, can never be as effective as personal instruction and example, and hence it seems a desideratum that some establishment should be arranged to afford assistance to such amateurs as are anxious to qualify themselves as practical astronomers. It is certain that could such instruction be imparted on reasonable terms, there are many amateurs who would gladly avail themselves of the opportunity. The main purpose might be to train observers to the use of equatoreals, transit instruments, micrometer work, photography, &c., and in the proper reduction of observations and computation of orbits.

The fault with amateurs seems to be that they are devoid of organisation, and generally of proper education to the work in hand. Labouring independently and intermittently they have, as a rule, no definite purpose in view other than the mere gratification of curiosity. It is obvious that some means should be adopted to attract them to suitable channels for systematic work, so that they may be enabled not only to find pleasure, as hitherto, in seeing objects of interest, but also more effectively to aid the progress of the science by making their observations of practical utility. For it cannot be doubted that the means of determining exact positions and the capacity to reduce them will naturally increase the ardour and interest of observers, and must introduce a new and powerful element to the further advancement of astronomy. The number of amateurs is steadily increasing year by year, and there are now in this country a very large assortment of efficient telescopes which are lying comparatively idle or so misdirected as to be of little service. Under these circumstances it seems desirable to make some attempt to organise the labours of amateurs in directions suitable to their means and inclinations, and to utilise such results for the benefit of astronomy.

It is generally the case that amateurs employ their instruments in spasmodic fashion, and do not tenaciously follow up important observations even when such are well within their grasp. For instance, an interesting marking on a planet may be once seen and recorded as a feature of peculiar interest, but it is then allowed to escape subsequent observation, and thus the value of the record is lost. It is not sufficient to see a thing; we must hold it as long as possible, watching its variations of motion and form, and thus possibly arriving at something definite as to its behaviour and physical character. We cannot, it is true, expect amateurs, who generally are much pressed with other engagements, to work for long periods and at inconvenient hours, because this directly means a sacrifice of other interests which it is imperative should not be neglected. But by the exercise of discretion, and by the utilisation of favourable opportunities, we think that observers, though their time may be much restricted and their instrumental means very limited, may yet contrive to do valuable work in one or other of the many attractive departments of astronomy.

The fact sometimes forces itself upon us that astronomical *work* is not nearly commensurate with the *means*. The large number of powerful instruments now in use might surely be expected to yield a most abundant harvest of results; but we cannot deny that this is far from being the case. It is sometimes the boast of the fortunate possessors of a 10-inch refractor or 12-inch reflector that their instruments are comparable, as regards performance and reach, with those employed by the first Herschel; and this being granted, how comes it that there is such a manifest lack of new discoveries and of that unwearying enthusiasm exhibited by the earlier observers? Possibly some of our best instruments are merely erected as playthings serving to gratify popular curiosity. The possessor of a "big" telescope is always courted to a certain degree by people who, though knowing little and caring nothing about the science, yet profess great interest in order to be permitted to view some of the most interesting wonders in the sky. These ordinary sightseers love novelties of any kind; moreover a view of such objects and an explanation by the "astronomer" himself is a thing to be desired, because one acquires self-importance and can dilate upon the subject to one's open-mouthed friends who have never been honoured with such marked distinction. It is needless to say that such exhibitions are mere waste of time; valuable opportunities—and they are few enough in this climate!—are lost never to return.

Many fine telescopes, though occasionally in use, are not directed to the attainment of any important ends. Year after year they are kept in splendid adjustment; a speck of dust on the lens is removed with scrupulous care; a spot of dirt on the circles is rubbed off with anxious energy, and the owner stands off a few paces to view his noble instrument with intense pleasure. How grand it looks! How massive! Surely this splendid machine is able to reveal the most crucial tests of observational astronomy? The knowledge that he has the means to see great things is in itself a sufficient satisfaction without any practical application. Besides, how can he think of departing from his invariable custom of going to bed at 10.30 p.m. and risk catching a slight cold into the bargain? His intention certainly had been to make a prolonged vigil to-night, but that was decided on in the sunny afternoon before the frosty air came on and before the fog began to rise up from the valley, and so he decides with some show of reluctance to leave it all to another night! Here is the hour, but not the man.

It is a fact to be regretted that many promising amateurs have had to relinquish, prematurely, all astronomical work on account of circumstances. A man on first experiencing the desire to do something to astronomy buys a few books, and then, when he finds it indispensable, a telescope, thus expending it may be the hard-earned savings of a few years. He becomes more interested with new facilities, and devotes much time to the subject. Ultimately the fact is realised that his business affairs are suffering from want of proper attention, and what is of even more importance his health is failing with over-application to work. There is no alternative but to relinquish his favourite hobby, and he parts with his books and instruments for what little they will fetch. How many are there who have had this experience? How many promising observers have left the science because it offers no pecuniary rewards or benefits such as other work commands? "Life is real, life is earnest"; the telescope must be neglected for the ploughshare, and the solitary though withal happy hours of vigil must be given over to Morpheus! Many have realised all this, and though their names will never be known as astronomers, they have deserved as much credit for their disinterested efforts as many others who have from more fortunate circumstances achieved eminence.

It must be admitted that observers of the present day have many advantages over their predecessors, owing to the greater perfection and size of instruments and the conspicuous advances in the serial literature of the science. The latter has developed wonderfully during the last few years with such publications as *The Observatory*, *Copernicus*, *L'Astronomie*, *Sirius*, *Ciel et Terre*, *The Sidereal Messenger*, &c. Formerly we had but the *Astronomische Nachrichten*, *Wochenschrift fur Astronomie*, and *Astronomical Register*. This leads us to hope for a corresponding increase in the number of astronomical workers.

It cannot be questioned that the essential direction of labour on the part of amateurs should be more of a systematic or methodical character than hitherto. A certain department or definite work should be taken in hand and followed up persistently. Little good is likely to accrue from erratic work or from the hasty and necessarily incomplete examination of many different objects. Every observer has a leaning towards a speciality, and he should pursue this exclusively even to the absolute neglect of other departments. Astronomy offers such a large number and variety of objects that to attempt an investigation of more than a mere fragment will tax more than the energies of a lifetime. We would therefore recommend amateurs to apply themselves sedulously to such special branches as they may individually select, for the indiscriminate use of a telescope is to be deprecated on many grounds. W. F. DENNING

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT[1]

II.

M. VULPIAN has observed that the excitability of the lower parts of the spinal cord increases as the upper part is gradually shaved away, so that each layer of the cord appears to exercise an inhibitory action on the one below it. M. Brown-Séquard supposes that in each layer of the cerebro-spinal system there are both dynamogenic elements and inhibitory elements for the subjacent segments.

We are, in fact, almost obliged to assume that each nerve-cell has two others connected with it, one of which has the function of increasing, and the other that of restraining the function of the nerve-cell itself.

Applying this same hypothesis to Newton's rings, we would say that certain parts of the lens or of the glass plate possessed the property of interfering with the rays of light, or were inhibitory centres for them. Others again had the property of increasing the brightness, or were stimulating centres for them; and, moreover, that different parts of the lens or of the glass plate contained each its stimulating and inhibitory centres for different coloured rays.

The multiplication of centres in the lens and glass plate soon becomes more than the imagination can well take in; and we are at present almost precisely in the same condition regarding inhibitory and stimulating centres in the nervous system.

As soon as we get rid of the idea that the darkness caused by the interference of the rays of light at certain points is due to some peculiar property inherent in the glass, and attribute the interference simply to the relationship between the waves of light and the distance they have to travel, the whole thing becomes perfectly simple, and the same is, I think, the case in regard to inhibition in the nervous system.

Let us now take a few more examples of inhibition.

We find in experiments with the frog's foot exactly the same as on our own hand. Thus, when a little turpentine is placed upon the toes it excites a violent reflex, but if a little turpentine be injected under the skin of the same foot, the reflex is abolished.[1] We find also that irritation applied to a limited region of the skin usually causes marked reflex, but if the same stimulus be applied to the sensory nerve supplying that region, the reflex is very much less.[2] In the cases just mentioned the irritation is applied to sensory nerves of the same part of the body, and close together, and the explanation of its different results is the same as that already given for the different effects of tickling and pressure. Different sensory nerves on the same side of the body, but at some distance from each other, will also cause inhibition of motor reflexes; thus it has been shown by Schlosser[3] that simultaneous irritation of the skin over flexor and extensor surfaces will lessen reflex action.

Some years ago I observed that frogs suspended by the fore-arms with cords, or tied with their bodies against a board, reacted less perfectly to stimulation of the foot by acid than a frog suspended by a single point, as in Turck's method. Tarchanoff[4] has also observed that frogs held in the hand also respond less perfectly than when hung up; the gentle stimulation of the sensory nerves in the skin of the body appearing to exercise an inhibitory action over the reflex from the foot.

The injection of acids or irritating solutions into the mouth[5] or dorsal lymph sac[6] also exercises an inhibitory action on reflexes from the foot.

A similar effect is produced by irritating the sciatic nerve on one side by a Faradaic current, and applying a stimulus to the other foot. So long as the irritating current is passed through the sciatic nerve, no reflex movement can be elicited by stimulation of the other foot; but so soon as the Faradaic current stops, the reflex excitability again appears in the other foot.[7] As this phenomenon occurs when the influence of the brain and upper part of the spinal cord has been destroyed by a section through the cord itself, the inhibition which occurs must be due to an action which takes place in the lower portion of the spinal cord.

Stimulation of the nerves of special sense has also an inhibitory action on reflex movements. This we can readily see in ourselves, by observing our actions in the dark. If we touch something cold or wet, or if something suddenly comes against our face, we give an involuntary start, sometimes almost a convulsive one. If, however, we were able to see, we should not give a start in the least when we touched a piece of wet soap, or when the end of a curtain suddenly came against our cheek.

Without entering into the nervous mechanism through which sight effects this change in our actions, but only reducing it to its simplest form of expression, as we would in talking of animals, we say that the stimulus to the sensory nerves of the hand or cheek, by contact with the wet soap or with the curtain, caused in us a reflex spasm, which was inhibited by the stimulus applied to our optic nerves. A similar occurrence is observed in frogs, and the reflex actions produced by stimuli applied to the feet are much stronger when the inhibitory effect of the optic nerves upon them is removed by covering up or destroying the eyes, or by removal of the optic lobes.[1]

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[1] Continued from p. 421.

[1] Richet, Muscles et Nerfs, Paris, 1882, p. 710

[2] Marshall Hall, Memoirs on the Nervous System, London, 1837, p. 48.

[3] Arch of Physiol. 1880, p. 303, quoted by Richet, *op cit.* 709

[4] Quoted by Richet, *op cit* p. 709

[5] Setschenow, Physiologische Studien uber die Hemmungsmechanismen fur die Reflexthätigkeit des Rückenmarks im Gehirn des Frosches, Berlin, 1863, p 33.

[6] Brunton and Pardington, St. Bartholomew's Hospital Reports, 1876 p. 155.

[7] Nothnagel, Centralblatt d med. Wiss 1869, p 211.

Regarding the optic lobes, we will have a good deal more to say presently, for they have been considered to be special inhibitory centres, and are often known by the name of Setschenow's centres.

If we try to explain all those instances of inhibition by the assumption of special inhibitory centres for each action, we must suppose, in connexion with every sensory nerve, that centres exist which lessen or abolish the ordinary reflexes produced by stronger or weaker stimulation applied to the nerve. Besides this, we must suppose other centres which inhibit motor actions in other parts of the body: as for example, when irritation of the extensor lessens reflex excited by irritation of the flexor surfaces, or *vice versâ*, or when the irritation of one sciatic stops reflex action from mechanical irritation of the other foot. A special inhibitory centre must be placed also in the optic lobes in connection with the optic nerves. This complication reminds us of the multitude of inhibitory centres which one must imagine in glass, in order to explain the occurrence of Newton's rings by them, but it seems to me that all these cases are readily explained on the hypothesis that the motor and sensory cells concerned in them are so placed with relation to each other that the stimuli passing from them produce interference *under normal or nearly normal conditions of the organism*.

A spot of light may be caused to disappear by throwing another ray upon it, so as to interfere with it, but it may be also made to disappear from the place where it was, by simply reflecting it somewhere else.

A similar occurrence to this takes place in the body, and although two stimuli may interfere with and destroy each other, we not unfrequently find that the apparent abolition of the effect of a stimulus is simply due to its diversion into some other than the usual channel. In very many cases, where we have inhibition we have also diversion; and it is not at all improbable that when the stimulus is very strong complete inhibition may be impossible by interference alone, and can only be effected by diversion of part of the stimulus. We have already said that two waves of sound will neutralise each other and produce silence, but this only occurs when the waves are not too powerful. When they reach a certain intensity they produce secondary waves which give resultant tones, and several facts seem to point to an analogous condition in animal organisms.

We have hitherto considered cases in which the inhibition was probably brought about by interference of two stimuli, so that the one counteracts the other in much the same way as two rays of light interfered with one another in Newton's rings. In one case which we have mentioned, the movement of the hand when it is tickled is entirely arrested by a strong effort of the will, and the hand is allowed to remain perfectly passive and limp. Here we suppose the impulse sent down from the motor centres in the brain to interfere with that which has originated in the cord by irritation of the sensory nerves, and to counteract it so that no muscle whatever is put in action. But very frequently we find that a result apparently similar is produced by a different mechanism, viz. by diversion of the stimulus into other channels. In the former case the arm is felt to be quite limp, but in the latter though it is quite quiet, it is perfectly rigid—all the muscles being intensely on the stretch. Here the stimulus which would usually have excited convulsive movements of the arm, and probably of the body, resulting in a convulsive start, have been diverted from the body into other muscles of the same limb.

A similar power of diverting a stimulus is seen in the instinctive muscular efforts which any one makes when in pain. One of the most common of these is clenching the teeth, and it used to be a common practice in the army and navy for men to put a bullet between the teeth when they were being flogged, and at the end of the punishment this was usually completely flattened. A patient seated in a dentist's chair usually grasps convulsively the arms of the chair, or anything which may be put into his hand; and there can be little doubt that pain is better borne, and appears to be less felt, when the sensory stimulus occasioning it can thus be diverted into motor channels. In children the motor channels into which diversion usually takes place are those connected with the respiratory system, and the sensory stimulus works itself off in loud yells. At a later age the stimulus is often diverted into those motor channels through which reaction occurs between the individual and his surroundings. Thus most people probably remember how a kick in the shins at football often served simply to accelerate their speed; and during the heat of battle the pain of a wound is often but little felt, the stimulus having been diverted into motor channels.

Many more instances might be given of the effects of diversion of stimuli, but having discussed this subject at length in a former paper,[1] I shall not pursue it further here.

Sensory stimuli are also capable of inhibition by interference. Hippocrates[2] noticed, and it is a matter of general observation, that pain in one part of the body may be lessened or removed by the occurrence of pain in another. In many instances, the removal of the pain to one part may be indirect, through the action exerted on the vessels by the pain in the other part. But in some instances it may be, and probably is, due to the direct interference of sensory impressions.

This question of the removal of pain by the interference of waves in the sensory nerves or nerve-centres has been very fully and clearly discussed by Dr. Mortimer Granville.[3] Starting from the hypothesis of interference, he has also devised a plan of treatment which appears to give satisfactory results. By means of a small hammer moved by clockwork or electricity, he percusses over the painful nerve in order to induce in it vibrations of a different rhythm to those which are already present and which give rise to the pain. Thus he percusses rapidly over a nerve when the pain is dull or grinding, and percusses slowly when the pain is acute, in order to produce interference if possible. In many instances the treatment is successful, and its success affords additional support to the hypothesis on which it is based.

We have hitherto spoken of reflex inhibition in the cerebro-spinal axis alone, but we find also reflex inhibition of motor actions produced by irritation of sympathetic nerves; and, *vice versâ*, we find inhibition of the movements of internal viscera produced by irritation of cerebro-spinal nerves. Thus strong irritation of the sensory nerves of the liver, intestine, uterus, kidney, or bladder, occasionally abolishes the power of walking or standing. Irritation of a sensory nerve will frequently arrest the movements of the heart.

The phenomena which occur in swallowing afford an excellent example, not only of inhibition occurring in parts innervated by the sympathetic system, but also of partial diversion of stimuli. Kronecker has found that when we swallow, the food or water is sent down at once into the stomach by the contraction of the muscles of the pharynx, and that afterwards a peristaltic contraction of the œsophagus occurs. When several attempts to swallow are made one after the other, however, the œsophagus remains quiet until they are ended, and then it occurs at the same interval of time after the last, that it would have done after a single act of swallowing.

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[1] Langendorff, Arch. f. Anat. u. Physiol. 1877; Von Boettcher, Ueber Reflexhemmung, Inaug. Diss., Jena, 1878, p. 12.

[1] Brunton, "Inhibition, Peripheral and Central," West Riding Asylum Reports, 1874.

[2] Hippocrates, Aphorisms, sec. i. 46; Sydenham Soc. Ed. vol. ii. p. 713.

[3] Mortimer Granville, Nerve Vibration and Excitation. (London: Churchill, 1883.)

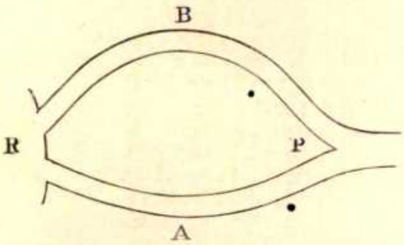

FIG. 2.—Diagram to illustrate Sir J. Herschel's observations on interference. Adapted from his article on "Absorption of Light," Phil. Mag. 1883, p. 405.

If we now refer again to our diagram (Fig. 2, which for convenience we repeat here) we will see that it answers just as well for the contractions of the œsophagus as for the tides at Batsha by simply giving a different meaning to the letters. Let R now instead of representing a reservoir or the open sea represent the ganglia of the pharynx, A and B the nerve fibres which conduct nervous impulses from these ganglia to P, and let P be the ganglia of the œsophagus which stimulate its muscular fibres to peristaltic action. A single wave passing from R causes two waves at P, one succeeding the other, but a number of waves from R under the conditions supposed also cause only two waves: one at the beginning and one at the end, for during all the intermediate period they neutralise each other.

It might perhaps seem that the two stimuli should cause two contractions of the muscular fibres of the œsophagus. But it frequently happens that a single stimulus is unable to produce muscular contraction. It only increases the excitability of the contractile tissue to a second stimulus, and when this is applied contraction ensues. The effect of the first wave then would be to increase excitability, that of the second wave to cause contraction. This is well shown in the accompanying tracing from the contractile tissue of medusæ, which I owe to the kindness of my friend, Mr. Romanes. He has found that when very slight stimuli, such as from weak Faradaic shocks, are applied, the first has no apparent action, but the effect of each successive stimulus is added to that of the preceding ones, until contraction is produced. Two shocks were applied before the first small contraction shown in the tracing occurred, and the shocks are all of the same strength, although the last ones produce the maximal contraction of which the tissue is capable, and the first had apparently no effect at all. This relation of the contractile tissue to stimuli is usually expressed by saying that the tissue has the power of summation.

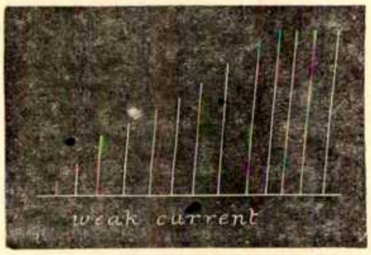

FIG. 6.—Showing the increasing contractions of the tissue of medusa when stimulated by repeated weak induction shocks of the same intensity.

At the same time that a stimulus is sent down from the pharynx to the œsophageal ganglia, which has an inhibitory action, there appears to be another sent to the medulla oblongata, which acts on the roots of the vagus nerve. This latter stimulus has a very curious effect, viz. inhibition of inhibition. The vagus usually exercises an inhibitory action on the heart, rendering its beats less rapid than they would otherwise be, but during swallowing this inhibitory action is removed and the heart pulsates at nearly double its normal rate.[1] Here we seem to have a stimulus one part of which passes along one path, while another part is diverted and passes along another. Each part interferes with the nervous actions which would occur in its absence, but one part interferes so as to prevent, and the other so as to increase muscular activity in the œsophagus and heart respectively.

The same diversion of a stimulus which we find in the case of the œsophagus seems to occur frequently throughout the body. Thus we find it almost invariably in relation to the vascular changes which occur on stimulation of a sensory nerve. When a sensory nerve going to any part of the body is irritated, the vessels of the district which it supplies usually dilate, while those of the other parts of the body contract.[2] The stimulus in this case passes to the vasomotor centre, and thence is reflected as an inhibitory stimulus in one direction and as a motor stimulus in another.

Some results of the greatest interest have recently been obtained by Dastre and Morat, in some experiments which they have made on the subject of vascular dilatation or inhibition.

In many cases the stimulation and inhibition of vascular nerves take place in the medulla oblongata, or in the spinal cord, and the inhibitory and motor centres are close to each other; but in other cases, such as those experimented on by Dastre and Morat,[3] we find the inhibitory and motor centres separated from one another, some of the motor centres being in the cord and some of the inhibitory in a ganglion situated nearer the periphery.

It was previously known that in some cases, as in the dilatation of the vessels of the submaxillary gland on irritation of the chorda tympani, small ganglionic structures were situated at the terminal branches of the nerve, and it was supposed that these ganglia, by their interposition between the nerve and the structure on which it was to act, converted its motor power into an inhibitory one. The experiments of Dastre and Morat are much more definite on this point. Excitation of the cervical sympathetic nerve has the effect of causing the vessels of the ear to contract very greatly in the rabbit, but irritation of the same nerve causes in the dog enormous dilatation of the vessels of the mouth. Moreover, in the rabbit this constricting action on the vessels of the ear is exerted only when the nerve is irritated between the first cervical ganglion and the ear. When the nerve is irritated beyond the cervical ganglion, instead of causing constriction, it produces dilatation.

In order to explain this action, the authors suppose that the fibres of the sympathetic, in passing through the ganglion, end in the ganglionic cells, and thus suspend the tonic action which they exert on the constricting fibres which issue from the ganglion and pass to the ear. It seems to me, however, that a more satisfactory explanation of this fact also is afforded by the hypothesis of interference.

In the cerebro-spinal system, cells being ranged above, below, and around one another with free communication between them, we have ample provision for the passage of two stimuli along paths of such different length, as to enable them to interfere with and inhibit each other.

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[1] In my own case the proportion is 120 to 76.

[2] Ludwig and Loven, Ludwig's Arbeiten, 1865, p. 17.

[3] Archives de Physiologie, 1882, tom. x. p. 326.

But in peripheral nervous mechanisms, such as those in the heart of the frog, where we have no such provision, and the cells are not only few in number, but not arranged in strata, we find a special form of ganglion cell which seems constructed for this very purpose. This is the spiral cell described by Beale, in which we find one nerve-fibre twisted round and round in a way which reminds us of a resistance coil in a galvanic circuit. The object of this peculiar arrangement has, so far as I know, not been discovered; but it seems to afford the exact mechanism which is wanted, in order to alter the distance two stimuli have to travel, and thus allow them to interfere with and inhibit each other. The occurrence of these ganglia in the heart and other viscera seems to afford in itself some support to the hypothesis here advanced; but we will defer the consideration of the mode in which inhibition occurs in the heart and other internal viscera, and pass on at present to the effect of various parts of the central cerebro-spinal system upon each other.

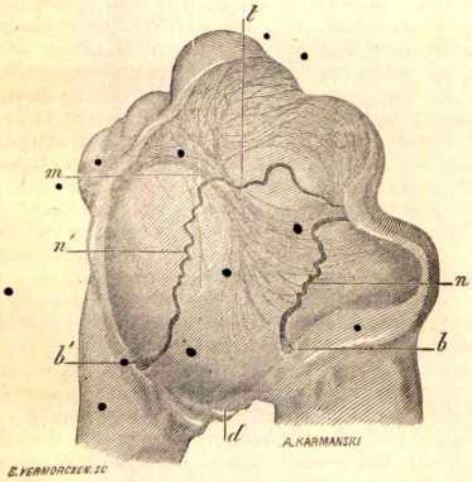

FIG. 7.—View of the auricular septum in the frog (seen from the left side). *n* is the posterior, and *n'* the anterior cardiac nerve. *t* is a horizontal portion of the latter nerve; *b* is the posterior, and *b'* the anterior auriculo-ventricular ganglion: *m* is a projecting muscular fold. This figure is taken by the kind permission of my friend, M Ranvier, from his Leçons d'Anatomie Générale. Année 1877-8.—Appareils nerveux terminaux, t. 6, p. 79. (Paris: J. B. Baillière et Fils, Rue Hautefeuille 19.)

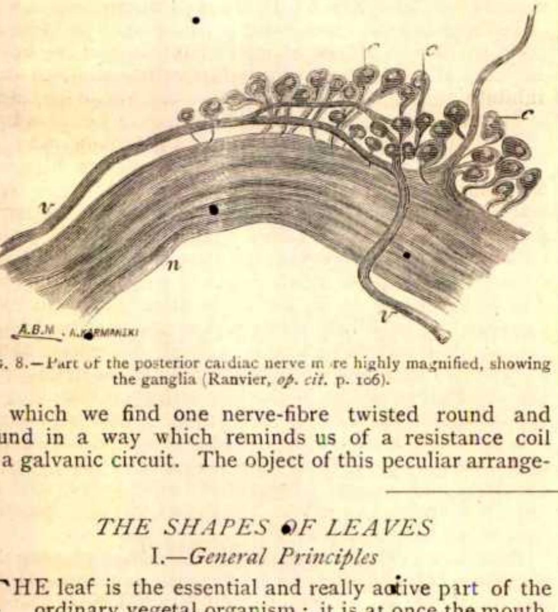

FIG. 8.—Part of the posterior cardiac nerve more highly magnified, showing the ganglia (Ranvier, *op. cit.* p. 106).

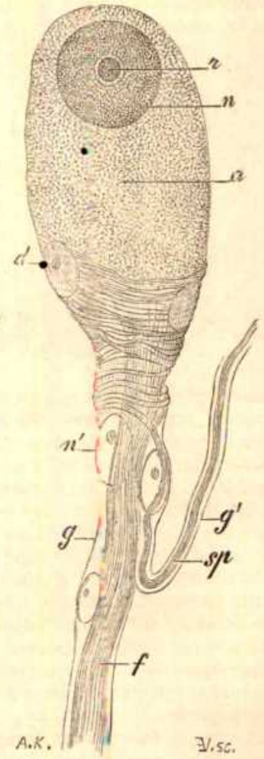

FIG. 9.—Spiral ganglion cell from the pneumogastric of the frog. This figure is not taken from the cells in the cardiac nerves, as in them the connection between the spiral and straight fibres has not been clearly made out, but it is probable that these cells have a structure similar to the one figured (Ranvier, *op. cit.* pp. 114-20). *a* is the cell body, *n* the nucleus, *r* the nucleolus, *d* nucleus of the capsule, *f* the straight fibre, *g* Henle's sheath, *sf* spiral fibre, *g'* its gaine, *n* nucleus of Henle's sheath (Ranvier, *op. cit.* p. 114).

T. LAUDER BRUNTON

(*To be continued.*)

THE SHAPES OF LEAVES

I.—*General Principles*

THE leaf is the essential and really active part of the ordinary vegetal organism: it is at once the mouth, the stomach, the heart, the lungs, and the whole vital mechanism of the entire plant. Indeed, from the strictest biological point of view every leaf must be regarded as to some extent an individual organism by itself, and the tree or the herb must be looked upon as an aggregate or colony of such separate units bound together much in the same way as a group of coral polypes or the separate parts of a sponge in the animal world. It is curious, therefore, that so little attention, comparatively speaking, should have been given to the shapes of the foliage in various plants. "The causes which have led to the different forms of leaves," says Sir John Lubbock, "have been, so far as I know, explained in very few cases." Yet the origin of so many beautiful and varied natural shapes is surely worth a little consideration from the evolutionary botanist at the present day, the more so as the main principles which must guide him in his search after their causes are simple and patent to every inquirer.

The great function of a leaf is the absorption of carbonic acid from the air, and its deoxidation under the influence of sunlight. From the free carbon thus obtained, together with the hydrogen liberated from the water in the sap, the plant manufactures the hydro-carbons which form the mass of its various tissues. Vegetal life in the true or green plant consists merely in such deoxidation of carbonic acid and water, and rearrangement of their atoms in new forms, implying the reception of external energy; and this external energy is supplied by sunlight. We have thus two main conditions affecting the shape and size of leaves: first, the nature and amount of the supply of carbonic acid; and second, the nature and amount of the supply of sunshine. But as leaves also aid and supplement the roots as absorbers of water, or even under certain circumstances perform that function almost entirely alone, a third and subordinate element also comes into play in many cases, namely, the nature and amount of the supply of watery vapour in the air.

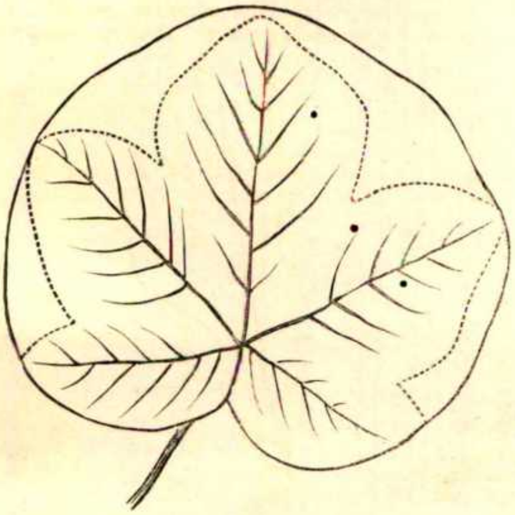

FIG. 1.

This last element, however, we may leave out of consideration for the present, confining our attention at the outset to the first two.

Carbonic acid is the true food of plants: water, one may say, is only their drink. The roots can almost always obtain a sufficient amount of moisture; and though no doubt there is sometimes a fierce struggle for this material between young plants, yet its effects are not usually so obvious or so lasting on the shape of the parts concerned. But for the carbon of which their tissues must be built up there exists a competition between plants as great and as evident as the competition between carnivores for the prey they pursue, or between herbivores for the grasses and fruits on which they subsist. The plant endeavours to get for itself as much as it can of this fundamental food-stuff; and all its neighbours endeavour to frustrate and to forestall it in the struggle for aërial nutriment. Again, the carbon is of no use without a supply of sunlight in the right place to deoxidise it and render it available for the use of the plant. Hence these two points between them, mainly govern the shapes of leaves. Natural selection insures in the long run the survival of those types of foliage which are best fitted for the performance of their functions as mouths and stomachs in the particular environments that each species affects. Accordingly, in the final result each plant tends to have its chlorophyll disposed in the most economical position for catching such sunlight as it can secure; and it tends to have its whole absorbent surface disposed in the most advantageous position for drinking in such particles of carbonic acid as may pass its way. The importance of the first element has always been fully recognised by botanists; but the importance of the second appears hitherto to have been too frequently overlooked.

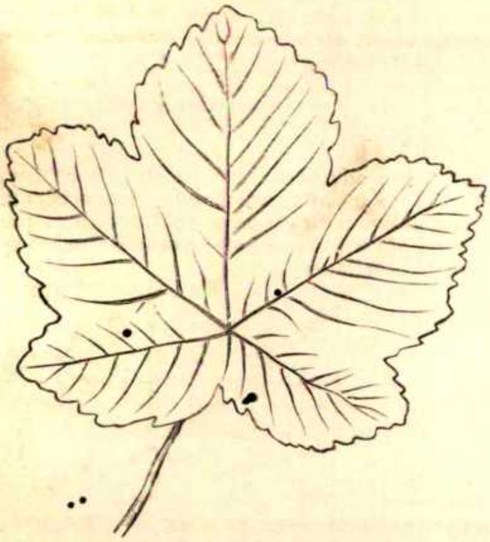

FIG. 2.

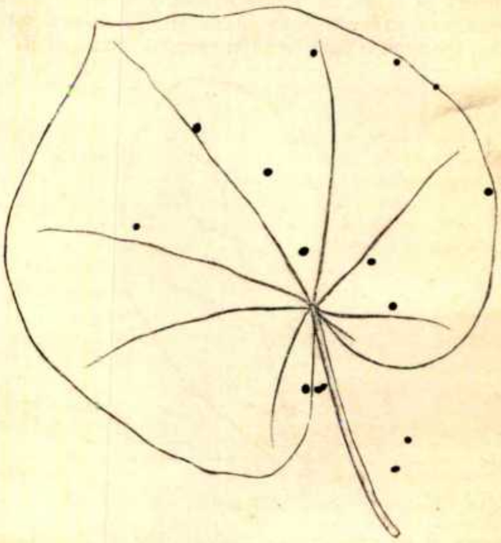

FIG. 3.

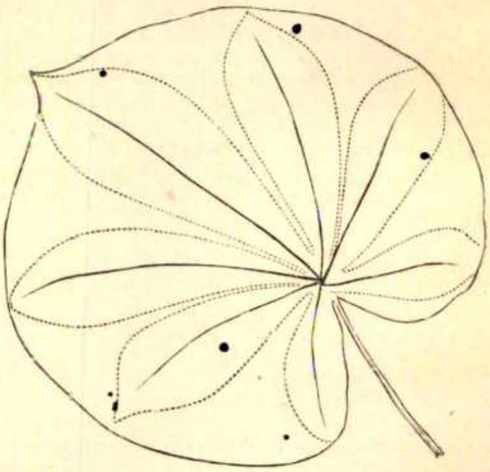

FIG. 4.

At the same time, the shape of the leaf in each species is not entirely determined by abstract considerations of fitness to the function to be performed: as elsewhere in the organic world, evolution is largely bound by hereditary forms and ancestral tendencies. Each plant inherits a certain general type of foliage from its ancestors; and it modifies that type so far as possible to suit the exigencies of its altered conditions. It cannot remake the leaf *de novo* at each change of habit or habitat: it can only remodel it in accordance with certain relatively fixed ancestral patterns. Hence, as a rule, each great group of plants—family, tribe, or genus—has a common type of leaf to which all its members more or less closely approximate. Occasionally, as among the composites, the diversity of types in a single family is very great; at other times, as among the peas and still more among the pinks, the type is fairly well preserved throughout. But, in spite of all apparent exceptions, and of numerous very divergent cases, there is a general tendency in most allied plants to conform more or less markedly to a certain general central and ideal form of leaf—the form from which all alike are hereditarily descended with various modifications. The actual shape in each case is not the ideally-best shape for the particular conditions; it is only the best possible adaptive modification of a pre-existing hereditary type.

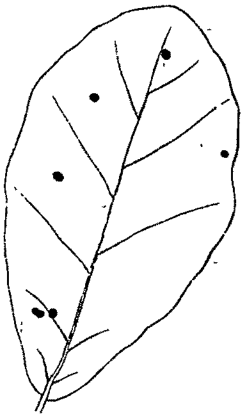

Fig. 5.

The point that is most common to leaves of different sorts in the same group is their vascular framework or ground-plan; in other words, their venation. This is the typical thing which tends most of all to reproduce itself, under all varieties of external configuration. The plant seems to build up first, as it were, its ancestral skeleton, and then, if it can afford material, to flesh it out with the intervening cellular tissue (not, of course, literally, for all the leaf buds out at once from a single knob). A glance at the accompanying diagrams will show how easily, by failure of growth in the intervals between the principal ribs, a simple primitive rounded leaf may be converted during the course of evolution into a lobed or compound one. In Fig. 1 we have such an ovate leaf, with digitate venation: the dotted line marks the chief intervals between the ribs, mainly filled by cellular tissue. In Fig. 2 we have the leaf of a sycamore, with the same venation, but with the intervals between the ribs unfilled. Here it will be noticed that the apex of the five main lobes corresponds in each case with the termination of a main rib; and the largest lobe answers to the midrib. Similarly, the apex of each minor serration answers to the termination of a secondary riblet. The type remains the same throughout; only in Fig. 1, material has been supplied to fill it all in, and in Fig. 2, only enough has been supplied to cover the immediate neighbourhood of the main veins.

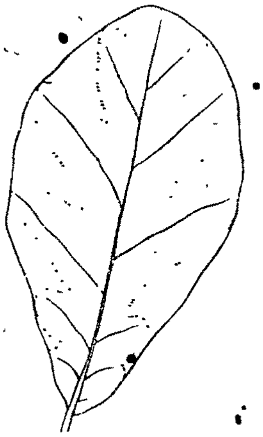

Fig. 6.

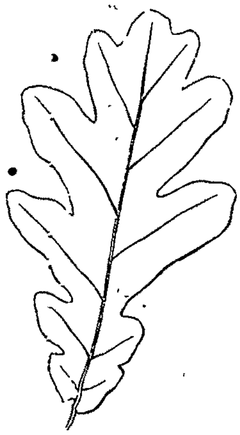

Fig. 7.

In Figs. 3 and 4 we get a further modification of a similar type. Here the cutting of the lobes goes so deep as to divide the entire blade into separate leaflets; and the result is the compound leaf of the horse-chestnut.

The same thing may also occur with pinnately-veined leaves. In Fig. 5 we get a typical leaf of this character, where the supply of carbonic acid and sunshine under the average circumstances of the plant is sufficient to allow of its having assumed a full and rounded specific form. Fig. 6 shows the less fully-veined tracts in such a type of foliage; and in Fig. 7, where the ordinary conditions do not favour full development, we get the familiar irregularly-lobed blade of the English oak. The diagrammatic representation in Fig. 8 suggests the steps by which a regularly pinnately-veined leaf, such as that of the common olive, may pass into a pinnatified and pinnatisect form by non-development of the mainly cellular tracts. We may thus get either a lobed leaf like the hawthorn, as

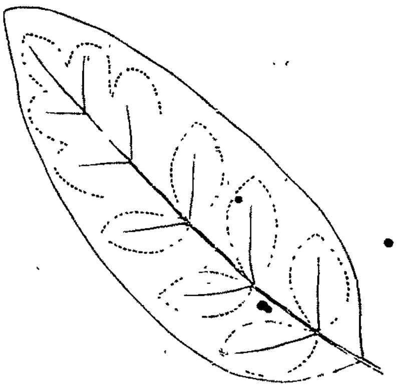

Fig. 8.

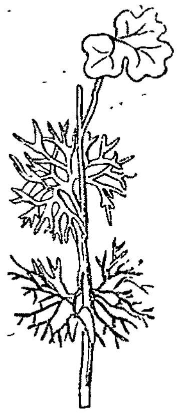

Fig. 9.

adumbrated at the summit of the diagram, or a compound leaf with pinnate leaflets like the commonest papilionaceous type, as shown in the lower portion. These examples will at once make clear the principle that with very slight changes in the real structural composition of a leaf we may have very great differences in the resulting outline. How the various underlying types of venation themselves are acquired or modified we must consider at a later stage; for the present we must take them for granted as relatively fixed generic or tribal characteristics.

It may be necessary to warn the reader in passing that comparatively little importance must be attached to the particular circumstances of each individual leaf. It is the average circumstances of the species which give rise to the specific type. True, each particular blade cannot grow at all except in so far as material is supplied to it during its growth from the older and more settled members of the complex plant-commonwealth; but even when such material is supplied to it, it will only grow to the extent and into the shape which natural selection has shown to be the best on the average for all its predecessors. For example, no plethora of available material would make the sycamore or the oak produce leaves like those represented in Figs. 1 and 5; it would only make them produce a greater number of normal leaves like those represented in Figs. 2 and 7, since these embody the final result of all the past experience of the race—the residuum of countless generations of unsparing selection.

A single illustration of the way in which these general principles work can best be found, as a first example, in the foliage of the water-crowfoot (*Ranunculus aquatilis*, Fig. 9). This well-known plant, growing as it does in streams or pools, has two forms of leaf on the self-same branch, strikingly different from one another. The lower or submerged leaves, which wave freely to and fro in the water, are minutely subdivided into long, almost hair-like, filaments; the upper or floating leaves, which loll upon the surface of the stream, are full and rounded, though more or less indented at the edge into from three to six obovate lobes. Familiar as is this curious little English plant, the causes which give it its two types of leaves admirably illustrate the laws which we must employ as the general key to all the shapes of foliage throughout the vegetal kingdom.

First, as to the submerged leaves. These organs, growing in the water under the surface, have not nearly so free access to carbonic acid as those which grow in the open air. For the proportion of carbonic acid held in solution by water is very small; and for this small amount there is a great competition among the various aquatic plants. As a rule, the cryptogamic flora of fresh waters consists of long streaming algæ or characeæ, which assume filamentous shapes, and wave about in the water so as to catch every passing particle of the precious gas. When flowering-plants, like the water-crowfoot, take to inhabiting similar spots, their submerged leaves also tend to assume somewhat the same forms, and to move freely with every current in the pond or stream, so as to catch whatever fragments of carbon may happen to pass their way. In this case, there is no dearth of sunshine, no interference of other plants with the incidence of the light; the waving thread-like form depends solely upon the comparative want of carbon in the surrounding medium. The leaves have acquired the shape which enables them best to lay hold on whatever carbon there may be in their neighbourhood; any other arrangement would involve a waste of chlorophyll—a misplacing of it in an unadvantageous position. Full round leaves would be useless under water, because there would not be work enough for them to do there.

On the other hand, when the leaves reach the surface, they have room to spread out unmolested into an area singularly free from competing foliage. Here, then, they plim out at once into a larger rounded type, as they can obtain abundant carbonic acid from the air around, and can catch the unimpeded sunlight on the surface of their pond. The two cases, as Lamarck long since remarked, are somewhat analogous to those of gills and lungs; for though in the one case it is oxygen that is required, and in the other case carbonic acid, yet inasmuch as both are gases dissolved in water, the parallelism on the whole is very close.

It is to be noted, however, that in both cases the central ranunculaceous type of leaf is faithfully preserved in the ground-plan or framework. This central type of leaf is found in a rounded form in the lesser celandine (*R. ficaria*), and in the radical leaves of the goldilocks (*R. auricomus*). It is more divided and cut, or (to put the same thing conversely) less filled out between the ribs in the common meadow buttercup (*R. acris*). But in the water-crowfoot, the floating leaves remain very close to the rounded form of lesser celandine, though a little more lobed at the edge; while in the submerged leaves, we get hardly anything more than an attenuated skeleton of the venation, still essentially keeping up the typical form, though in a somewhat exaggerated and minutely subdivided manner. When one compares these submerged leaves with the equally filiform and minutely dissected submerged foliage of the water-violet (*Hottonia palustris*) and the water-milfoil (*Myriophyllum spicatum*), one sees at once that the same effect has been obtained in the various cases by like modification of wholly unlike ancestral forms. While assuming extremely similar outer appearances, all these plants retain essentially diverse underlying ground-plans.

Furthermore, there are various minor forms or varieties of the water-crowfoot in which minor peculiarities of like import may be observed. The form known as *R fluitans* lives chiefly in rapidly-running streams, where none of its leaves can reach the surface; hence all its foliage is submerged, and deeply cut into very long, thin, parallel segments, which wave up and down in the rapids, and are admirably adapted to catch the floating particles of carbonic acid carried down by the water in its course. The variety known as *R. circinatus* grows mainly in deep still pools, where also its leaves cannot reach the top; and it has likewise submerged foliage with finely-cut segments, but the separate pieces are "shorter and more spreading," because this form is best adapted to catch the stray dispersed particles of carbonic acid in the quiet waters. The common type (*vulgaris* of Bentham) has both forms of leaves, floating and submerged, and grows mostly in shallow pools or slow streams. The type known as ivy-leaved crowfoot (*R hederaceus*) creeps on mud or ooze, and has only the full three-lobed leaves. Finally, it may be noted that even the particular position of individual leaves here counts for something; since nothing is commoner than to find one of the finely-cut submerged leaves with a few upper segments floating on the surface; and these upper segments begin to fill out at once into broader green tips, thus giving the end of the leaf an odd, swollen, and bloated appearance.

GRANT ALLEN

(*To be continued.*)

HERRING AND SALMON FISHERIES

At a meeting of the Executive Committee of the Edinburgh International Fisheries Exhibition of 1882, which proved so successful, held on Wednesday, February 28, it was resolved, on the motion of Mr. John Murray, seconded by Sheriff Irvine, that the funds at the disposal of the Executive Committee be granted to the Council of the Scottish Meteorological Society to carry out the proposed investigations with reference to the herring, salmon, and other fisheries which are described in the Circular submitted by the Council with their letter of application to the Committee of May 23, with power to arrange for a zoological station, and with a recommendation that an application be made to Government for assistance. The sum granted is upwards of 1500*l.*

The results already obtained by the Scottish Meteorological Society in connection with the herring fishery show a close relation between the fluctuations of the catches and changes of temperature, wind, sunshine, cloud, thunder, and other weather phenomena. Thus the observations show, for the six years ending with 1878, that a low temperature is attended with large catches, and a high temperature with small catches. Good catches are also had when the temperature fluctuates about the average, and high temperatures, if short continued, scarcely diminish the catches. So far as the discussion of the observations has gone, it appears that the maximum catches are made when the temperature of the sea is about 55°·5, but this point requires further investigation. Thunderstorms, if widespread, are followed for some days with small catches over the region covered by them.

The Council has hitherto been unable, from want of funds, to complete the discussion of the observations already made; to inspect the fishing districts and confer with the fishermen, and thereby secure observations of the fulness and exactness which are required; and to carry on certain investigations in physics and in natural history which are essential to this inquiry. Of the physical investigations may be mentioned the heating power of the sun's rays at different depths of the sea, which appears to have important bearings, directly and indirectly, on the depth at which the herrings are caught. The inquiries in natural history are mainly those which concern the food of the herring and also the food of the animals on which the herrings prey, together with the influence of weather and season on the distribution of these animals in the sea. In carrying out the latter inquiries, the fishermen would be invited to assist, by entering, in schedules prepared for the purpose, observations as to the colour and appearances of the sea-water, due to the presence of minute organisms. As regards the discussion, it will be necessary to make weather maps of Scotland for each day of the fishing seasons—say upwards of 500—in which special prominence is given to charting the temperature, wind, cloud, thunder, and the other elements of weather which affect the fishings,—together with the catch of each day entered on the positions of the maps where they were severally made round the coast. From these maps some of the causes which tend to localise the shoals will become apparent.

The desiderata at present requiring to be supplied in carrying on the investigation of sea and river fishing are these:—1. Fuller and more exact observations of the temperature of the sea at the surface, and at different depths, by the fishermen at the fishing grounds. 2. The resumption of continuous maximum and minimum temperature observations at Peterhead, and the establishment of similar observations at other points round the coast. 3. The observation of maximum and minimum temperatures in other of the more important salmon rivers. 4. Daily temperature of the sea, by boat at some distance from land, at about six selected places. 5. The discussion of past observations, particularly of the herring fishings as described above. 6. Assistance of specialists in carrying on investigations into the food of the herrings, and into the heating power of the sun's rays at different depths.

We are glad to think that with the surplus funds of the Edinburgh Fisheries Exhibition, so wisely disposed of, the Scottish Meteorological Society will be able to prosecute their researches on these points with some hope of a satisfactory result.

NOTES

THE mathematical papers and memoirs of the late Prof. Henry Smith are, we believe, to be collected, and published in two volumes quarto by the Press of his own University. Miss Smith will contribute a biographical introduction; and the general editorship of the work, which will include a considerable quantity of hitherto unpublished material, will be intrusted to Mr. J. W. L. Glaisher.

IN NATURE for February 1 we gave a brief account of the remarkable results obtained by Prof. Lemstrom with his network of wires arranged up the face of the mountain at his station at Sodankyla, in North Finland. By this means he succeeded in procuring an appearance exactly similar to that of the aurora borealis. In connection with these experiments Mr. G. A. Rowell, assistant in the Natural History Department at Oxford, has issued a circular calling attention to the suggestion made by him forty years ago in reference to similar experiments. "My views on the cause of auroræ," Mr. Rowell states, "are that they result from electricity carried over with vapour by the superior trade-winds, from tropical to polar regions, and its occasional accumulation in the latter to such a degree as to flash back to lower latitudes, through the atmosphere at a reduced density, but still within the regions at which vapour is flotable although in a frozen condition. The directive properties of the magnetic needle I attribute to the return current of electricity from polar to tropical regions. The following is the concluding paragraph of the report on my paper on this subject:—'The author supports his opinion by general reference to the observations on the aurora, &c., in the appendix to Capt. Franklin's "Journey to the Polar Seas," and concludes with proposing the experiments of raising electrical conductors to the height of the clouds in the *frigid regions during the frosts in winter*, which in his opinion would cause the aurora to be exhibited and lead to important discoveries in the science of magnetism.'"—(*Report* of the British Association, 1840, *Transactions* of the Sections, p. 49.)

DURING the past winter, the weather in Shetland and the north has been more stormy than for a number of years. In evidence of the severity of the weather, the inhabitants of the Island of Foula, which lies about eighteen miles to the west of Shetland were only able last week for the first time this year to cross to the mainland in their boats. The large supplies of food laid in, as is usually done, were in many cases exhausted, and several families were only saved from starvation by help received from neighbours who were better supplied.

ARRANGEMENTS have been completed for an exhibition, on an important scale, of hygienic dress, sanitary appliances, and household decoration, under Royal and distinguished patronage, and under the direction of the National Health Society, at Humphreys' Hall, Knightsbridge. The exhibition will be opened on June 2 next. The exhibits will be divided into seven classes, and besides hygienic, rational, and artistic dress, will include food-products, appliances for the sick-room, home nursing and home education, industrial dwelling and cottage hygiene, the sanitation of the house and hygienic decoration, heating, lighting, and cooking apparatus, fuel, &c. The Superintendent is Mr. E. J. Powell, 44, Berners Street, W.

THE National Smoke Abatement Institution is making arrangements for opening a permanent exhibition in a central part of London in an extensive range of buildings, for the display of apparatus, fuels, and systems of heating, combining economy with the prevention of smoke, and the best methods of ventilating and lighting. The exhibition will be free to the public, and will include examples of all the most recent inventions and improved apparatus. A lecture hall for the reading of papers, and instruction classes will be provided; also testing rooms under the supervision of experts, for the purpose of continuing the series of tests and trials commenced in connection with the South Kensington and Manchester Smoke Abatement Exhibitions of 1882. Particulars may be obtained at the offices of the National Smoke Abatement Institution, 44, Berners Street, Oxford Street, London, W.

The Executive Committee of the International Fisheries Exhibition have come to a decision to light their galleries by electricity, and they have already made arrangements for the illumination of fully two-thirds of the area. Messrs. Davey Paxman and Co. have undertaken to supply the necessary motive power, which has been estimated at little less than 700 horse-power.

The International Medical Congress, which, in accordance with the resolutions of the Italian Congress of last year, is to be held this year in Holland, will take place at Amsterdam, during the Colonial Exhibition, from September 6 to 8 next.

We have on good authority the following instance of the liberality of Dr. Oscar Dickson, who has contributed so largely to the various expeditions of Baron Nordenskjold:—An energetic Swedish botanist, Sven Berggren, was some years ago engaged in studying the flora of New Zealand, of which he gave some account in the Swedish *Aftonblad.* In one of his letters he stated, however, that his studies would have to be discontinued from want of funds. The next day a sum of 1000*l.* was received anonymously by the *Aftonblad,* with instructions to forward it to Herr Berggren. It was only many years after that it leaked out that the generous donor was Dr. Oscar Dickson.

Part IV. of Mr. Distant's "Rhopalocera Malayana" appeared this week. A complete synoptical key is given to the genera, and the geographical distribution of the genera and species is fully described. An attempt is made to allude to all biological facts which can illustrate or explain the many complexities in the distribution and economy of Malayan butterflies, and to draw attention to the different theories which have been promulgated to account for the same. The work may thus prove useful as an introduction to the study of Rhopalocera. Already it has assumed much larger proportions than estimated owing to the number of additional species recently received or found in other collections. Woodcuts have also been given, and the plates are equal to anything yet produced by chromolithography. Mr. Distant's work deserves every encouragement.

An International Congress for the Protection of Animals is to be held at Vienna in September next. A great number of local societies, such as those of Berlin, Cologne, Munich, Dresden, Hanover, &c., besides several Spanish, Italian, and Russian, have expressed their intention of being represented at the Congress. Anti-vivisectionist societies will not be invited, as the promoters of the Congress, eminent men of science, do not consider them as societies for the protection of animals, and hold them to be generally incompetent regarding questions relating to such protection.

The Dutch press considers the demand made by Baron Nordenskjöld perfectly legitimate and just.

The death is announced of Dr. Bertillon, the well-known French anthropologist and statistician.

At its January meeting, the Russian Chemical and Physical Society awarded its Sokoloff premium to Prof. Menshutkin, for his researches into the influence of isomerism of alcohols and acids on the formation of compound ethers.

It is interesting to examine the items in the budget of Norway for the ensuing year, which has just been issued, relating to the "extraordinary" grants made in that country for the benefit of science. The following are some of the donations for this year:—To the academies of science in Christiania and Throndhjem, 600*l.*; the museums of Bergen, Stavanger, and Tromsö, 900*l.*; travels of scientific students abroad, 350*l.*; the European geodetic commission, 400*l.*; international observations of the physical condition of the polar regions, 700*l.*; *Archiv* of mathematics and natural sciences, 70*l.*; other scientific journals, 130*l.*; a new natural history journal, 70*l.*; "further," towards the publication of the works of the distinguished Norwegian mathematician, Abel, 100*l.*; a work by Herr Norman on the Arctic flora of Norway, 350*l.*; Herr Tromholt for the study of the aurora borealis, 60*l.*; the *Acta mathematica,* 60*l.*; scientific study of the Norwegian sea fisheries, 300*l.*; for the artificial hatching of salmon ova, 90*l.*; geological researches of Southern Norway, 600*l.*; the society for promoting the Norwegian fisheries in Bergen, 1600*l.*; publication of the reports of the North Atlantic expedition, 100*l.* These amounts, as well as the 3000*l.* granted towards the expenses of the Fishery Exhibition in London, are all in addition to the ordinary subsidies of the year.

The Swedish Government has granted a sum of 60*l.*, for this year, to an entomologist, whose duty it will be to advise farmers as to the best means of destroying injurious insects.

We are informed by the secretary of the Society of Telegraph Engineers and of Electricians that the Crown Prince of Austria has consented to become patron of the Vienna Electrical Exhibition, and that the Emperor has signified his intention of devoting some highly decorated rooms for the purpose of testing the effects of incandescent lighting in connection with various styles of decoration. The time fixed for the receipt of applications for space has been extended from the 1st to the 20th inst., by which latter date they should be in the hands of the Secretary of the Society, 4, The Sanctuary, Westminster. We are also authorised to state that the Committee at Vienna are making arrangements for a reduction in the rates of transit on all goods forwarded to Vienna for exhibition.

It is a common belief among persons who keep poultry that the shocks and tremors to which eggs are subject during transport on road or railway affect the germ contained in the egg. M. Dareste, who has been studying this matter (*Comptes Rendus*), found, a few years ago, that in eggs submitted to incubation directly after a railway journey, the embryo very generally died; but a few days' rest before incubation obviated this. He has lately inquired into the effect of shocks on the fecundated egg-germ, with the aid of a *tapoteuse*, or machine used by chocolate-makers to force the paste into the mills; it gives 120 blows a minute. Monstrosities were always the result of the tremors so caused. This teratogenic cause is the more remarkable that it acts before the evolution of the embryo; whereas the other causes M. Dareste has indicated, as elevation or lowering of temperature, diminution of porosity of the eggshell, the vertical position of the egg, and unequal heating, only modify the embryo during its evolution. The modification impressed on the germ by those shocks did not disappear after rest, as in the case mentioned above; but it is not known why. A few eggs escape the action.

The radiometer is an instrument which may render good service in the hands of the teacher. Prof. Rovelli has been showing this (*Riv. Sci. Ind.*), and among other experiments he suggests are these:—Placing the instrument at the focus of a parabolic mirror, while a mass of snow is put at the focus of a like mirror facing the first a little way off; placing it, with sulphuric ether, under the bell-jar of an air-pump, and exhausting, afterwards letting in the air (the motion is opposite after the air is admitted); exposing the radiometer at the focus of a parabolic mirror turned towards the weak light reflected from snow, on a cloudy day, then turning the mirror away from the snow. Prof.

Rovelli finds that 8° of dark heat neutralise the effect of the weak light emitted by a common candle at the distance of 45 centimetres from the radiometer. The instrument may serve advantageously to demonstrate the relation between the absorptive and the emissive power of bodies, and to determine their respective values.

M. Ferry, the new Premier in the French Cabinet, as well as Minister for Public Instruction, will deliver the usual address to the Congrès des Sociétés Savantes at the end of this month.

M. Houzeau, the director of the Brussels Observatory, has returned from San José, but has obtained leave from his Government, and will spend the remaining part of the winter at Cannes. The King of Belgium is anxious to have the Observatory transferred to Laeken, to an eligible site placed in the vicinity of his castle, but nothing is decided in that respect. A temporary shed has been erected for the new meridian circle by Repsold, but the readings are taken with the old one.

M. Shulachenko, who managed the Russian military telegraph during the Kulja expedition, communicates to the Russian Physical Society the following results of his experiments with Siemens' telephones:—At a distance of 93 miles, music, singing, and speaking were heard quite distinctly; at 130 miles, conversation was difficult,—it was necessary to shout loudly, and those who received messages had to display a great sensibility of ear; but it was possible to have conversation even at a distance of 212 miles. When six pairs of telephones were put side by side, having each its wire, and the wires not being connected with one another, the conversation on one of them was heard on all the others. When the connecting wire of one pair of telephones was broken, the conversation on this pair was heard on the next pair of telephones the wire of which was in good state.

A commemorative stone has been placed on the house No. 17 in Via Dei Prefetti, Rome, to Morse, the telegraphist. The inscription was as follows, translated into English:—"Samuel Finkez Breese Morse inhabited this house from 20th February, 1830, to January, 1831, inventor of the writing electro-magnetic telegraph. He was born at Charlestown 27th April, 1791; died at New York, 2d April, 1872."

The last number of the *Izvestia* of the Russian Geographical Society gives interesting particulars of the naphtha-wells in the province of Ferghana, in Turkistan. There are no less than 200 wells which are situated at the foot of both mountain ridges that inclose the valley of Ferghana. One range of wells, twenty-seven miles long, is situated on both banks of the Naryn, twenty miles north of Namangan. The other, about sixty-five miles long, is situated in the latitude of Makhram, in the districts of Marghilan and Kokan. There is a third intermediate group some thirty miles east of Andijan. The wells are situated in the limestones and slates of the "Ferghana level" of the chalk formation. The specific weight of the Ferghana naphtha is 0·950 at 17° Cels., 0·9517 at 28°, and 0·945 at 43°; it belongs therefore to the heavy mineral oils. The heavier parts remaining after the evaporation of naphtha in open air are known under the name of *khilk*, and when mixed with sand give an excellent waterproof cement, sometimes used by natives for irrigation canals. There are also mines of mountain-wax on the Kok-tube Mountain, in the district of Namangan, and a very good mine of sulphur at Karim-davany.

M. Domojiroff continues to publish in the *Izvestia* of the Russian Geographical Society his anemometric observations on board the clipper *Djighit*. In June, 1881, during the cruise from the Zond Strait to the Seychelles Islands, he met mostly with south-east winds, the velocity of which varied from 3 to 7·5 metres per second, with one exception, on June 9, when it reached 15 metres. On the cruise from the Seychelles to Aden, from June 25 to 30, the wind was mostly south-west, and varied from 5 to 12·7, reaching 14·3 metres per second on June 29. The observations are carried on in the same way as was described in a preceding number of Nature.

The young West Siberian branch of the Russian Geographical Society proposes to publish in its next volume of *Memoirs* a botanical description of the district of Tara, which has the interest of having an intermediate flora between the forest region and the Steppes, the Irtish being a boundary-line between the two. The same Society continues the excavation of several koorgans in the district of Yalutorovsk.

From various parts of the Greek Archipelago and from the Pelikon district continued volcanic phenomena are reported. The neighbourhood of Volo in Thessaly is particularly affected. Also the island of Chios seems again to be a centre of disturbance. The volcano at Santorin is very active.

On February 16, at 8.10 a.m., a slight earthquake was noted at Bologna and the whole Southern Romagna. Mount Vesuvius increased its activity on that occasion.

A discovery, which is expected to throw some light on prehistoric times in what is now Germany, has been made near Andernach on the Rhine. Remains of prehistoric animals have been found in a pumice-stone pit, and Prof. Schaaffhausen of Bonn has investigated the spot closely. A lava-stream underlying the pumice-stone was laid bare, showing a width of only two metres. The crevices between the blocks of lava were filled with pumice-stone to a depth of one-half to one metre; below this, however, there was pure loam and clay, and in this were found numerous animal bones, apparently broken by man, as well as many stone implements. It is supposed that there was a settlement there, of which the food-remains fell into the lava-crevices *before* the whole was covered with pumice-stone.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Miss Annie M. Davis; an Ocelot (*Felis pardalis*) from South America, presented by Mrs. A. Harley; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Miss G. Gordon Clark; a Black Rat (*Mus rattus*), British, presented by Mr. H. B. Stott; a Tawny Eagle (*Aquila nævioides*) from South Africa, presented by Mr. Roland Trimen, F.Z.S.; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, presented by Mr. A. Anderson; a Common Magpie (*Pica rustica*), British, presented by Mr. Charles Davis; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Miss Bibby; a Common Curlew (*Numenius arquata*), a Golden Plover (*Charadrius pluvialis*), British, purchased.

OUR ASTRONOMICAL COLUMN

The Comet 1883 *a*.—In a circular issued from the Imperial Academy of Sciences, Vienna, are the following elements of a comet discovered at Rochester, N.Y., on the 23rd ult., founded by Dr. Hepperger upon observations on February 24, 25, and 26.

Perihelion passage, February 20·20206 M.T. at Berlin.

	°	'	"	
Longitude of perihelion	33	23	51	M.Eq. 1882·0.
,, ascending node	280	4	20	
Inclination	77	32	48	
Logarithm of perihelion distance ...	9·879124			

Motion—direct.

Prof. Millosevich kindly communicates observations made at the Collegio Romano in Rome:—

	Rome M.T h. m. s.	R A h. m s.	Decl. ° ' "
Feb. 28 ...	7 43 12	23 43 19·58	+31 37 54·5
March 1 ...	7 53 14	23 53 1·27	+31 49 7·0

From Prof. A. Riccò, who writes from Palermo on February 28, we learn that he has found the spectrum to be formed of the three bands of hydrocarbons, with an extremely faint continuous spectrum of the nucleus; the sodium line (D) was not present.

The comet is receding from the earth as well as from the sun. The elements have but little similarity to those of any comet previously calculated.

THE GREAT COMET OF 1882.—Prof. Julius Schmidt has published some particulars of his observations of this remarkable body since the commencement of the present year. On Jan. 3 the tail was traced through upwards of 11° with the naked eye; on the 10th it was visible for 8°, on the 28th it had diminished to 5½°, but was readily seen without the telescope; on the 30th its length was 3°. On February 5 a tail 2° in length was perceptible to the naked eye; Prof. Schmidt obtained his last distinct glimpse of the comet without the telescope on February 7.

Dr. B. A. Gould, director of the Observatory at Cordoba, who is now in London *en route* for the United States, informs the writer, that on February 11, three days out from Rio Janeiro, he was satisfied of the visibility of the tail of the comet to the naked eye; its distance from the earth at this time was 2·48, and its distance from the sun 3·05.

THE VARIABLE STAR U CEPHEI.—Mr. G. Knott secured a good observation of the minimum of this variable, at Cuckfield, on the night of March 2. An uninterruptedly clear sky enabled him to keep a watch on the star from 7h. 24m. to 14h. 30m. G.M.T. At about 8h. 15m. it began to fade from 7·2m., and at 14h. 30m. it had risen again to 8·1m. The observed time of minimum was 12h. 36m., or seven minutes earlier than the time assigned in the ephemeris in NATURE, and the magnitude at minimum was 9·45. The star remained at minimum for nearly 2½ hours. The low magnitude attained, Mr. Knott considers, is confirmatory of a suggestion he made from his earlier observations, that at alternate minima the star touches a lower magnitude than at those which intervene.

NEW NEBULÆ.—M. Stephan, director of the Observatory at Marseilles, publishes a catalogue of fifty nebulæ observed there, forty-five of which he believes to be new. A group of four pretty bright nebulæ he gives as identical with *h*, Nos. 2352, 2356, 2358, and 2359, but their relative positions resulting from his observations are not in accordance with Sir John Herschel's Catalogue. The Marseilles places and descriptions are—

	R.A. 1880·0.			N.P.D.			
	h.	m.	s.	°	′	″	
No. 42 ...	11	9	8·45 ...	71	14	8·7	Assez belle, assez petite, ronde, condensation centrale.
„ 43 ...	11	10	28·49 ...	71	19	39·1	Assez belle, assez petite, ronde, condensation centrale.
„ 44 ...	11	10	36·52 ...	71	17	35·0	Belle, ronde, assez étendue, condensation graduelle centrale très forte.
„ 45 ...	11	10	40·73 ...	71	11	46·7	Assez belle, ronde, condensation graduelle centrale assez forte.

The catalogue is published in the *Comptes Rendus de l'Académie des Sciences* of February 26.

GEOGRAPHICAL NOTES

WE are now enabled, on the authority of Dr. Oscar Dickson, to give the following particulars of the programme of Nordenskjöld's proposed expedition:—The expedition will leave Sweden early in May next, in all probability in the Government steamer *Sophia*, and if the state of the ice is favourable to a landing on the east coast this will be effected; but as this is not expected to be the case until later in the season, Baron Nordenskjold will proceed to the west coast, not for geographical discovery, but to study the appearance and extent of the inland ice on this side before attempting to penetrate from the eastern side. There are also known to exist on the west coast some very large blocks of ironstone, perhaps of meteoric origin, which a party of the expedition will be despatched to examine. When these researches are finished, and the state of the ice more favourable, the vessel will make her way from Cape Farewell along the eastern shore in the open channel, which is generally found between the coast and the drift-ice. With regard to the "break" or oasis, believed by Baron Nordenskjold to exist in the interior of Greenland, to which we have previously referred, the explorer has been led to this conviction during his wanderings on the inland ice on a former occasion. He maintains that not only the constant advance of the ice-mass, but the fact that the country does not rise continually in the interior, show that the whole land is not covered with perpetual snow and ice; and this theory, he states, has been further corroborated by the studies made by him and others of the temperature and moisture of the air on the inland ice. The expedition, which will be accompanied by a complete scientific staff, will also aim at studying the conditions of the drift-ice between Iceland and Cape Farewell, the fossil remains in Greenland, as well as the appearance and quantity of the cosmic dust there. One object will also be, if possible, to discover traces of the former Norse settlements. It is expected that the party will return in September next. We understand that the reason why Baron Nordenskjold has not issued any official programme concerning his expedition is that, being occupied with preparations for his journey and public duties, he would not be able to enter into any critical controversies as to his plans and theory.

IT appears from a letter of Dr. L. E. Regel to the Secretary of the Russian Geographical Society, that this Central-Asian traveller successfully pursued his explorations during last summer. He left Samar-land at the end of June last, and to reach Hissar he chose the shortest route, *viâ* Penja-kent. This route, by which the expedition visited the Fan River and Lake Iskander-kul, and crossed the Mur Pass, was very difficult; but the botanical collections and the geographical results were all the richer. In the centre of this region is situated a great mountain range, whose summits—the peaks of Kuli-kalan and the Chandar and Bodhan Mountains—are seen from Samarkand. To the south of this range runs the Saridagh valley, beyond which rises the Hissar range proper; to the north it has the Kul-i-kalan plateau and the valleys of a tributary of the Voron and the Pasrut River. The plateau of Kul-i-kalan has a circumference of about thirteen miles, and is dotted with five lakes 10,000 feet above the sea-level. The mountains around it have no real glaciers, but there are old moraines which can be traced also along the tributary of the Voron, which is fed by one of these lakes. We have here a separate Alpine landscape, the mountains of which are mostly fossiliferous limestones (sandstones with casts of thick fossil trees are found in the Pasrut valley), and with a vegetation not only richer than that of any other part of the basin of the Zarafshan, but also more varied as to its distribution. The forest vegetation is richest in the zone between 4000 and 8000 feet above the sea-level: M. Regel found there apple, cherry, and nut trees, together with the *Archa*. The upper zone, where the *Archa* also predominates, contains birches, willows, and an arborescent *Ephedra*; it reaches 10,500 to 11,000 feet, and the vegetation altogether goes higher up than the limit of perpetual snow. The Mur Pass—about 14,000 feet high—is very steep; the expedition had to cross snow-fields for nearly four miles, and found on the southern slope immense accumulations of snow, which probably is due to the foggy climate of Hissar, although the amount of rain is small in this region. The vegetation of the southern slope is very rich and much like that of Karateghin. The range is composed of syenite; the next range, of the same height, between Khoja-Hassan and Hakimi, consists of granite, syenite-gneiss, and fossiliferous slates. Between Hakimi and Karatagh there is a series of lower parallel ridges, consisting of fossiliferous sandstones. The same sandstones are met with also between the two main ranges; they contain fossils at Khoja-Hassan. Changing his former plan, M. Regel proceeded further directly to Kala-i-Khumb, while his topographer was despatched to Kulab, *viâ* Hissar, the two to meet in the Darvaz. The remainder of M. Regel's letter gives several interesting topographical details, and information about different routes, as well as an enumeration of the chief questions that must be resolved as to the topography of this region.

WE announced last week that a Danish expedition would explore the east coast of Greenland during the summer. The funds required for this expedition were voted by the last Danish Parliament, and it will consist of two lieutenants in the navy, G. Holm, and T. Garde, with two scientific men, but the remaining members will be natives of Greenland. The expedition will only employ boats for their purpose.

THE Ural Mountains are again becoming the field of exploration for Russian geologists and geographers. We learn from the *Izvestia* of the Russian Geographical Society that M. Nasiloff is spending a third year in the exploration of the Northern Ural. After having explored the river Lala under 59° N. lat., where he discovered layers of spherosiderites which were not yet known on the eastern slope of the Ural Mountains, he explored the banks of the Sosva—their geological structure, and the koorgans that are met with in its basin, as well as the fauna and flora of the region. In 1882 he visited the banks of the Lozva and Sosva, and the old mines of this locality, and made large geological, botanical, and ethnographical collections. He followed the Lozva to its junction with the Tavda, and went up the Sosva. The collections brought home by M. Nasiloff are now in the Mining Institute, in the St. Petersburg University, and in the Geographical Society. Another member of the Geographical Society, M. Malakhoff, continued his zoological and ethnographical researches on the Middle Ural. He explored the lake-dwellings discovered in the neighbourhood of Ekaterinburg, and, together with a member of the Mineralogical Society, explored the 3000 feet high mountain, Kachkanar, making there interesting collections of plants and insects. Later on in the summer he visited the districts of Irbit, Ekaterinburg, and Trvitsk, and discovered close by Irbit very interesting accumulations of bones, lake-dwellings on Lake Ayat, containing large implements of slate, and finally stone and bone implements in a cavern close by the Mias ironworks. At Lake Bagaryak he discovered interesting forms for casting animal and human figures during the prehistoric epoch.

HARTLEBEN of Vienna has published a unique little work by Dr. Jos. Chavanne, on "Afrika's Ströme and Flüsse," in which the author briefly surveys the hydrography of Africa as far as recent discoveries have furnished them. The book is accompanied by a well-drawn hydrographical map.

IN the March number of Hartleben's *Deutsche Rundschau* for geography and statistics, Dr. Chavanne has a sketch of the progress of discovery in Africa during 1882. There are interesting biographies, with portraits, of General Strelbitski and the late Prof. Henry Draper.

THE following papers will be read at the third German "Geographentag," which will be held at Frankfort-on-the-Maine on the 29th-31st inst:—On the importance of Polar research to geographical science, by Prof. Ratzel (Munich); on the commercial conditions of South Africa, by Dr. Buchner (Munich); on the significance of the International Colonial Exhibition at Amsterdam with regard to geographical science, by Prof. Kan (Amsterdam); on the reciprocal relations of climate and the shape of the earth's surface, by Dr. Penck (Munich); on the means of determining the geographical position at the time of great discoveries, by Dr. Brensing (Bremen); on the latest efforts made to determine more accurately the shape of the earth, by Dr. Gunther (Ausbach); memoir of Emil von Sydow, by Dr. Cramer (Gebweiler); on topography as an introduction to geography, by Dr. Finger (Frankfort); on the pedagogic requirements and principles in drawing wall-maps for the use of schools, by Herr Coordes (Cassel); on the method of representing various objects on maps, by Prof. Jaroslaw Zdenck (Prague); on the Prussian teaching order and examination with reference to geographical instruction, by Dr. Kropatschek (Brandenburg); on the geographical handbooks of M. Neander, by Dr. Votsch (Gera). Three other highly interesting papers are also promised, viz. notes from his botanical journeys in Tropical America extending over five years, by F. R. Lehmann; on the Balkan Mountains, by Prof. Toula (Vienna); and a report on his great journey across Africa, by Lieut. Wissmann.

NEWS from Zanzibar, dated November 8, 1882, brings the sad announcement of the death of Dr. Kayser, who had been sent by the German African Society to their station on the shores of Lake Tanganyika, together with Drs. Boehm and Reichard, and who had left his station and was on his way to the Gold Coast.

THE CONSERVATION OF EPPING FOREST FROM THE NATURALISTS' STANDPOINT[1]

THE great expanse of primitive woodland in the immediate neighbourhood of East London declared "open" to the public on May 5, 1882, by Her Majesty the Queen, should be regarded as one of the numerous bequests to posterity marking the enlightenment of our times. The feelings leading to the agitation for the preservation of open spaces in and around the metropolis are sure indications on the part of the public of a recognition of the necessity for protecting and conserving our common lands for outdoor recreation—a recognition which must be considered as marking a decided advancement in the ideas of the British holiday-maker. If we compare a map of the environs of London of, say, twenty years ago, with the actual state of the country at the present time, it will be seen that large tracts of open land have disappeared; shady coppices and furze-clad heaths have been inclosed and built upon, and the country-loving Londoner has had to go further and further afield for his rambles. If it is obviously true that increased pressure of population demands more dwelling accommodation, it is equally true that a denser population requires more open spaces. The indifference of the public in former times to their own rights and to the wants of their successors is naturally making itself more and more seriously felt with a rapidly-augmenting population and a corresponding spread of buildings. The formation of such public bodies as the Commons Preservation Society and the Epping Forest Fund was a healthy sign that people were beginning to be alive to the gravity of the situation, and we may now fairly say that rural London is on the defensive. The remarks which I am about to offer on the present occasion are based on an unpublished article written many months ago, when that wooded area in which our interest as a society centres was threatened by tramway invasion. The withdrawal of the Great Eastern Railway Company's bill for extending their line from Chingford to High Beech in 1881, and the apparent collapse of the tramway scheme had led to the hope that the "people's forest" would remain unmolested, and that the Epping Forest Act of 1878 would be carried out in spirit and in letter. But unfortunately new grounds of alarm have recently arisen, and our honorary secretaries, to whom I showed the original manuscript, did me the honour of thinking that the views which I had expressed would still be found to be in accordance with those of our own and kindred societies.

Like other open tracts in the metropolitan district, the great Waltham Forest, which comprised the forests of Epping and Hainault, was rapidly undergoing absorption. From the report of the Select Committee of the House of Commons presented in 1863, it appears that of the 9000 acres which constituted the Forest in 1793, only 6000 acres then remained uninclosed. In 1871, when the Corporation of London took up the Forest question, this area had been reduced to 3500 acres. I do not here propose to trouble you again with the now familiar history of the rescue of this picturesque remnant of primeval Britain (see Mr. J. T. Bedford's "Story of the Preservation of Epping Forest," *City Press* Office, 1882). The work—commenced more than a decade ago by the Corporation of London—received its crowning reward at the late Royal visitation. We shall the more appreciate the results of the action taken by the Corporation when we bear in mind that the total area dedicated to the public last May is very nearly equal to the expanse of 6000 acres reported upon by the Select Committee of 1863. But whilst expressing the gratitude of metropolitan field naturalists generally for the restoration of one of their largest and most accessible hunting grounds, it certainly does seem to me that the shout of triumph raised by the Conservators has been allowed to drown the smaller voices of those who had previously demonstrated to certain rapacious lords of manors by somewhat forcible means that a "neighbour's landmark" was not a movable thing. It must not be forgotten that prior to the year 1871, besides many vigorous individual protests, both the Commons Preservation Society and the Epping Forest Fund had declared war against illicit inclosure. The restoration of the Forest to the people has cost a sum of money considerably exceeding a quarter of a million pounds sterling, and it will be generally admitted that this amount has been well if not wisely spent in the public cause. There are no doubt many who have suffered by their own cupidity, or by that of former manor lords, who still feel aggrieved at the action of the Corporation, and it must indeed be conceded that many whose estates have suffered curtailment have been the unconscious receivers of illegally acquired property and are thus deserving of commiseration. The principles involved in the conflict between public rights on the one hand and manorial actions on the other are of the very deepest importance to the community at large, and it is therefore no matter of surprise that the "Forest Question" should have acquired a quasi-political aspect during the last few years in this neighbourhood.

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[1] Being a paper read before the Essex Field Club, at the meeting held on February 24, by Raphael Meldola, vice-president of the Club.

As far as I have been able to learn, the motives leading to the preservation of our Forest at the great cost specified appear to have been purely philanthropic. The main object was to secure this splendid area for the "recreation and enjoyment" of Londoners generally, and more especially for the East-End inhabitants, whose chances of holiday-making are only too often limited to an occasional day in the country. In one sense the latter class may now, thanks to the movement first set in action by Mr. J. T. Bedford, claim to have a decided advantage over their wealthier West-End brethren, for the total area of the West-End parks (including Regent's) amounts only to about 1150 acres as compared with the 5000 to 6000 acres of open country so easily accessible to East Londoners. In the face of such an obviously enormous gain to the country rambling holiday folk, it may perhaps seem ill-advised to attempt to criticise the action of the Conservators in their dealings with the Forest. It is with great reluctance on my part that I forsake the peaceful paths of scientific study to take up a question which generally appears to lead to nothing more than a manifestation of angry controversy, and I only do so now on behalf of that numerous and ever increasing scientific class of holiday-makers whose claims thus far appear to have been altogether put out of court.

Long before the question of encroachment or of preservation had been brought into its present prominence, botanists, entomologists, microscopists, and students of nature generally were in the habit of frequenting our Forest and of rambling in quest of the objects of their study through this woodland expanse so conveniently situated with respect to the great scientific centre of this country. There are records which prove that Epping Forest has been for more than a century the hunting ground of many who have gathered materials from its glades for the great storehouse of human knowledge, and who have taken a true and purely intellectual delight in studying its animal and vegetable productions. The London naturalists of the present time should surely have something to say in connection with the fate of this favourite haunt, made classic ground to them by the memories of such men as Richard Warner, the author of the "Plantæ Woodfordienses" (1771), Edward Forster, the Essex botanist, who wrote between the years 1784 and 1849, and Henry Doubleday, of Epping, our own grocer-naturalist, who died in 1877. It is time for the natural history public, by no means such an insignificant body as is generally supposed, to raise their voice on behalf of these "happy hunting grounds." The position to be taken up is not necessarily one of antagonism towards the Conservators, but it is certainly desirable that some understanding should be come to respecting the claims of those who, in pursuit of knowledge, have long been contented to bear with the pitying smile of the ignorant for "trifling away their time upon weeds, insects, and toadstools." The numerous scientific societies and field clubs of the metropolitan districts have already declared their views on former occasions, and it is chiefly with the object of attempting to define the respective attitudes of the parties concerned that I have entered the arena on the present occasion.

There are at the present time more than twenty Natural History Clubs in the environs of London, and of these many have long been in the habit of making collecting excursions to our Forest. Our own Society and our Walthamstow colleagues have their head-quarters in the Forest district. Some of the East-End clubs are entirely composed of working men, and have done excellent work in fostering a healthy taste for the study of outdoor natural history among this class of the community, a matter of considerable importance to us when we so often hear that the Forest has been acquired as a recreation-ground chiefly for the working men of East London. In addition to these numerous local clubs, there are the great London Societies, which, like the Linnean, Zoological, Entomological, Royal Microscopical, and Quekett Club, are all interested in promoting the study of biology in its various branches. Now, in face of the rapid destruction of all the truly wild tracts of country in the vicinity of London, it must assuredly be of the greatest importance to the natural history public as a body to watch with a most jealous eye the dealings by those in authority with this the largest, wildest, and most accessible of all the open spaces in the metropolitan district. To naturalists generally such a tract of primitive country as that which has come under the management of the Corporation is something more than a mere picnic-ing ground—to all students of nature it is a *biological preserve.* Nay, I will even go so far as to declare that forest management is essentially a scientific subject in itself—a natural history question in the broadest sense. Now with the exception of our esteemed members, the Verderers, by whom we were invited to a conference some months ago, it appears to me that the Conservators as a body—and a confessedly unscientific body—are not aware that scientific counsel is necessary to enable them to faithfully carry out the Act of Parliament, *i.e.* to keep the area committed to their charge in its "natural aspect" as a forest. I will therefore take the present opportunity of pointing out that scientific criticisms would have been disarmed and the fears of natural history students allayed if the Epping Forest Committee had only recognised the claims of science by consulting, let us say, the Directorate of Kew Gardens, or by appealing to the Councils of some of the London Societies.

If we consider the actual work done during the period that the Forest has been under the jurisdiction of the Corporation, we may fairly say that the energies of this body have hitherto been developed in the direction of landscape gardening; *i.e.* of *artificialising* certain portions of the Forest. The great hotel at Chingford has been made the centre of convergence of a number of roads, some of which have been newly cut even at the risk of being superfluous. The aquatically-disposed holiday-maker may hire boats in which he can paddle about on an "ornamental water," or can embark on a floating machine turned by hand-paddles, and possibly constructed with a view to delude the occupants into the belief that they are on board a steamer. The exhausted East Londoner whose vitality appears to require that recuperation which seems to be derivable from swinging, steam-roundabouts, and throwing sticks at cocoa-nuts, has been amply provided for, and his wants have in every way been attended to. In 1881 the Forest was threatened by a railway; in 1882 by a tramway, and again this year another railway bill is about to be introduced into Parliament. To all these schemes the Committee, no doubt with the best motives, gave and still give their support, and one has to seriously ask what is the meaning of the word "conservator," and how far this attitude is compatible with the instruction that "the Conservators shall at all times as far as possible preserve the natural aspect of the Forest," and "shall by all lawful means prevent, resist, and abate all future inclosures, encroachments, and buildings, and all attempts to inclose, encroach, or build on any part thereof, or to appropriate or use the same, or the soil, timber, or roads thereof, or any part thereof, for any purpose inconsistent with the objects of" the Act of 1878. It must not be supposed that there is any desire on the part of naturalists to exclude the general public. I wish only to emphasise the fact that up to the present time it would appear that the Forest has fallen into the hands of those who are disposed to regard it exclusively from the point of view of excursionists and "cheap trips," and in accordance with the principle that supply and demand act and react, it may be expected that this class—which has thus far alone been catered for—will more and more frequent the Forest district. Increased accommodation for excursionists means, if we may judge from the line of action pursued by the Conservators, an extension of facilities for swinging and donkey-riding. The "improvements" that have hitherto been made have not been of such a nature as to preserve the woodland in its native beauty, but have been limited to the conversion of a portion of the Forest land into a resort for pleasure-seekers of the class indicated. To the naturalist—and I am sure I may say to the intelligent public generally—such a tract of primitive country is beautiful only so long as Nature is given full sway, and the adjustments which for long ages have been going on slowly and silently under the operation of natural laws remain unchecked and uninterfered with by man. No unscientific body of Conservators can possibly realise to the fullest extent the seriousness of the charge committed to their care.

With respect to the management of the Forest, the views of naturalists are now so well known that no excuse can be made for ignoring them. Our wants are of the simplest and most economical nature—our case is perfectly met by the trite aphorism, "let well alone." The whole Forest area at present existing may be considered to consist of primitive woodland and of tracts formerly under cultivation. The former can best be dealt with by leaving the "management" to Nature; whilst the latter should be naturalised as soon as possible. And here we cannot close our eyes to the fact that while a large amount of money has been expended in altering portions of the Forest proper, no attempt has yet been made to plant or to restore to a natural condition those unsightly tracts which were formerly inclosed, and of which many remain as barren wastes to the present time. The cause of the naturalist is thus imperilled both by the active and by the passive position of the Committee—he is like the pitcher in the Italian proverb, which says that "whether the pitcher hits the stone or the stone hits the pitcher, it is always the worse for the pitcher."

It is now quite unnecessary to make detailed statements of the views of individual naturalists with reference to the present subject. It will be remembered that at a meeting of this Society held last year, Sir Thomas Fowell Buxton brought forward a proposal—and a very excellent one it was—that all landowners round the Forest district should agree to stop generally the destruction of all birds and animals on their estates, so that a great experiment might be carried out for some years, leading to a true "balance of nature" in the whole area comprised between the valleys of the Lea and Roding. At the discussion arising from that suggestion the preservation of the fauna and flora as a whole was advocated, and many naturalists whose opinions will carry great weight expressed their views on the question of forest management. The complete report of this meeting has not yet appeared, but I will refer you to it prospectively, and in an appendix to the part of our *Transactions* now going through the press will appear papers, drawn up at the request of the Council, by Dr. M. C. Cooke, Mr. J. E. Harting, and Prof. Boulger. The evils of deep drainage, from the naturalist's point of view, which form the text of Dr. Cooke's protest, have already been pointed out by many, and I will just call your attention to some remarks on this subject by our eminent honorary member, Mr. A. R. Wallace, in an able article published in the *Fortnightly Review* for November, 1878, wherein he says:—"It must be remembered, too, that a proportion of bog, and swamp, and damp hollows are essential parts of the 'natural aspect' of every great forest tract. It is in and around such places that many trees and shrubs grow most luxuriantly; it is such spots that will be haunted by interesting birds and rare insects; and there alone many of the gems of our native flora may still be found. Every naturalist searches for such spots as his best hunting grounds. Every lover of nature finds them interesting and enjoyable." After enumerating some of the rarer marsh plants of our Forest, Mr. Wallace continues:—"These and many other choice plants would be exterminated if by too severe drainage all such wet places were made dry. The marsh birds and rare insects which haunted them would disappear, and thus a chief source of recreation and enjoyment to that numerous and yearly increasing class who delight in wild flowers, birds, and insects, would be seriously interfered with."

It is somewhat exceptional for a society founded for the study and promotion of natural science to find itself engaged in active polemics, but in taking up the position into which we have been forced, we are simply carrying out that line of action which at our foundation I ventured to lay down as our true function with respect to the Forest. (Inaugural Address, *Transactions*, vol. i. pp. 19, 20.) It is extremely unfortunate that the claims of science should appear to be opposed to the wants of the general public—I say should *appear* to be opposed, because I am convinced that there is no real antagonism. The grievance of naturalists is not only that their claims have been ignored, but the action of the Conservators has hitherto been entirely on the destructive side, and a feeling of alarm has arisen lest the whole of the Forest should piecemeal be desecrated in the name of a fictitious philanthropy. The public wants—as interpreted by the Board of Conservators—are made to take the form of clearing of underwood, drainage, roadmaking, the intersection of the Forest by railways and tramways, and ample public-house accommodation. If these are really the fundamental requirements of holiday-seekers, then there must for ever be a strong antagonism between this class of the public and those whose cause I have taken it upon myself to advocate. At this juncture, however, we may fairly ask whether this kind of artificialised recreation-ground, *à la* Cremorne, is actually demanded by the frequenters of the Forest. I believe myself that it is not. The notion of keeping a holiday in what is only too often a bestial manner is not a fair estimate of the British excursionist. If he gives way to the temptations which have been so lavishly scattered in his path, it is, as Shakespeare puts into the mouth of King John, because "the sight of means to do ill deeds makes ill deeds done." The East Londoner who wishes to spend a day in a "people's park" is provided for elsewhere, but if we consent to the denaturalising of our Forest, the more intelligent class of excursionists—and their name is legion—will be either driven from its precincts or will suffer that degeneration which the line of action at present pursued is exclusively calculated to bring about.

In the course of these remarks I may have somewhat exaggerated the supposed antagonism between the two classes most interested in the conservation of Epping Forest, but I have done so with the object of defining as sharply as possible the position of the hitherto unconsidered naturalist. The conditions requisite for transforming the Forest into a "people's park" are fatal to its preservation as a natural history resort. Any piece of waste land can be made into a park, but a tract of wild forest once destroyed can never be restored. I would once more urge, and most emphatically, that there is not the slightest desire on the part of naturalists to exclude the "toiling million," or to prevent their full enjoyment of the Forest. I wish only to point out that my present contention is that in the long run the wants, both of the naturalist and of the ordinary excursionist, will be found to be absolutely coincident. If the neighbourhood of a railway terminus with its concomitant evils leads to the destruction of the "natural aspect" of any portion of the Forest, that portion is ruined, not only for the naturalist, but likewise for the general public who come to enjoy a day in the country far from "the busy hum of men." By judicious management the requirements of both classes can be met, and it rests entirely with the Conservators to determine whether the attitude of the respective parties is to be pacific or the reverse. It must be remembered that long before the Forest was rescued by the Corporation this district was a favourite resort of multitudes of holiday folk, and, not being interfered with to any considerable extent, was at the same time available to the naturalist. The note of alarm must be sounded, or we may find ourselves worse off than in pre-Conservatorial times. The constitution of the Epping Forest Committee is apparently prejudicial to our interests if we may judge by the standard of past and present actions. Of this Committee the Verderers, who, as representing the Commoners and as residents in the Forest district, are best qualified to advise with respect to the management of the Forest, form but four of a Committee of sixteen. However enlightened the views of these gentlemen may be—and I only wish I could say that the present Verderers were unanimously of our way of thinking—they are thus liable to be outvoted. Another evil, and a most serious one so far as we are concerned, is that the Committee is practically a secret one—its proceedings are conducted with closed doors, and the people at large, whether naturalists or excursionists, have no means of making their voices heard. Whether this action is just in a case where the funds are derived from a public source it does not enter into my province to consider.

The views which I have now put forward are offered with the best of intentions with respect to the body Conservatorial. We cannot be unmindful of our obligation to the Corporation for having saved the Forest, but we appeal to them to assist in exalting the ideas of those who frequent this place as a holiday resort instead of pandering solely to the more degraded aspect of human nature. A day spent amid the natural beauties of our sylvan glades is the beau ideal of a holiday, intellectually, morally, and physically, to those whose pursuits keep them confined to the town. Let Epping Forest be preserved for the multitudes who have for so long enjoyed it rationally. The "recreation and enjoyment of the public" will thus become possessed of a higher meaning, and the naturalist while carrying on his studies as heretofore will be doubly grateful to those who have secured these time-honoured preserves as a public space free from all fear of inclosure or destruction. The ideas which I have attempted to formulate are I know entertained by large numbers not only of working naturalists, but also by the continually growing class of lovers of the country and of nature in general. It is becoming a matter of almost national importance that the surviving tracts of open country in the neighbourhood of all large towns should be rigidly preserved, and opinions in accordance with this have from time to time been forcibly expressed both with respect to our own Forest and all the common lands in the environs of London.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following further appointments to Electoral Boards have been made:—

Professorship of Botany: Sir Jos. Hooker, Dr. F. Darwin, Dr. M. Foster, Prof. Oliver, Dr. Hort, Dr. Phear (Master of Emmanuel College), Rev. G. F. Browne, and Rev. M. J. Berkeley.

Woodwardian Professorship of Geology: Prof. Prestwich (Oxford), Rev. E. Hill, Mr. W. H. Hudleston, Mr. A. Geikie (Director of Geological Survey), Dr. Phear, Mr. R. D. Roberts, Mr. Ewbank, and Prof. A. Newton.

Professorship of Zoology and Comparative Anatomy: Prof. Flower, Prof. Moseley (Oxford), Dr. M. Foster, Prof. Huxley, Mr. J. W. Clark, Dr. F. Darwin, Prof. Humphry, and Mr. D. McAlister.

The Woodwardian Professor has been authorised to apply a sum equivalent to the late Assistant's stipend in payment of Demonstrators for this and the next term.

The regulations for the degrees of Doctor in Science and Doctor in Letters have been confirmed, with minor modifications.

The additional mathematical examination of candidates for honours in the "Little Go" is to be discontinued; Elementary Logic is to be hereafter allowed as a substitute for Paley's Evidences; Euclid is to be limited to the more useful propositions; algebra is to be increased in quantity; and the examination is to be held three times a year, the additional time being at the beginning of October.

The subject for the next Sedgwick Prize Essay, 1885, is "The Jurassic Rocks of the Neighbourhood of Cambridge."

The last Report of the Mathematical Board recommends that the Moderators and Examiners shall be the adjudicators of the Smith's Prizes, and that the Smith's Prizes be awarded on the results of Part III. of the Mathematical Tripos. This would give more distinction to the examination in the higher subjects. The concurrence of Professors Stokes, Adams, and Cayley in this recommendation is a strong point in its favour.

The report of the Moderators and Examiners in the last Mathematical Tripos, the first under the new system, gives particulars about Part III., to which only the Wranglers are admitted. Of the twenty-nine Wranglers, sixteen presented themselves for Part III., of whom two were not finally classed. In order to give opportunity to a candidate who had confined his reading mainly to one group of the higher subjects to employ his whole time in questions in that group, the examiners in the five bookwork papers gave at least four questions in each group which came into the paper, and fixed five as the limit of questions to be answered. In the fifth paper, subjects for essays were chosen from each group. The majority of candidates attempted too many subjects, and their answers as a rule were poor and meagre. The Examiners are far from satisfied with the average performance of the candidates in Part III., but they expect better results when the new system is better understood, especially the encouragement given to limiting reading in the higher subjects to one or two groups.

FREE admission to the lectures and courses of practical instruction in the Normal School of Science and Royal School of Mines at South Kensington and Jermyn Street will be granted to a limited number of Teachers and Students of Science Classes under the Science and Art Department, who intend to become Science Teachers. The selected candidates will also receive a travelling allowance, and a maintenance allowance of twenty-one shillings per week while required to be present in London. The courses given and the duration of each are as stated below:—Chemistry: Part I., October to February; Parts II. and III., October to June. Physics: Part I., October to February; Parts II. and III., October to June. Biology: October to June. Geology. Part I., February to June; Part II., October to February. Mechanics: Part I., February to June; Parts II. and III., October to June. Metallurgy: October to June. Mining: October to June. Agriculture: October to January. Attendance is required from 9 or 10 a.m. to 4 or 5 p.m. daily, in addition to the time necessary in the evening for writing up notes, &c. Students will be required to attend the Classes for Mathematics, Geometrical Drawing, and Freehand Drawing, so far as may be considered necessary. Candidates for these Studentships must send in their applications before May 31, on Science Form No. 400, copies of which may be obtained on application to the Secretary, Science and Art Department, South Kensington. When the same Student is a candidate for more than one course, the order of preference should be given. It should, in all cases, be stated for which course or courses the student is a candidate.

SCIENTIFIC SERIALS

American Journal of Science, February.—Henry Draper, by G. F. B.—Fauna at the base of the Chemung group in New York, by H. S. Williams.—Geological chemistry of Yellowstone National Park.—Geyser waters and deposits, by H. Leffmann.—Rocks of the Park, by W. Beam.—Electromagnetic theory of light; general equations of monochromatic light in media of every degree of transparency, by J. W. Gibbs.—The rainfall in Middletown, Connecticut, from 1859 to 1882, by H. D. A. Ward.—Discoveries in Devonian Crustacea, by J. M. Clarke.—Observations of the transit of Venus, 1882, made at the Lick Observatory, by D. P. Todd.—The antennæ of Meloe, by F. C. Hill.—Hypersthene-Andesite, by W. Cross.—Method for determining the collimation constant of a transit circle, by M. Schæberle.

The American Naturalist, December, 1882, contains:—A pilgrimage to Teotihuacan, by R. E. Hills.—On the grey rabbit (*Lepus sylvaticus*), by Samuel Lockwood.—The Palæozoic allies of Nebalia, by A. S. Packard, jun.—American work on recent mollusca in 1881, by W. H. Dall.—The organic compounds in their relations to life, by L. F. Ward.—The reptiles of the American Eocene, by E. D. Cope.

January, 1883, contains:—The history of anthracite coal in nature and art, by Jas. L. Lippincott.—The development of the male prothallium of the field horse-tail, by D. H. Campbell.—On the geological effects of a varying rotation of the earth, by J. E. Todd.—On the bite of the North American coral snakes (*Elaps*), by F. W. True.—Achenial hairs and fibres of Compositæ, by G. Macloskie.—Instinct and memory exhibited by the flying squirrel in confinement, with a thought on the origin of wings in bats, by F. G. King.—The extinct Rodentia of North America, by E. D. Cope.

February, 1883, contains:—The Kindred of Man, by A. E. Brown.—Indian Stone Graves, by C. Rau.—On organic physics, by C. Morris.—The mining regions of Southern New Mexico, by F. M. Endlich.—The extinct Rodentia of North America, by E. D. Cope.—Spencer and Darwin.—The Beastiarians.

Annalen der Physik und Chemie, No. 2.—The electric conductivity of some cadmium and mercury salts in aqueous solutions by D. Grotrian.—On the change of the double refraction of quartz by electric forces, by W. C. Rontgen.—On the optical behaviour of quartz in the electric field, by A. Kundt.—On the function of magnetisation of steel and nickel, by H. Meyer.—Contributions to the history of recent dynamo-electric machines, with some remarks on determination of the degree of action of electromagnetic motors, by A. von Waltenhofen.—On the viscosity of salt solutions, by S. Wagner.—Researches on the absorption of gases by liquids under high pressures, by S. v. Wroblewski.—Strecker's memoirs on the specific heat of gaseous biatomic compounds of chlorine, bromine, iodine, &c., by L. Boltzmann.—On the luminosity of flames, by W. Siemens.—Distillation in vacuum, by A. Schuller.—Researches on the elasticity of crystals of the regular system, by K. R. Koch.—On absolute measures, by C. Bohn.—Correction of the method adopted by R. Kohlrausch in his researches on contact-electricity, by E. Gerland.—The volume-change of metals in melting, by F. Nies and A. Winkelmann.—Correction, by A. Guebhard.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 22.—"Preliminary Note on the Action of Calcium, Barium, and Potassium on Muscle." By T. Lauder Brunton, M.D., F.R.S., and Theodore Cash, M.D.

It has been shown by Ringer that calcium prolongs the contraction of the frog's heart. This prolongation is diminished by the subsequent addition of potash.

It occurred to us that calcium and potassium salts might exercise a similar action on voluntary muscle. On trying it we found this to be the case. Calcium in dilute solution prolongs the duration of the contraction in the gastrocnemius of the frog. Potassium salts subsequently applied shorten the contraction. We have been led to try the effect of barium on muscle by considerations regarding the relations of groups of elements, according to Mendelejeff's classification, to their physiological action. These considerations we purpose to develop in another paper. The effect of barium is very remarkable. It produces a curve very much like that caused by veratria, both in its form and in the modifications produced in it by repeated stimuli. We have found that the veratria curve is restored by potash to the normal in the case of the gastrocnemius, just as Ringer found it in the case of the frog's heart. The peculiarity which barium produces in the gastrocnemius is also abolished by potash. We have tested a number of other substances belonging to allied groups, and find that some of them have a similar, though not identical, action with barium. The results of these experiments, as well as the general considerations to which we have already alluded, we purpose to discuss in another paper.

"On the Formation of Uric Acid in the Animal Economy, and its Relation to Hippuric Acid." By Alfred Baring Garrod, M.D., F.R.S.

The paper is divided into an introduction and three parts. The introduction contains the results of a series of experiments upon the solubility of uric acid and its most important salts, at the temperature of the body; and upon the effects of mixing the urates of sodium and ammonium with the phosphates and chlorides of the same bases.

Part I. contains observations upon the physical and microscopic characters of the urinary excretions of birds, reptiles, and some invertebrata, as well as chemical investigations of such excretions, and of the blood of the same classes of animals, with a view to the detection therein of uric acid. Part II. deals with the formation of uric acid in the animal economy. The rival theories are discussed, and from the consideration of the very large quantities of uric acid, in proportion to the body-weight, excreted by many of the lower animals, as well as the inability of the kidneys to excrete uric acid which has been taken by the mouth or injected into the blood, the author is led to the opinion that the uric acid is a product of changes which take place in the kidneys itself, and is not merely filtered off from the blood. This view receives further support from the fact that, whilst the kidneys excrete ammonium urate, uric acid when found in the blood is in the form of the more stable sodium urate.

It is further shown that when solutions of hippurates are mixed with solutions of urates, the salts exert an influence upon each other, and details of experiments to demonstrate this action are embodied in an appendix.

Linnean Society, February 15.—Sir John Lubbock, Bart., F.R.S., president, in the chair.—Mr. Jenner Weir exhibited a perfect hermaphrodite butterfly (*Lycæna icarus*), and a blue male and brown female of the same species for comparison. The hermaphrodite in question possesses two spotless blue wings on the left, and two spotted brown wings on the right, thus being intermediate in colour between the two sexes.—Dr. W. C. Ondaatje exhibited a collection of thirty species of Ceylon corals, of which twenty were of a stony character. The series agree in the main with those of the Indian fauna; four are new species, viz. two of *Cœloria*, one of *Pavonia*, and one of *Alcyonium*, the two latter however showing most affinity to forms met with in islands of the Pacific Ocean.—Mr. T. Christy called attention to examples of Carnauba palm leaves and to the wax of the tree; and he also showed specimens of a hybrid Primula (*P. japonica* and *P. sinensis*) with double whorls of flowers.—Mr. J. G. Baker read his third contribution to the flora of Madagascar. In this he gives descriptions of the new Incompletæ and Monocotyledons contained in the collections recently made in Madagascar by the Rev. R. Baron and Dr. G. W. Parker. The only new genus is *Cephalophyton*, a Balanophorad used in medicine, of which the material is not complete. Most of the new species belong to widely spread tropical genera, such as *Ficus*, *Loranthus*, and *Croton*. Cape types are represented by *Faurea*, *Peddiea*, *Dais*, *Kniphofia*, and *Dipcadi*, one species of each, and by four Aloes. Of *Obetia*, a genus of arborescent stinging-nettles known only in Madagascar and the neighbouring islands, there are four new species. The Bamboo common in the woods of Imerina proves to be conspecific with that of the interior of Bourbon. There is a curious *Exocarpus* with phyllocladea, nearly allied to species from Norfolk Island and the Malay archipelago.—Mr. C. B. Clarke has contributed a complete synopsis of all the species of *Cyperus* known in Madagascar and the neighbouring islands.—Mr. George Murray read a paper on the outer peridium of *Broomeia*. This gasteromycetous fungus, which is nearly related to *Geaster* consists of a mass of individuals closely seated together on a corky stroma. These individuals have been found up till now with only one peridium, and the Rev. Mr. Berkeley, who first described the plant in 1844, treated the stroma as the homologue of an outer peridium. Mr. Murray has found on some specimens recently brought from Dammara Land a true outer peridium common to all the individuals. From an examination of it he is able to throw light on the mode of development of this fungus.—A paper was read on the "Manna" or Lerp insect of South Australia, by Mr. J. G. Otto Tepper. This contained observations on the insect in question and on the peculiar saccharine substance derived from it, which is deposited on Eucalypt trees.—Mr. W. B. Hemsley read a communication on the synonymy of *Didymoplexis*, and on the elongation of the pedicle of *D. pallens*. The latter saprophyte orchid is widely scattered in tropical Asia, though apparently nowhere very common. It is remarkable for the elongation of its pedicles after flowering. At the time of flowering the pedicles are shorter than the flowers, which are less than half an inch long; but afterwards they elongate, sometimes as much as a foot. The object seems to be to carry the ripening fruit clear of the wet decaying vegetable matter in which the plant grows.

Zoological Society, February 20, W. H. Fowler, F.R.S., president, in the chair.—Prof. F. Jeffrey Bell exhibited a selection of microscopical preparations received from the Zoological Station at Naples, and made some remarks upon them.—Mr. J. J. Weir exhibited and made remarks on an apparently hermaphrodite specimen of *Lycænea icarus*.—Mr. Sclater gave an account of the birds collected by Mr. H. O. Forbes, F.Z.S., during his recent expedition to Timor Laut, and exhibited the specimens. The species were fifty-five in number, sixteen of which were described as new to science under the following names:—*Ninox forbesi*, *Strix sororcula*, *Tanygnathus subaffinis*, *Geoffroius timorlaoensis*, *Monarcha castus*, *Monarcha mundus*, *Rhipidura hamadryas*, *Myiagra fulviventris*, *Micrœca hemixantha*, *Graucalus unimodus*, *Lalage mœsta*, *Pachycephala arctitorquis*, *Dicæum fulgidum*, *Myzomela annabellæ*, *Calornis crassa*, and *Megapodius tenimberensis*. The general facies of the avifauna as thus indicated was stated to be decidedly Papuan, with a slight Timorese element, evidenced by the occurrence of certain species of the genera *Geocichla* and *Erythura*; while the new owl (*Strix sororcula*) was apparently a diminutive form of a peculiar Australian species.—Prof. F. Jeffrey Bell read the second of his series of papers on the Holothuroidea. The present communication contained the descriptions of some new species which the author had discovered while examining the specimens of this group contained in the collection of the British Museum.—Dr. Hans Gadow read a paper on the suctorial apparatus of the Tenuirostres, pointing out that the tubular structure of the tongue in this group is produced by the overgrowth of the horny lingual sheath, the edges of which curl upwards and inwards.—A paper was read by Mr. L. Taczanowski, C.M.Z.S., Curator of the Museum at Warsaw, in which he gave the descriptions of some new species of birds in the collection made by Dr. Raimondi during his recent explorations in Peru. The species in question were seven in number, belonging to six genera, namely, *Carenochrous seebohmi*, *C. dresseri*, *Phytotoma raimondi*, *Ochthœca jelskii Upucerthia pallida*, *Cynanthus griseiventris*, and *Psittacula crassirostris*.—Mr. Taczanowski also read a communication from Dr. Dybowski, in which the sexual differences between the skulls of *Rhytina stelleri* were pointed out.—A communication was read from Mr. G. B. Sowerby, jun., containing the descriptions of nine new species of shells and of the opercula of two known species.

Entomological Society, February 7.—J. W. Dunning, M.A., F.L.S., president, in the chair.—Two Members and one subscriber were elected.—Mr. J. R. Billups exhibited a species of *Conocephalus* which was found in a greenhouse at Lee and kept alive some time.—Mr. F. P. Pascoe read some comments on a letter recently contributed to NATURE by the Duke of Argyll, respecting a moth observed by him at Cannes.—Mr. E. A. Fitch exhibited three species of *Hymenoptera* from Ambarawa, Sumatra.—M. L. Peringuey communicated notes on the habits of three species of *Paussus* observed by him at the Cape of Good Hope.

Mineralogical Society, February 15.—Mr. W. H. Hudleston, F.G.S., president, in the chair.—Prof. Church exhibited and described a specimen of siliceous matter obtained by Mr. Vicary from the Upper Greensand of Haldon, which contained 98 per cent. of silica.—The President then read a paper on a recent hypothesis with respect to the diamond rock of South Africa. A discussion ensued in which Profs. Rupert Jones, John Morris, and Church took part.—A paper from Mr. J. H.

Collins was read on the minerals of Rio Tinto. The President, Prof. Morris, and Mr. Kitto joined in the discussion.

Meteorological Society, February 21.—Mr. J. K. Laughton, F.R.A.S., president, in the chair.—Rev. W. R. C. Adamson, R. P. Coltman, W. F. Gwinnell, Capt. C. S. Hudson, T. Mann, F. G. Treharne, and W. Tyson, were elected Fellows.—The following papers were read:—Notice of a remarkable land fog bank, "the Larry," that occurred at Teignmouth on October 9, 1882, by G. W. Ormerod, M.A., F.M.S. The "Larry" is a dense mass of rolling white land fog, and is confined to the bottom of the Teign Valley, differing therein from the sea fog which rises above the tops of the hills; it appears about day-break, and has an undulating but well-defined upper edge, which leaves the higher part of the hillsides perfectly clear. The author gives an account, illustrated by photographs, of the remarkable fog bank that occurred at Teignmouth on the morning of October 9.—Barometric depressions between the Azores and the continent of Europe, by Capt. J. C. de Brito Capello, Hon. Mem. M.S. The author gives the tracks of several depressions from the Azores to Europe, and shows that if there had been a telegraphic cable, nearly every one of them could have been foretold in England.—Weather forecasts and storm warnings on the coast of South Africa, by Capt. C. M. Hepworth, F.M.S.—Note on the reduction of barometric readings to the gravity of latitude 45°, and its effect on secular gradients, and the calculated height of the neutral plane of pressure in the tropics, by Prof. E. D. Archibald, M.A., F.M.S.

Physical Society, February 24.—Prof. Clifton in the chair.—New members: Prof. A. W. Scott, M.A., Mr. F. E. M. Page, B.Sc.—Mr. Lewis Wright read a paper on the optical combinations of crystalline films, and illustrated it by experiments. He exhibited the beautiful effects of polarisation of light and the Newtonian retardation by means of plates built up of thin mica films and Canada balsam. The wedges thus formed gave effects superior to those of the more expensive selenite and calcite crystals. The original use of such plates is due to Mr. Fox, but Mr. Wright showed many interesting varieties of them, including what he termed his "optical chromotrope," formed by superposing a concave and $\frac{1}{4}$ wave-plate on each other. Norenberg's combined mica and selenite plates were also shown. Mr. Spottiswoode praised the results very highly, and pointed out their value to the teacher and student as showing how the effects can be produced step by step. The phenomena can be shown by an addition to the ordinary microscope, costing some two guineas, as made by Messrs. Swift and Sons.—Mr. Braham then gave an experimental demonstration of the vorticle theory of the solar system by rotating a drop of castor oil and chloroform in water until it threw off other drops as planets.

EDINBURGH

Royal Society, February 19.—Mr. A. Forbes Irvine in the chair.—Mr. G. Auldjo Jamieson read a long and interesting paper on land tenure in Scotland in the olden time, in which the author, after describing in detail the various ancient systems and the survivals of them that exist even now in different parts of the country, strongly deprecated the position taken by some that a return to the old systems would be beneficial.—Prof. Rutherford, in a paper on the microscopical appearances of striped muscular fibre during relaxation and contraction, maintained that the views held generally by physiologists as to which is the contractile portion of the fibre were quite erroneous. A great deal of the inconsistency that seemed to exist was due to difference in the appearance of muscular fibre according as it was relaxed or contracted; and previous observers had been unable to explain this simply because they had not hit upon an effective method of preserving the fibre in either condition. The paper was illustrated by enlarged diagrams and by microscopical preparations of the fibres in various conditions.

BERLIN

Physiological Society, February 9.—Prof. du Bois Reymond in the chair.—Dr. Walten, who was present as a visitor, gave a detailed account of his experiments upon the power of hearing in hysterical, hemianæsthetic persons. He has determined the presence of different degrees of deafness, in cases of partial and complete hemiæsthesias, in addition to the manifold motor and sensory hyperæsthesias and anæsthesias. In all cases anæsthesia of the external auditory meatus and of the membranum tympani existed on the affected side; the lesser degrees of deafness manifested themselves in the same way that senile deafness sets in, *i.e.* in interference with the propagation of sounds through the cranial bones, while direct hearing by the ear was still normal. When a higher degree of hysterical deafness was present, high tones could not be perceived by the ear. In extreme cases deafness is absolute on the affected side. All degrees of uni-lateral hysterical deafness could, like the remainder of the manifestations of hemiæsthesia, be transferred to the healthy side through the operation of a powerful magnet. Dr. Walten was able to measure the gradual decrease of deafness on the affected side and its gradual increase on the healthy side.—Dr. Martius, reasoning by analogy from the fact that a frog's heart cannot contract unless it is bathed in a nutritive fluid, from which it takes the energy required for its work, has tried to determine by experiment if other organs, *e.g.* the brain, require a continual supply of a nutritive fluid in order to keep up their activity. He therefore replaced the blood of frogs by a neutral salt-solution of the strength of 0·6 per cent., with which he washed out the blood-vessels until the fluid ran off free from blood, and as clear as water, and he observed the functions of the central nervous system in these frogs (salt-frogs). It was, however, soon discovered that all the blood had not been removed from the body by driving salt-solution through once, because, when the process was repeated after a few hours' time, the fluid flowed off deeply coloured with blood, and this process had to be renewed very frequently before the blood was reduced to a minimum. Hence, in the experiments with "salt-frogs," the brain was supplied by blood that was more and more diluted, and it reacted as follows:—After washing out the vessels once with salt-solution, the frog behaved like a brainless frog behaves. It sat still and did not make any spontaneous movements; it breathed normally, and exhibited the croak-reflex to perfection. After the vessels had been washed out twice, the croak-reflex had disappeared, and breathing was irregular and intermittent; finally, when the blood was still further diluted, respiration entirely ceased and the general reflex-irritability was greatly increased, as in frogs whose spinal cord is separated from their medulla oblongata. The conclusion to be drawn is that the brain, as well as the heart, requires the presence of a nutritive fluid from which it abstracts the energy for its work. The frogs that were operated upon recovered perfectly from the first stage in a few days' time, but did not recover from the later stages. It is evident that they bore the transfusion of such large quantities of salt-solution very well. On the other hand, they could not be used for experiments upon blood-transfusion, because they died even after very moderate transfusions of blood from other animals.

GÖTTINGEN

Royal Society of Sciences, December 27, 1882.—On an onyx cameo not hitherto known, with a replica of the representations on the upper and middle layers of the large Paris cameo of La Sainte Chapelle, by F. Wieseler.

CONTENTS

PAGE
THE ORIGIN OF CULTIVATED PLANTS 429
OUR BOOK SHELF:—
  Rankine's "Useful Rules and Tables relating to Mensuration, Engineering, Structures, and Materials" 431
LETTERS TO THE EDITOR:—
  Mr. Stevenson's Observations on the Increase of the Velocity of the Wind with the Altitude.—THOMAS STEVENSON . . . 432
  The Supposed Coral-eating Habits of Holothurians.—W. SAVILLE KENT 433
  Influence of a Vacuum on Electricity.—A. M. WORTHINGTON (*With Illustration*) 434
  The Meteoroid of November 17, 1882.—H. DENNIS TAYLOR . . 434
  A Meteor.—R. W. S. GRIFFITH 434
  Aurora.—JOSEPH JOHN MURPHY 434
  Hovering of Birds.—Dr. J. RAE, F.R.S. 434
AMATEURS AND ASTRONOMICAL OBSERVATION. By W. F. DENNING 434
ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT, II. By Dr. T. LAUDER BRUNTON, F.R.S. (*With Illustrations*) 436
THE SHAPES OF LEAVES, I. By GRANT ALLEN (*With Illustrations*) 439
HERRING AND SALMON FISHERIES 442
NOTES 443
OUR ASTRONOMICAL COLUMN:—
  The Comet 1883 *a* 445
  The Great Comet of 1882 446
  The Variable Star U Cephei 446
  New Nebulæ 446
GEOGRAPHICAL NOTES 446
THE CONSERVATION OF EPPING FOREST FROM THE NATURALISTS' STANDPOINT. By RAPHAEL MELDOLA 447
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 449
SCIENTIFIC SERIALS 450
SOCIETIES AND ACADEMIES 450

THURSDAY, MARCH 15, 1883

THE ZOOLOGICAL STATION IN NAPLES

THERE are few of those interested in biological studies who are not more or less familiar with the history and character of the great international laboratory on the shore of the Bay of Naples, which has had so profound an influence on the progress of zoology in the last nine years; scarcely a volume belonging to recent zoological literature, British or foreign, can be taken up, but the acknowledgment of indebtedness to the resources of the Naples station comes under the eye; the publications of the station are on the shelves of most scientific libraries; and many accounts of its organisation have appeared from time to time in scientific periodicals and even in the daily press. But the institution is much too interesting a topic of discussion to be easily exhausted; it is constantly developing and exhibiting new stages of existence. There is soon to be added a new department that of comparative physiology, the work of which will be carried on in a separate laboratory; and on the eve of an expansion so considerable, it is natural to reflect on the work the station has already accomplished, its present state of activity, and the probabilities of its future.

In no branch of zoological science has such rapid and important progress been made in recent years as in embryology, and the investigations into the development of marine forms of all classes by which this progress has been chiefly effected, have been in great part the result of the special facilities which the resources of the Naples station offer for this kind of research. The brilliant career of the lamented Francis Balfour was begun while he occupied, on the opening of the station in 1874, the table rented by Cambridge University. His stay on this occasion lasted from February to June, and resulted in the publication of his first paper, "On the Development of Elasmobranchs," in the *Quarterly Journal of Microscopical Science.* The material for the researches which he continued to carry on at Cambridge on his return was sent from the station. In 1875 he again spent some months at Naples, and again published the results of his work in the same *Quarterly Journal*, this time under the title "A Comparison of the Early Stages in the Development of Vertebrates." The following year he did not visit the station, but in 1877 he investigated there the spinal nerves of Amphioxus, and added to his work on Elasmobranchian embryology. These studies appeared in the *Journal of Anatomy and Physiology*, vols. x. and xi. In the preface to his "Monograph on the Development of Elasmobranchs," which, published in 1878, was at once recognised by all biologists as a classical work, Balfour gratefully acknowledges how much his researches owed to the resources of the zoological station and the support of its *personnel.* It is unnecessary to dilate here on the importance of Balfour's work; the significance of the discoveries which he made, such as the openings of the renal organs into the body cavity in Selachians as in Annelids, the epiblastic origin of the sympathetic system, the history of the blastopore in vertebrates, and its relation to the medullary canal, the head cavities, &c., and the masterly way in which he applied the results of his observations to the solution of the great problems of vertebrate morphology, have given him a place among those whose names mark epochs in the progress of science.

Another English name connected with work in the field of vertebrate embryology which does honour to the Naples station is that of Mr. Milnes Marshall, who has more than once occupied the British Association table. Much of our knowledge of the development of Salpa, the excentric relation of the vertebrates, is due to the work in the station of Professors Salensky and Todaro.

Molluscan embryology has benefited by the existence of the station through the work of Prof. Lankester and the Russian embryologist, Dr. Bobretzky. The former carried on researches in the laboratory in the spring of 1874, and obtained many of the important results which are embodied in his memoir "On the Development of Cephalopoda" (*Quart. Journ. Mic. Sci.* vol. xv.), and his paper "On the Development of Mollusca" (*Phil. Trans.* 1875). Dr. Bobretzky of Kiev occupied the Russian table in 1874, and applied the methods of technical histology to the study of the ova of various Gasteropods, Nassa, Fusus, &c., and of Loligo and other Cephalopods. His Russian memoir on the latter (Moscow, 1877) contains the most complete and reliable series of figures we have of the anatomy of Cephalopod embryos.

In the embryology of sponges, Prof. Oscar Schmidt of Strassburg has published the results of important researches carried on in the station in the years 1875 and 1877. Prof. Selenka of Erlangen worked out the development of various Holothuria at the Bavarian table in 1875, and of Echinidæ in 1879. The work of Dr. Carpenter on the development of Antedon (*Proc. Roy. Soc.* 1876) was done at the British Association table, and the contributions of Dr. Goette to the same subject are based on studies made in the station in 1875. One of the best known of recent studies in development which have proceeded from the station is that of Dr. Spengel, on Bonellia, published in 1879.

Leaving works of a strictly embryological character, we will mention some of the principal contributions to general morphology, which have taken their origin in the station. Prof. Grenacher's great work on the eyes of Arthropods, which forms one of the chief recent additions to our knowledge of the class, is based on researches begun at the Mecklenburg table in 1876. Dr. Hubrecht's researches on Nemertines were carried out at the Dutch table. The contributions to and confusions of Molluscan morphology, which we owe to Von Ihering, proceeded from work done in the station, and both are not without value in the progress towards truth. Dr. Spengel's important paper on the "Geruchsorgan der Mollusken" (*Zeitschr. f. wiss. Zool. Bot.* xxxv.), was produced while he was a member of the staff of the institution. The remarkable volume of the brothers Hertwig, "Die Actinien," describing a nervous system still existing in the primitive condition, was the result of an occupation of two of the German tables.

The honour of the discovery of Symbiosis in animals is shared by two zoologists, who both carried out their researches in the station, Mr. Geddes and Dr. Brandt; and the studies which the latter is still carrying on there have resulted in many other contributions to our knowledge of the Radiolarians.

The investigations of Von Koch into the relations of the skeleton in corals, Flemming's researches on the ova of Echinoderms, Metschnikoff's on Orthonectidæ, those of Dr. Vigelius on the anatomy of Cephalopoda, of Prof. Greef on Alciopidæ, are a few more examples of good work, of which some of the credit belongs to the station. Since the laboratory was opened more than 200 scientific workers have studied at its tables.

Besides this success which the institution has obtained as an international laboratory, it has also produced great results by its own individual activity. A vast amount of complete and careful work is devoted to the preparation of the series of monographs which commenced with the Ctenophoræ of Dr. Chun in 1880. Of these six have appeared—four zoological and two botanical—and a large number, embracing many important classes of animals, are far advanced towards completion. The Planarians, by Dr. Lang, will be received with interest on account of the discoveries and original views which his work has already produced. The Actiniæ are being worked out thoroughly, for the first time, by Dr. Andres. The Sponges, the Radiolarians, the Copepoda, and the Capitellidæ are also at present undergoing a complete study in the station, and two of the volumes already announced will treat of families of Algæ. An enterprise of such magnitude has never before been undertaken in the field of zoological investigation; only an organisation of the power and resources of the station at Naples could attempt it; an organisation which is able to offer to zoologists, of energy and zeal, unlimited material in the living condition, unlimited leisure for work, and immunity from all distractions save some slight duties connected with the routine of the laboratory.

The other two publications of the station are of a less colossal character. The *Mittheilungen* was begun in 1879, for the sake of publishing the numerous discoveries and new views which result from the work of the staff occupied with the "Fauna and Flora," or from the researches of those occupying the rented tables. The circulation has already reached 400 copies, and the few volumes which have appeared constitute a valuable addition to the literature of biology. In its pages are described the new processes in the *technique* of microscopical work which have been invented in the station, one of which, the method of preparing series of sections, devised by Dr. Giestrecht, and now used in every laboratory in Europe, is an improvement whose importance it is impossible to estimate too highly.

The object of the *Zoologischer Jahresbericht* was to supply a bibliographical report, which should not only give a list of published works but a *résumé* of the matter contained in each, and which should give perfect facilities for reference. The latter object is attained by means of two indices—one of the names of authors, the other of subjects. The English *Zoological Record* and the report of the *Archiv fur Naturgeschichte* are devoted chiefly to systematic zoology; in the *Jahresbericht* every publication on anatomy, embryology, morphology, or physiology, is catalogued and summarised.

In contrast with the activity exhibited by the station in the directions which we have hitherto considered, activity whose results are as conspicuous as they are important, is the unobtrusive work of the department presided over by the energetic conservator, Salvatore Lo Bianco,—the department for the preservation and distribution of marine animals. All the material procured by the expeditions of the two steam launches, and the smaller boats belonging to the station, or by purchase from Neapolitan fishermen, passes first into the control of this department. Whatever is needed by the various occupants of the work-tables and by the scientific staff is selected and allotted according to applications made from day to day. The rest is either put into the tanks of the public aquarium, or preserved. Marvellous progress has been made in the art of preserving delicate and sensitive creatures in their naturally extended condition, and inland laboratories can be provided with specimens of Alcyonaria, Zoantharia, Medusæ, Ctenophora, Annelids, &c., which show the form if not the colour of the living animal, and in which all the organs are in a perfect condition for anatomical and generally even histological study.

There is scarcely a biological laboratory in Europe which has not had recourse to the preparing department of the Naples station in order to procure material for investigation or for teaching purposes. An example of the work of the department is to be exhibited in the approaching International Fisheries Exhibition—a most beautiful collection of preparations is now in the station, ready to be sent to London.

In connection with this department arrangements have been made with the naval authorities of Germany and Italy, by means of which an officer is sent from time to time to the station to learn the methods of obtaining and treating marine creatures for the purpose of scientific study; so that the cruises of war-ships in remote seas may contribute to valuable scientific results when each has an officer on board who understands what is of zoological interest and how it should be preserved.

In conclusion it will be of interest to give a few details concerning the finances and arrangements of the station. The annual income is between 5000*l.* and 6000*l.*, of which 1200*l.* is derived from the public aquarium, 1600*l.* from the rented tables, about 800*l.* from the sale of the publications, including 260 annual subscriptions of 50*s.* each for the monographs, 600*l.* from the preparation department, and 1500*l.* is the amount of the German Government subsidy.

The total number of those in the permanent service of the station is thirty-seven, of which eight comprise the scientific staff, and the rest are made up by the engineers under the direction of Mr. Petersen, the fishermen, and the conservator and his assistants. The number of tables at present rented is twenty-one, but the station has space for thirty. At the beginning only seven tables were taken, two each by Prussia, England, and Italy, and one by Holland. The School of Biology at Cambridge has derived much support and benefit from its connection with the station, and the taking of a table by Oxford would probably give to zoological studies there an impetus which is much needed. Of the few zoologists which Oxford produces, some have already had recourse to the British Association table. It is probable that some one of the many rich institutions in America will soon take a table for the use of American zoologists, many of whom, imperfectly acquainted with the organisation of the station, and therefore unaware that no table can be occupied unless taken either by a corporation or a private individual for a whole year, have applied for permission to work there. Last year Mr. Whitman, whose observations on the development of Clepsine are well known, received this permission under special circumstances by the courtesy of the staff, and carried out some excellent researches on Dicæmidæ, which are published in the last number of the *Mittheilungen*. Recently an increased number of similar applications have been received from American zoologists.

In speaking of the arrangements of the station, the perfection of the organisation for the supply of material, by means of the dredging and fishing of the gulf, cannot be too warmly praised or admired. Except in continuously bad weather, the beautiful and wonderful creatures comprising the rich Mediterranean fauna are brought in to the station in an abundance that is perfectly bewildering to a zoologist on his first visit. The possession of two steam launches, the larger of which, the *Johannes Müller*, was given by the Berlin Academy in 1877, while the smaller was purchased subsequently, gives to the fishing department the facilities for rapid locomotion and transport, without which such abundance and perfect condition of the living material could not be obtained; especially as some of the most fruitful localities are widely separated, and a great many of the creatures, including all pelagic forms, are of extreme sensitiveness and delicacy.

The zoological station, although only nine years have passed since its first opening, has become a necessity for the progress of zoology; its international character enables every country to contribute to its support and share in the benefits derived from it; it is a great organisation by which forces of various kinds are brought together to aid in the attainment of one great object, the investigation of the facts and phenomena of marine life in all its diversities, and their explanation in accordance with the principles of evolution. The progress which is brought about by the work actually done in the station is not more important than the indirect influence it exerts in various ways; its example has produced similar enterprises in various parts of the world; the benefit of the experience it gains extends to other centres of scientific research, and other branches of biology than marine zoology, and by its own vitality and its influence on the zoologists who study at its tables it has done much to sustain and develop the great impulse which the genius of Darwin gave to zoology twenty-three years ago. J. T. CUNNINGHAM

EPPING FOREST

THE House of Commons divided last Monday afternoon upon the Chingford and High Beech Railway Bill. An amendment was proposed by Mr. Bryce, Chairman of the Commons Preservation Society, and was supported by Mr. Thorold Rogers, Sir H. J. Selwin-Ibbetson, who framed the Epping Forest Act of 1878, Mr. Fowler, Mr. Firth, Mr. T. C. Baring, Lord Eustace Cecil, Mr. Ritchie, Mr. James, Mr. Caine, and Mr. Waddy. As a fitting sequel to Mr. Meldola's paper, which we published last week, the result of the division, which was announced amidst cheers, was: For the second reading, 82; against it, 230; majority against the Bill, 148. It is to be hoped that this will be the last attempt to tamper with what Mr. Bryce justly described as "a priceless heritage of the people of London."

It is inevitable from the growth of our great towns that the student of Nature dwelling in their midst must go farther and farther afield for the objects of his study. It seems, moreover, that our science is at present inadequate to prevent the lethal influence of smoke and acrid fumes from dealing destruction to vegetation over a wide region outside the actual boundaries of these towns. The sanitary necessity of open spaces has been amply demonstrated; but it was not as a mere open space or people's park that Parliament allowed the Corporation of London to acquire Epping Forest in 1878.

The so-called rights of those who had inclosed the Forest, were overridden in order that an expanse of natural and, in some senses, primeval forest might be secured for the benefit of all classes of the public free from encroachment for ever. Parliament directed that it was to be preserved "in its natural condition as a forest," and conferred upon a Committee—composed of some members of that Corporation which holds the manorial rights, together with four resident gentlemen as Verderers, elected nominally by the commoners—the position of Conservators.

Unfortunately Common Councilmen seem to share the popular ignorance as to what constitutes the natural aspect of a forest. Many people believe a forest to be a large wood or plantation, and the Conservators seem to have been mainly actuated by fears lest visitors should get their feet wet or find the Forest less amusing than other suburban resorts. Draining and roadmaking have been their main tasks with a view to maintain the natural aspect the Forest wore for centuries, while during the five years they have been in office no attempt has been made at reafforesting the now unsightly fallows that the intruders had reduced into an arable condition. Pieces of artificial water have been constructed, mostly with outlines reminding one of the so-called Round Pond in Kensington Gardens; pleasure-boats have been licensed upon them at a rental estimated at over 200*l.* per annum; free displays of fireworks in connection with a huge tavern, shooting-galleries, and steam-roundabouts have been authorised as contributing to a truly ideal forest.

These steps have of course been taken with the idea that the Conservators had the power to act in the way they think best calculated to elevate and refine the working-classes; but they are diametrically opposed to the spirit of the Act of 1878, which did not aim at establishing a tea-garden or at pandering to the lowest tastes of any class of the community.

As is seen from Mr. Meldola's article, the Essex Field Club and other scientific societies have more than once protested against such mismanagement; but the Conservators had not yet filled up the full measure of their iniquities. They must promote a railway, if not a tramway as well.

English public opinion is beginning to awaken to the idea that we have now almost as many railways as are required for any purposes but providing fees for directors and engineers and feeding the jealousies of rival companies. In the present session of Parliament the railway companies have evinced in the Bills they are promoting a partiality for common land that would be remarkable were not the reason for it sufficiently obvious. Common land can be had cheap; for it is everybody's business to oppose its spoliation, and everybody's business is proverbially nobody's. It is to be hoped, however, that the knell of these schemes was sounded on Monday last, when the House of Commons, on the motion of leading men of both parties, rejected the Chingford and High Beech Extension Bill, promoted by the Corporation and the Great Eastern Railway, by an overwhelming majority.

The House was fully aware that the line then proposed by Sir Thomas Chambers and Lord Claud Hamilton was only the first section of a longer one which would ultimately surround the Forest, and that it was intended to serve at first mainly as a feeder to another large tavern All lovers of nature will rejoice that the collecting ground of Edward Forster, the Doubledays, and thousands of London naturalists less known to fame, has been rescued from destruction.

Authorities inform us that lopping and smoke have reduced the number of lichens and insects even during the last twenty years, and Conservatorial draining may have a similar effect upon other groups of organisms, so that the help of a railway in the work of devastation is certainly not required.

It is to be hoped that the verdict of Parliament will show the Conservators that forest management has a scientific basis and that their powers are not unlimited. It is equally desirable that the public interested in the Forest will form some organisation for its protection from encroachment and mismanagement in the future, so as to relieve a scientific body such as the Essex Field Club, which has borne the chief labour of opposition, from a task which, from its political and litigious character, must necessarily be uncongenial.

G. S. BOULGER

PERRY'S "PRACTICAL MECHANICS"

Practical Mechanics. By John Perry, M.E. (London: Cassell, Petter, and Galpin, 1883.)

THIS book is one of a series of manuals now being published by Messrs. Cassell and Co., intended for the use of technical students, and claims, to quote the preface, "to put before non-mathematical readers a *method* of studying mechanics," which, if carefully followed, will supply "a mental training of a kind not inferior to that the belief in which retains in our schools the study of ancient classics and Euclid." A principal feature of the method consists in "proving" the various formulæ of mechanics by quantitative experiments. Of these many are described in the book, several of which, such as those relating to torsion and other stresses, &c., are carried on in many physical laboratories, and belong rather to physics than to mechanics. Another feature of the method more novel than the last is the gathering together of a few of the definitions and elementary theorems of mechanics, such as the parallelogram of forces, in a chapter at the end of the book called a glossary. Even then no formal proofs are given, probably because they are unnecessary, since on p. 2 we are told that the reader "cannot know the parallelogram of forces till he has proved the truth of the law half a dozen times experimentally with his own hands."

This kind of proof is very different from the evidence usually tendered for the fundamental laws of mechanics, but we must not forget the class of readers, entirely different as they seem to be from any we have ever encountered, for whom the book is intended. We are reminded of this on p. vii., when we are told that "the standpoint of an experienced workman in the nineteenth century is very different from that of an Alexandrian philosopher or of an English schoolboy, and many men who energetically begin the study of Euclid give it up after a year or two in disgust, because at the end they have only arrived at results which they knew experimentally long ago."

Thus the empire of the Greeks in geometry must give place to the supremacy of the intelligence of the working man, and even Euclid himself must fall from his high estate to be compared and contrasted with the modern schoolboy. But this latest born of time apparently possesses even higher powers. If made "to work in wood and metal," "to gain experience in the use of machines and use drawing instruments and scales," he will arrive at a condition in which "he may regard the 47th proposition of the First Book of Euclid as axiomatic," and "he may think the important propositions in the Sixth Book as easy to believe in as those in the First." Truly here at last has been found in geometry a royal road. But when Prof. Perry has raised our opinion of the modern schoolboy and working man to this high eminence we feel a rude shock on reading the second page of the book, when we discover that these rarely gifted, ideal beings, so favoured of the gods in geometry, may perhaps not be able to apply to a practical example a simple algebraical rule.

In reading the book, especially in its earlier chapters, we are struck by the want of logical arrangement and of strictness in the definitions, by the frequent use of terms which have not been previously defined, or not adequately defined, and of writing so careless in its style as frequently to become unintelligible. The theory of friction, in the limited extent to which alone it is given, is inserted piecemeal into parts of the two first chapters and into the glossary, and the ordinary laws are not explicitly given until nearly the end of the book, but in their place we have the loose statements, "friction is proportional to load," and "friction is a passive force, which always helps the weaker to produce a balance." The English of the last sentence is as curious in character as that of one on p. 13, "This rubbing is a very slow motion."

The doctrine of the conservation of energy or of the conversion of energy into heat is nowhere explicitly given, although the theory is assumed in numerous applications. Can it be that the modern schoolboy, duly equipped, is able not only to surpass Pythagoras by regarding the 47th proposition of Euclid as axiomatic, but that he has come to view the great physical theory as equally self-evident? It must be so; otherwise, having only been told of energy as the equivalent of mechanical work (p. 5), he would not understand the meaning of the obscure sentence—"Every experiment we can make shows that energy is indestructible, and consequently, if I give energy to a machine, and find that none remains in it, it must all have been given out by the machine."

We find the leading laws of hydrostatics inserted in a paragraph on water, which is included in the chapter on materials, fifty pages after the uniform transmission of fluid pressure has been assumed in the article on the hydraulic press, and we are told (*note*, p. 75) that a cubic foot of water possesses, "in virtue of the steadiness of the motion, pressure or potential energy," &c. On p. 74 "total pressure" is used for resultant pressure. Nowhere throughout the book is the theory of the centre of gravity given, or the name even defined, yet the author—to the chagrin of any student who believes it—does not hesitate on p. 142 to preface with the words "it is evident" an application of the usual formulæ defining the position of the centre of gravity to the case in hand. The term "radius of gyration" is used on p. 144, but not defined until p. 196. The statement that "velocity is the speed with which a body moves" reminds one of Lord Palmerston's definition of an archdeacon, and we wonder what kind of notion will be gained of the motion of a body in a curve by any one who is told in a definition of centrifugal force that, "if a body is compelled to move in a curved path, it exerts a force directed outwards from the centre." We have also the following as a definition of the pitch circle:—"Two spur wheels enter some distance into one another, and the circle on one which touches a circle on the other, the diameters of these circles being proportional to the numbers of teeth on the wheels, is called the pitch circle." Could even the common sense of high quality, postulated of the readers of the book, enable them to select, from the infinite number of pairs of circles satisfying the above conditions, those which represent the pitch circles required?

In the rule for the differential pulley block we are surprised to find that the movable pulley rises through the whole, instead of half, the difference of the amounts of rope uncoiled from the two pulleys in the upper block. On p. 30 it is said:—"In the study of the motion of a slide valve it is much too usual to assume that the piston's motion is what is shown in Fig. 18 as pure harmonic motion." How shall we reconcile this with the information we have already received on the previous page that Fig. 18 (a skeleton drawing of a crank and connecting rod) does not represent pure (why not "simple?") harmonic motion except when the connecting rod is infinitely long?

In the rule which is inserted on p. 46 to find M, the constant should be twice that given, or about 59,500. On p. 64 our powers of comprehension are baffled in endeavouring to attach a meaning to the assurance that "50 foot-pounds is the total energy stored up in the wire *in the shape of a strain*." (The italics are ours.) In the rule given in Art. 192—we presume for *perfectly* elastic bodies—the momentum communicated from the one body to the other is just twice that stated.

We are told (p. 193) that the motion of a point in the balance of a watch is very nearly pure harmonic, if we suppose the point to move in a straight line instead of a circle, but we confess that the advantage of so describing the motion is not apparent, nor should we be disposed to call the friction in a twisted wire fluid friction (p. 199) because the friction in this case, as in that of fluids moving slowly, is proportional to the velocity.

The long array of mistakes given above, which by no means exhausts our list, forms a very serious accusation against the author.

His book has much disappointed us, for although some of the chapters, such as those on shear and twist, beams, graphical statics, and spiral springs, treat in a simple manner subjects which in parts present some difficulty, yet the defects to which we have alluded are far too grave to be compensated by any excellence in particular parts of the work. In the earlier chapters especially, the author has failed in the fundamental excellences of book-writing, in logical arrangement and clearness and exactness of expression, in just those qualities in fact in which he would have been most successful if he had aimed at writing more from "the standpoint of an Alexandrian philosopher."

J. F. MAIN

OUR BOOK SHELF

Der Norske nord-hass-expedition, 1876-1878. VIII. Zoologi, Mollusca. I. *Buccinidæ*, ved Herman Friele. Med 6 plancher og 1 kart. 4to. (Christiania: Gröndahl and Sons, 1882.)

I HAVE already, in the *Annals and Magazine of Natural History* for this month, given some account of the scientific expeditions which were made by the Norwegian Government during the years 1876, 1877, and 1878, to explore the sea-bed lying between the coasts of Western and Upper Norway and Iceland, Jan Mayen, and Spitzbergen; and I also noticed the series of publications which embody the result of these expeditions, including the present volume. I now propose to say a few more words on the subject of Herr Friele's work.

The great family of *Buccinum*, which is treated in it, is most perplexing in a taxonomical point of view; and its generic type, *Buccinum undatum*, is so unusually prolific and abundant, and consequently so variable, that no two conchologists agree as to the number of species belonging to it. In a short paper of mine on the northern species of *Buccinum*, which appeared in the *Annals* for December 1880, I ventured to consider as varieties of that species and of *B. grœnlandicum* (which is probably also a variety of the polymorphous *B. undatum*) no fewer than 25 other so-called species. Such amalgamation will doubtless not be admitted by many conchologists; but the examination and careful comparison of an immense number of specimens from all parts of the North Atlantic which have fallen under my examination, warrant me in forming the above opinion. If we were to substitute the German word "gestalt" or form for species, subspecies, and varieties, it might perhaps be a more safe and convenient mode of definition; but naturalists are not yet prepared to change the time-honoured system of Linnean and Lamarckian classification.

Herr Friele's work and the other publications to which I have referred are written in excellent English, as well as in his native language. The descriptions of new species are in Latin, which is scarcely so well adapted as English or French for the terminology of natural history at the present time; although his descriptions are far superior to the barbarous if not illiterate productions of Reeve and some other modern conchologists. The distinctive characters of new species are for the most part given in the same order, so that the description of one species can be more easily compared with that of a congener. This is an important and nearly indispensable desideratum. One new genus (*Jumala*) is proposed, having *Fusus Turtoni* for its type; and it appears to be based on Prof. G. O. Sars's description of the odontophore or dentition. Ten species are also for the first time described and figured, viz. one of *Jumala*, seven of *Neptunea*, and two of *Buccinum*. I regret that I must disagree with my friend the author as to the number of genera (six) into which he has divided the northern species of *Buccinidæ*. I should be disposed to attach more value to the operculum than to the odontophore as a generic character. Nor can I accept all his new species. The species which he considers my *Fusus curtus* is very different from the *F. Sabini* of Gray, or the *F. togatus* and *F. Pfaffi* of Mörch (all enumerated by Friele as synonyms); and I regard the last-named three species as the *F. ebur* of Mörch and not as my *F. Sarsi*. However, notwithstanding any trifling errors, if they be errors, the work of Herr Friele is not only admirable and valuable, but is imbued with that scientific merit and modesty which are peculiar to our fellow-workers in Scandinavia; and we shall look forward with great interest to the continuation of his papers on the Mollusca of the Norwegian North-Sea Expedition.

J. GWYN JEFFREYS

Tables for the Use of Students and Beginners in Vegetable Histology. By D. P. Penhallow, B.S., late Professor of Chemistry and Botany in the Imperial College of Agriculture, Japan. (Boston, 1882.)

THIS little work by no means meets the expectations which its title arouses. The author states, indeed, in his preface that the scope of the work is purposely limited, but the limits are so narrow that the work will not be of much use to the student who has a competent teacher, and it will not be of any use to the beginner who is attempting the study of vegetable histology by himself. The book deals simply with the micro-chemistry of plants; the reagents are enumerated, as are also the various substances to be met with in the cells, but no attempt is made to give an account of the mode of application of the reagents for the detection of the substances, and in certain important cases (the chloriodide of zinc, for example) the mode of preparation of the reagent is not given. Not a word is said about imbedding, nor is any mention made of staining. The general mode of treatment of the subject is thoroughly unpractical. For example, silica is said to appear in plants "as a transparent deposit"; but every histologist knows that the silica in a cell-wall can only be made evident by incinerating with nitric acid.

The priority which the author claims can hardly be granted in view of the fact that Poulsen's valuable "Microchemie" has been in the hands of European histologists for several years. The selection of literature given at the end also betrays the author's want of acquaintance with his subject, inasmuch as no mention is made of such important works as Dippel's "Mikroskop" and De Bary's "Vergleichende Anatomie."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Matter of Space

IN his paper on "The Matter of Space," in NATURE, vol. xxvii. p. 349, Mr. Charles Morris has given us an excellent exposition, and, as I believe, in general a perfectly correct one, of the fundamental laws and properties of matter and motion. But as I have for some time been investigating the views which he describes with exactly the results and consequences at which he has arrived (excepting only in one material difference to which I will presently return), a little outline of the mathematical form which I found that the discussion of the subject could receive, and to which it was accordingly submitted in my examinations of its scope and contours, will aid readers of Mr. Morris's paper, perhaps, in attaching clear ideas to some of the expressions which he uses, and in thereby discussing and estimating very easily and fairly the positive truth, in general, or in a few points, of the paper's considerations, the just degree of reliability at all events, which the marvellous maze of internetted motions possesses, which he has most tersely and graphically, and at least in the main, as it appears to me, correctly and truthfully described.

Angular momentum, or (for a particle of unit mass) the rate of description of sectorial areas, is, like actual energy, a quantity of two dimensions in space; it is in fact the vector-product of (or the quadrilateral area between) the two radii of the particle's orbit and hodograph. Tractive momentum, or the product of the unit-particle's radius-vector and the resolved part of the particle's velocity *along* (instead of across) the radius-vector, is equally a quadratic product (but differently estimated) of the two foregoing orbit and hodograph radii. It is not the rate of description of *an area*, like angular momentum, but the time-rate of the square on the orbit-radius. The time-rates of each of these momenta are similar to them in space-relation, and are respectively angular moment or twirl (of a force-couple) and tractive moment or wrest (of a motor-couple). But if a small step of angle is the ratio of a circular-arc step (or of a small step along its tangent) to the circle's radius, this being numerical, a twirl's work through this small step of angle is similar in space-relation to the twirl itself and to its time-effect, or angular momentum.

The same similitude in space-relation will exist between a wrest, or motor-couple, and its time-effect (or tractive momentum), and its small step of work, if, in imitation of the practice for a twirl's or force-couple's action, a wrest's space-step is defined to be the ratio of the particle's *step along* the radius to the orbit-radius. This counterpart of angle-step may be called a traction-step; and it is the small percentage of elongation which the radius undergoes. If this construction is assumed, there ensues from it a close, and evidently significant, analogy between the time-rate of *orbit-radius* square (which denotes at once, in space-relation, a *motor*-couple and its time- and space-effects) and the *hodograph-radius* square (which expresses simultaneously in space-relation a *force*-couple and its time- and space-effects). Although the square of the hodograph-radius signifies the square of the material point's velocity, or its *directed* actual energy, I conceive that the square of the orbit-radius represents a square of undirected velocity, or an undirected energy of "higgledy piggledy" motion of the material point; and its time-rate is *a horse-power of* the point's quaquaversal, or undirected actual energy. Viewed in this light, twirls or force-couples and their time- and space-effects are all graphically synonymous with actual directed energy; but wrests or motor-couples and *their* time- and space-effects are all graphically synonymous with *horse-power* of *undirected* actual energy. For these latter quantities Mr. Morris uses indifferently the various words, "momentum," "heat momentum," "heat velocity," "heat," "motor energy," "heat energy," "heat vibration," "centrifugal energy," and "centrifugal or motor vigour," of a moving point; but while they are all, as he rightly opines, convertible quantities in their relation to graphic space, yet the theory of force-couples with which (*mutato nomine*) they are equally convertible in the same space, teaches us that a twirl-group falls mechanically, according to its association with time and angle, into three distinct divisions, of an action (the couple) and its time- and space-effects (angular momentum and accumulated work). It is so also with the motor-couple's graphic-space measure, "vigour." In proper combinations with time and traction-ratio[1] it becomes either an action or a kind of momentum or a form of work. But in discussing these new quantities' properties two maxims of construction and interpretation must be kept constantly in view.

In the first place, we must not expect a motor-couple (although it tends to alter ϕ) which endows a point with undirected horse-power, to tend to lengthen or shorten the point's radius-vector *in the same way* that a force would do. If by their actions motor-couples can *in any way* oppose the action of a force or force-couple, it must be, not by exerting force themselves, but by giving rise to force where they act. Now motor-couples can no more act intelligibly upon a single point (to range a radius's extremities towards or from each other) than a force-couple can (to turn a radius's two ends round each other). Hence motor-couples must produce force in a material point in virtue of the point's being an aggregation of material points, or in other words the appearance of force is a sign of the compositeness of the material point upon which it acts. *Per contra*, forces can produce force-couples, or if properly combined can balance them, on a collection of material points, if certain internal conditions (always including conservation of force-effects and conservation of twirl-effects) of the component points' mutual force and couple-actions on each other, which we call certain static relations of the system, are fulfilled; and then we have forces on such an aggregation either giving rise to or holding in check force-couples acting on it. But no combination of force-couples, on the other hand, can either produce in the system, or resist in it, the action of a single force.

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[1] The integral of traction-ratio, $\int d\phi = \int \frac{dr}{r} = \log \frac{r}{c} = \phi$, I identify with Rankine's "thermodynamic function" (for which he uses the same symbol, φ) usually termed "entropy" in works on thermodynamics.

Now as a motor-couple and its parts exert time-flow of one form of energy, they differ from a force couple and its parts in the same way that these differ from uniform rotation and translation; and as it happens that while rotations can combine on a system to produce translation, and not the opposite arrangement, and just the reverse of this relation prevails in force-couples and their forces, so we may infer that in a system of connected points motor-couples would have the opposite property to force-couples, and in combination together on the system, instead of being produced by, they would either wholly or partially produce, a form of resultant of the nature of a motor-couple part. This kind of resultant, too, will exert a tendency on the system as a whole, with equal and similar intensity at all its points. Such a combination of motor-couples on a body, therefore, will in general communicate to it by their conjunction, not horse-power of undirected actual energy in the same manner as a single couple would, but some or no resultant couple-part, and some or no resultant couple, just as a set of forces, (or rotations) applied to a body may yield a mixed resultant of a force and couple (or of a rotation and translation), the couple in one case and the translation in the other both taking effect upon the body as a whole, since each is quite devoid of any particular point of application in the body. This property, which we may reasonably assign to motor-couples, of furnishing in combinations on a group of material points a dual resultant in general, and the condition that they exert singly a time-flow of undirected energy, are together the first maxim to be kept in view in discussing their effects; for the double-resultant's nature, of a congregation of motor-couples, in general resembles that of a screw's motion, which is partly translative along and partly rotative round a polar axis. Along a given line through the system therefore this resultant acts jointly, partly as a wrest, or residual motor-couple, and partly as a couple-half, of whose nature and effects no attempt, in what precedes, has yet been made to give an explanation.

Although such views as these of matter and motion largely invite investigation, it is rather their conformity to observation and to such slender mathematical evidence as is derivable from the laws of graphic space than any rigid demonstration of their validity which has led me to put faith in them. Where time and entropy (which linear dilatation is above surmised to be) clasp and bind undirected energy in new ties of space, so singularly like but yet distinct from the well-known ones which regulate the transformations of directed energy, intrusion into the mathematical avenues of the problem is almost warded off by the obduracy of the new inquiry, and only scattered fields of cultivation, occurring ever and again along his road, assure the venturesome wanderer in the new tract that the course before him still always lies in habitable regions.

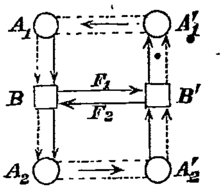
It would be presumptuous therefore to insist, until the mathematical field has been thoroughly explored, upon a preference of one view or hypothesis of motor-actions, as decidedly superior to another; but adopting, as Mr. Morris does, the opinion that the effects of motor-actions are conserved, and adding to this an assumption that in groups of points subjected to them the mutual conservation may not be (as it always is among the mechanical connecting forces of every piece of ponderable matter) perfect and complete in the system by itself, *without* reckoning on to it one other external point, then a material simplification of the views unfolded in his paper would, I believe, be introduced, by adopting a different hypothesis from that which Mr. Morris advances of the nature of the ether as an exceedingly attenuated form or "fourth state" of gross "gravitating" (or ponderable) matter.

If Nature's course could be retraced to the beginning of time, we may suppose in that *gouffre* of antiquity ether to have been differentiated from gross matter in this way, that whereas internal conservation of motor-effects suffices to weld a group of material points into a resultant yielding system, then, no limits of smallness being imposed upon the group, it is allowable to define a point (in the language of graphic space) of gross or ponderable "gravitating" matter as an originally differentiated mass of aerilian points, upon which the dual resultant of the conjoined motor-couples on the aerilian points will take effect. A part of this resultant is a couple-half, about which we know nothing; and we may reasonably suppose it to be attractive and repulsive force acting on the baric point. The other part of the resultant is an unbalanced motor-couple, only susceptible of conservation as to its effects by an equal and opposite one in some other similar mass or aerilian assemblage. A certain integrity can, I conceive, be imparted to this first hoard of scaffolding of the new theory's construction, by locating the conserving couples, of which the motor-couples supposed to be aboriginally welded together are the counter-equivalents, not in a single, but partly in one, and partly in another, set of free-moving aerilian points in such a way that, while the resultant motor-*couple* is balanced by the first set, the *force*-resultant of the massed couples' combination, will be balanced *through* a counter-equivalent force-resultant in another mass-point *by* the free motor-couples of the other aerilian set. The residue of this set's couples will be occupied in opposing the unbalanced couple-residue of the couples massed together in the second baric point, while these couples' transmittent force-resultant will be opposed *by* the still uncompensated portion of the motor-couples acting on the first free-moving set. Perfect compensation of the two dual resultants cannot then take place under these conditions, without exact counter-equivalence of the half-couple (or force-) resultants, and therefore also exact counter-equivalence in their native state between the two groups of motor-couples acting on the two free aerilian sets; at least, if we assume massed and moored aerilian points to have been all originally-endowed in pairs with equal counter-couples, and if their modes of collection into mass-points and of producing force-resultants were aboriginally all alike.

In our present undeveloped knowledge of the mathematical properties of tractive or motor-couples, and of their random-energy horse-powers' geometrical relations to the common mechanical modes of exertion of directed energy in forces and couples, it would be premature and vain to speculate as Mr. Morris does, I believe too boldly and fearlessly, in his paper, upon Nature's established order of progressive collection of baric points into "spheres," or into the atoms and molecules which further build up atmospheres, suns, planets, and all ponderable bodies. My views diverge here from his in, at least, one salient point, that the ether (as we must still in sober science term his "interspheral matter") is held, in his opinion, to be ponderable or "gravitating," and to be endowed with a vigour of motion which exempts it from yielding to its vigour of gravitation. By thus identifying "interspheral matter's" or the ether's particles with those of matter "employing its motion secondarily about new centres of gravity" (of *really* gravitating or ponderable "spheres"), the way is barred at once of explaining the ultimate sources of attractive and repulsive force by exercises of motor "vigour." But further than this we must evidently abandon definitely all reasonable hope of constructing out of particles' "incessant leaps in nodes of an interminable network of motions, affecting in long motor lines myriads of interspheral particles," any intelligible framework of the important laws of radiation, magnetism, and electricity which we know that a clear comprehension of the "interspheral" ether's real constitution would immediately unfold to us, if its real nature and that of its relations to ponderable matter were rightly understood. In the form therefore in which Mr. Morris's theory presents itself to us, it fails completely (by only the slightest possible illusion, as I venture to submit, in the choice of its principal hypothesis) in attaining the admirably well pursued and well nigh compassed object of its otherwise exhaustively clear and excellently propounded arguments and demonstrations.

In the view which I have here advanced, massed assemblages of aerilian points form irrevocably the points of gross or ponderable matter, while an equal number of moored points, inseparably connected two and two with the former ones, form bound aerilian assemblages equally untransformable and forming active individual parts of the unchanging ether. That the latter points, unlike those of the massed group, may rove at large in graphic space, does not preclude them from all occupying a common point in another space domain, just as a number of balloons may be all at one height, whatever the courses of their tracks upon a map may be. Nor, again, does an encounter of two balloons' courses on a map necessarily entail collision between the two balloons, since at the time they may be at different heights. It is thus quite conceivable that, in a scale of space foreign to our graphic measures, the free roving aërilian set may all occupy a common place in this foreign scale of space, and that a massed and a moored aërilian point may have the same position in graphic space without impinging on each other, as such points are not at the time at one and the same place in the foreign space. The moored or bound ether may thus traverse the space occupied by the massed ether of gross matter without mutual interference; but, whether superposed or not in ordinary space, the pair of ether sets which compensate resultant actions of two gross-clustered sets of a pair of points of baric matter, will form, however they may mingle graphically, two orbs of ether exerting each (corporately) exactly counter-equal free-orb couples.

If for example the baric points B, B′ are urged towards each other with a ponderomotive force or flow of ordinary baric momentum F_1, by the motor actions on them of two ether-orbs A_1 A_1' in counter equal intensities, force-momentum only will be transferred from A_1 to A_1'; while the tractive momentum (or as we may presume, the heat-energy) accompanying it will only be transferred from A_1 to B, and a similar transfer of thermal energy or tractive-momentum will at the same time take place from B′ to A_1'.



Should it, in the next place, be required to oppose the action F_1 by an equal counter-force F_2, a pair of ether-orbs A_2 A_2' must be superadded to those already urging B and B_1', so as to urge them in the opposite direction. It will be seen from the figure that the total effect of this and of the previous orb-pair's actions will simply be that the pair of ether-orbs connected with B will transmit motor energy from one to the other (from A_1 to A_2), and the other ether-pair will also transfer an equal amount of energy contrariwise from one orb to the other, without any leakage of ordinary momentum occurring at B and B_1', by the neutralised action, into the channel B B′.

Newcastle-on-Tyne, February 10 A. S. HERSCHEL

(*To be continued.*)

Terrestrial Radiation and Prof. Tyndall's Observations

IN NATURE, vol. xxvii. p. 377, I see a notice on Prof. Tyndall's observations on terrestrial radiation, with the author's concluding remarks, that meteorologists should not be offended by his saying that from outsiders equipped with the necessary physical knowledge they may expect valuable aid towards introducing order and causality among their observations. May I be permitted to state that Prof. Tyndall will give no offence, at least to the meteorologists whose works are advancing this science at the present time.

Prof. Tyndall tries to prove by his observations the extreme importance of vapour of water as a check to terrestrial radiation, and he mentions the much greater difference between a thermometer in the air four feet from the ground and another on cotton wool on a morning when snow was lying on the ground than on other nights, equally clear, but with higher temperatures of the air and no snow. Now it is well known that, *pari passu*, a surface of snow will be colder than a surface without, because (1) snow is an excellent radiator; (2) because, as a very bad conductor, it shelters the surface from the influence of the higher temperature of the soil. In the observation on December 10, the thermometer on cotton wool was so cold because it was under the influence of the cold radiated by the snow, and besides immersed, so to say, in the coldest stratum of air near the ground. To my mind, the manner in which the observation was conducted does not prove what Prof. Tyndall advances. To isolate, so to say, the influence on radiation of the atmosphere itself, he should have placed, between two poles, at some feet above the ground, a plank, and on it his cotton wool and thermometer. No doubt that this thermometer, isolated from the snow, should have shown a higher temperature than his thermometer placed on the surface on cotton wool.

Prof. Tyndall lays great stress on the fact that the difference between the temperature in the air and on the ground was less in clear nights with a higher temperature and greater quantity of vapour of water in the air, and sees in this a confirmation of his opinion on the great influence of vapour of water in checking radiation. I do also see in this the influence of vapour of water, *but not of its absolute quantity*, but of *relative humidity*. Once the dew-point is attained, the cooling of the thermometer on the ground is arrested. The whole question between Prof. Tyndall and many physicists and meteorologists is this: nobody negates the influence of vapour of water on terrestrial radiation, but Prof. Tyndall ascribes this influence to vapour in the gaseous state, while his opponents hold the opinion that in this state vapour of water has a diathermacy scarcely different from dry air, while, condensed in small ice crystals or water droplets, it really interposes a very efficient screen to terrestrial radiation, even if, which sometimes is the case, it is perfectly transparent to light, *i.e.* invisible to our eye. Another influence of water on terrestrial radiation is admitted by all: that is, that of the latent heat in the deposition of dew and hoar frost.

If we wish to make meteorological observations bearing on the question, the following *modus operandi* should be adopted: (1) observations should be made in climates where, with a tension of vapour greater than that which obtains in England in winter, the relative humidity is yet so small that there is no dew on clear nights, or at least it appears rather late; (2) three thermometers placed on cotton wool, but at different heights above the ground should be observed, say one on the ground, and the others at heights, say from 10–100 feet above.

If Prof. Tyndall's views are right, the highest of the thermometers should show by far the lowest temperature, as it is not screened from radiation by the vapour of water diffused in the lowest stratum of air. I think every meteorologist will express the opinion that there will be scarcely a difference in this case. As to the observations in different climates, those made where the relative humidity is low should give no greater difference between the thermometer in the air and on the wool than the observations in England on clear nights, with the same vapour tension, if Prof. Tyndall's hypothesis be admitted. I think we have already many observations which prove that, with vapour-tensions much above 0″·181 (or 4·6 mm.), *i.e.* above that of saturation at 32° F., terrestrial radiation is very great, if only the sky is clear and the relative humidity small. No doubt the decrease of the temperature of the air from the midday maximum to the night minimum is caused by terrestrial radiation. I give some figures from the observations at Biskra, in the Algerian Sahara.[1]

	Difference of daily max. and min.	Mean temperature.	Tension of vapour.	Relative humidity.	Amount of cloud.
January	25·4	56·8	0·264	61	1·6
August	39·2	89·6	0·557	40	0·8
October	35·6	68·4	0·432	58	0·9

In an arid climate in low latitude the non-periodic variations are but small, and the difference between the maxima and minima is very near to the daily range of temperature. As the amount of cloud is very small in all three months taken here, the conditions for terrestrial radiation are very favourable. If vapour of water in the gaseous state impeded terrestrial radiation so much as stated by Prof. Tyndall, we should expect to find the daily range smaller in August than in January, on account of the double amount of vapour in the air. The reverse is the case, the daily range being by 14·8 greater in August than in January. Has anybody observed a daily range of 39·2 in England, be the amount of cloud and the vapour-tension ever so small?

I must add that in all observations bearing on terrestrial radiation we must not forget that other substances besides water in its three states may interpose a screen to radiation. I mean especially dust and smoke of all kinds. Now far from large cities, there are many reasons why in winter, especially when the ground is covered with snow, the air will hold less of these impurities than in summer, as in winter there are no fires of forests and peat-bogs, there is little inorganic dust, because the humidity of the soil, and still more so the snow, prevent it; organic dust, germines, &c., are also absent, or present in very small quantities, on account of the small amount of plant and lower animal life. The absence of dust and smoke explains the great purity of the air in winter, so favourable to solar and terrestrial radiation, as well as the purity of the air at great heights, especially above the snow-line.

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[1] "Annales du Bureau Central Météorologique de France," 1879, vol. ii.

Prof. Tyndall has certainly lost sight of this when he attributes the diathermacy of the air in winter only to the small amount of vapour of water. The same is the case when he points to the relatively small nocturnal radiation on clear nights in many tropical countries. In the case of many of them, besides dust and smoke, the *high relative humidity* has much to do with the small amount of cooling during the night. What quantities of latent heat are liberated by the formation of dew in humid climates of low latitudes, and how much the nocturnal cooling must be impeded by it, everybody can imagine who has been in these countries, or only read scientific travels to them.

A. WOEIKOF

Diurnal Variation in the Velocity of the Wind

THE observations discussed in Mr. Buchan's interesting article on this subject leave little to be desired, and with most of the conclusions meteorologists in general will agree. I am surprised, however, to find such an eminent authority accounting for the large diurnal oscillation on land, solely on the ground of its being due "to the superheating of the surface of the ground, and to the ascensional movement of the air consequent thereon, which tend to reduce the effects of friction and viscosity of the air."

There may perhaps be more hidden within this sentence than appears from the wording of it; but, taking it as it stands, it certainly omits what I believe to be the most important factor in the whole result, viz., *the interchange of motion between the upper and lower layers of the atmosphere, occasioned* by the ascensional movements during the day over superheated land. This has been most clearly shown by Dr. Koppen in an article in the Austrian *Zeitschrift fur Meteorologie*,[1] by successive rejection of inefficient causes, to be the only means by which such increase of velocity could be occasioned near the earth's surface.

It is not clear, moreover, how the ascension currents could otherwise diminish the friction of the air enough to account for such a large diurnal increase of velocity. The effect of the increased temperature alone, would certainly be to increase the friction, but as Köppen shows from Meyer's formula for the coefficient of gaseous friction, the daily range of temperature would only cause the friction of the air to vary from ½ to 1 per cent. of its whole amount,[2] so that this factor is evidently without any appreciable influence on the diurnal period.

In the paper already referred to, Dr. Köppen has gone into the whole question most minutely, and a perusal of it will, I think, convince most persons, that the chief factor in causing the diurnal increase of wind-velocity over land is the intermixture of air (*luft-austausch*) resulting from the uprise of heated air from the surface, and the consequent downfall of cooled air to it, "bringing down with it," as Espy told the British Association in 1840, "the motion which it has above, and which is known to be greater than that which the air has in contact with the asperities of the earth's surface."

Among the facts cited by Koppen in favour of his theory may be noted the following:—

1. The fact that in Europe the ratio of the velocity of the wind to the gradient, is greater for N.E. winds and in summer than for S.W. winds and in winter; together with the circumstance that the temperature decrement, and therefore also the facility with which local ascension and descension currents may be formed, is greater under the former conditions than under the latter.

2. That simultaneously with the diurnal increase in the velocity of the lower layers of the atmosphere, those above appear to be retarded.

3. That on stations near the earth's surface the curve of absolute humidity reaches its minimum about the time of maximum wind-velocity, while at elevated stations, such as the Faulhorn, the humidity reaches its maximum at the same time.

In fact it may be concluded, as Koppen graphically puts it, "that the greater the difference of the temperature of the air in a vertical direction, the smaller are the differences in the humidity, barometric pressure, and motion of the air, and that in the early hours of the afternoon the inhabitants of plains are placed to a certain extent on a higher, and the dwellers of Alpine heights on a lower, level, relatively to these elements."

E. DOUGLAS ARCHIBALD

The Large Meteor of March 2, 1883

THE meteor described by Mr. R. W. S. Griffith in the last number of NATURE was also observed at Bristol and Bath. At the latter place it was seen by Mr. J. L. Stothert at 9h. 33m. 40s., passing in the direction from α Hydræ to η Canis Majoris. The brilliancy of the meteor was equal to twice that of Venus; colour yellow; motion slow; no train. Comparing this observation with that obtained by Mr. Griffith, it would seem that the meteor probably belonged to a radiant point near Lyra, rising in the north-north-east at the time of its appearance. A meteor shower was observed by the writer on March 14, 1877, between 14h. 12m. and 15h. 43m. from the point α 277°, δ 25° +, the members of which were somewhat slow and devoid of streaks or trains, and the fireball of March 2 last appears to have belonged to the same stream.

It would be important to hear of additional observations of this meteor. Its considerable brightness, and the fact that it appeared at a time when it must have been widely observed, lead me to hope that many other records of its path have been preserved. In all such cases it is very desirable to give the R.A. and Dec. + or − of the beginning- and end-points of the observed path. Descriptions by the stars or compass-bearings are likely to be less accurate, and are often difficult to reduce.

In the *Observatory* for September, 1879, p. 129, I mentioned that "during the first four days of March fireballs have been very numerous, especially on the 1st, 2nd, and 4th." This meteoric epoch is therefore well confirmed by the fireball of the 2nd inst. which it is hoped will aid us in determining one of the chief radiant points of the date.

W. F. DENNING

Bristol, March 12

A VERY brilliant meteor was seen here on March 2 at 9.35 p.m. It burst forth in the immediate neighbourhood of Sirius, and passed downwards to the west at about an angle of 40° from the perpendicular, disappearing after a course of about 25°. Its light was so strong as to make the distant trees, fields, and hedges perfectly visible, brighter than the brightest moonlight. Its colours also were very decided, changing quickly, much as does Sirius to the naked eye, but showing more of the violet at first, and afterwards more of the red.

J. L. J.

Capel, Surrey

On the Movements of Air in Fissures and the Barometer

I SHOULD be glad to add to my article "On the Movements of Air in Fissures and the Barometer" (NATURE, vol. xxvii. p. 375) a reference to an instrument devised by Mr. Whitehouse, and described in 1871 before the Royal Society (*Proc. Roy. Soc.* vol. xix. p. 491). The apparatus, which was intended to record minute variations of atmospheric pressure, consisted of two hydraulic chambers, connected by a tube or siphon, and buried in the ground. One of the chambers was left open at the top and exposed to atmospheric pressure, the other was closed and removed from such pressure; the difference in the level of the water in the two was a measure of the variation in the atmospheric pressure. This instrument reproduces those conditions to which the oscillation of the water-level in certain chalk-wells, coincident with the barometic changes, has been attributed. It was believed by the inventor that by its aid he had been able to detect atmospheric waves or pulsations at a distance from a storm-centre; it has not however come into scientific use.

I may further add to my brief allusion to colliery explosions a reference to the paper by R. H. Scott, M.A., F.R.S., and W. Galloway, Mining Engineer, entitled "On the Connection between Explosions in Collieries and Weather" (*Proc. Roy. Soc.* vol. xx. p. 292, 1872).

A. STRAHAN

28, Jermyn Street, March 10

THE PITT-RIVERS COLLECTION

IT will be remembered that some time past Major-General Pitt-Rivers, F.R.S., most munificently offered his far-famed Anthropological Collection to the University of Oxford on the condition that the University should erect a building adequate to contain it and display it properly. On Wednesday, the 7th ult., a vote was passed by Convocation authorising the Curators of the Univerity

[1] "Die tagliche periode der Geschwindigkeit und Richtung des Windes," September heft, 1879.

[2] Meyer's formula in English measure is $\eta = \eta_0 (1 + {\cdot}0014t)$, where η η_0 are the friction coefficients at $t°$ and 32° Fahr. respectively

Chest to expend a sum of 7500*l.* on the erection of an annex to the east side of the present University Museum to contain the collection and to provide the requisite cases and fittings; a vote of thanks to General Pitt-Rivers was also passed.

This most important collection, therefore, which commenced its public existence at Bethnal Green, and has so long been exhibited at South Kensington, will rest finally at Oxford, where it cannot fail to be studied with ever increasing interest and benefit to learning generally. The title of the collection as the "Pitt-Rivers Collection" is to be maintained, and the developmental and gradational system of arrangement devised by the donor, and carried out by him in the greater part of the collection, with such valuable and interesting results is to be retained. The new building, which will be provided with two galleries, will be entered by two doorways at different levels from the present University Museum.

The delegates of the Museum have elected Dr. E. B. Tylor to be Keeper of the Museum in place of the late Prof. Henry Smith, so that the new collection, as well as the anthropological collection of the late Prof. Rolleston, will fall into the hands of the man most suited to arrange and explain them.

JOHN RICHARD GREEN

THE death of Mr. Green, at the early age of forty-five years, we regard as a serious loss not only to historical literature but to science. We have frequently maintained that science has no peculiar sphere, that every field of human research is capable of scientific treatment. As we pointed out in reviewing Mr. Green's famous "Short History" and his "Making of England," he has the credit of having been the first historian who appreciated the function of science in a State, or the moulding power of the environment of a people. Not only so, but he distinctly aimed at showing that the history of a people is simply an evolution dependent for its course and outcome on the action and reaction between the entity and its surroundings. This conception of the function of the historian was probably even more distinctly brought out in the "Geography of the British Isles," by Mr. Green and his accomplished and congenial wife. As we pointed out in our notice of the "Short History" moreover, Mr. Green not only wrote his "History" on a scientific method, but gave large space in that history to a record of the progress of science and of scientific societies, as distinct and influential elements in the life of our nation. Indeed he may be regarded as the first historian who, breaking away from the old conventional methods of writing history from the outside, and thus mistaking the shell for the kernel, adopted the method of the physical geographer as distinct from the mere topographer, and, penetrating deep beneath the surface, traced the forces which have actuated the nation and brought it to its present standpoint. Although the impulse given by Mr. Green to historical study will certainly bear fruit, his loss cannot be overestimated. His "Making of England" was evidently only a prelude to a series of volumes in which he intended to show in minute detail the interaction between the various elements that go to make up the life of these islands,—the ethnical and moral elements on the one hand, and the encompassing physical elements on the other. Happily he has left behind him in a nearly complete state a second volume on "The Coming of the Northmen," which brings his scheme down to the point when it may be said that all the forces were in the field, the continued action of which has gone to make up the England of to-day. Since Mr. Green's death ample testimony has been borne to his rigidly scientific method of work, and the patience with which he wrote and rewrote ere his own severely critical standard was reached. It will be difficult to find a successor to Mr. Green so far as stirring eloquence of style is concerned, but we trust that his scientific method may find favour, and that historians in future will endeavour to trace the life of a nation as he did, after the manner of the biologist and physical geographer.

THE BOTANY OF THE "CHALLENGER" EXPEDITION

FROM time to time various contributions to the Botany of the *Challenger* Expedition have been published in the *Journal* of the Linnean Society, chiefly in the fourteenth and fifteenth volumes; but hitherto no part of the botanical results has appeared in the series of sumptuous volumes in which are recorded the discoveries and observations of the expedition. The Government have at length decided to devote one volume of about 350 pages and fifty plates to the elucidation of the flora of the more interesting countries visited, which the writer of the present article has undertaken with the assistance and under the superintendence of Sir Joseph D. Hooker. There can be no doubt that the Government are right in their estimate of the relatively small importance of the results obtained in botany as compared with those obtained in other branches of science; yet we think we shall be able to show that the botanical collections are sufficient to form the basis of a most interesting volume. It is almost superfluous to state that the botanist of such an expedition has little chance of exhausting the flora of any of the numerous countries or regions visited; and the task of elaborating the materials seemed at first an unpromising one. At many of the places visited, and especially some of the more interesting ones, the stay was too short and the means inadequate for making and drying large collections of plants. Nevertheless the naturalist, Mr. H. N. Moseley, seems to have lost no opportunity, having collected in almost every place touched at. Unfortunately the plants of the least-known countries, such as the Aru and Admiralty Islands, reached England in a very much damaged condition. But imperfect as they are, they include a large proportion of novelties, and indicate a flora rich in endemic species. The best collections, so far as number and quality of the specimens are concerned, are those from Chili, Juan Fernandez, Japan, the Sandwich Islands, &c.; yet they contain little or nothing new to science, and by no means fully represent the vegetation of the several countries. There remain the collections made in the remote islets of the Atlantic and Southern Oceans, which, with what was previously known, afford material for a practically complete flora of these isolated spots, so interesting to the student of the distribution of plants and animals. And it has been decided that this shall be the scope of the work.

The Bermudas, the oldest English colony, come first in the arrangement adopted. These islands, having an area of about one-seventh of that of the Isle of Wight, are situated about six hundred miles from the American continent, and although settled as long ago as 1612, nothing approaching a complete and critical account of their vegetation has hitherto been published. The flora is a poor one, especially in regard to number of species, and is evidently of comparatively recent origin, being in this respect in striking contrast to that of various other Atlantic islands—that of St. Helena, for example. The indigenous element has been, almost without exception, derived from the West Indies and the extreme south-east of the mainland of North America. By the indigenous element we mean those species which have reached the islands independently of human agency, direct or indirect. With unimportant, though rather numerous, exceptions, the indigenous and introduced elements are easily distinguished. A remarkable feature in the vegetation is the almost total absence of endemic forms. The possible important exceptions are the native palms. There are two or possibly three species, of which one belongs to the genus Sabal. Without due investigation, it has been generally accepted as a fact that there was only one indigenous palm, and that this was identical with the Sabal Palmetto of south-eastern North America; but in elaborating the palms for the "*Genera Plantarum*," Sir Joseph Hooker became aware that the imperfect herbarium specimens in this country represent two species, one of them at least evidently different from *Sabal Palmetto*. Several historical passages in Sir J. H. Lefroy's work on the Bermudas confirm this view. Thus, in one place it is recorded that the only food certain fishermen took out to sea with them on a given occasion was "Palmitoe berries"; and in another place that the workmen did not hesitate to share this fruit with pigs and other animals, and even preferred it to bread to eat with their meat. Every effort is being made to obtain material this season to set this question at rest. The earliest references we find to the vegetable productions of these islands are in the "Historye of the Bermudaes," edited by Sir J. H. Lefroy, and some of these are valuable, because they enable us to say with certainty that one species of *Opuntia*, for example, existed in abundance previous to the settlement of the islands.

François André Michaux was the first botanist who visited the Bermudas. In his case it was unintentional, the fortunes of war having been the cause of his spending a week there in 1806. He published an interesting sketch of the vegetation, though the following extract reveals a want of exactitude: "Parmi ces plantes [*i.e.* les plantes naturelles au pays] on en trouve plusieurs de l'ancien continent, qui ne paroissent pas de nature à y avoir été transportées: telles sont *Verbascum thapsus*, *Anagallis arvensis*, *Mercurialis annua*, *Leontodon taraxacum*, *Plantago major*, *Gentiana nana*, *Oxalis acetosella*, &c." The two last names must have been a slip of the pen. Since Michaux's time two imperfect lists of Bermudan plants have been published, both in 1873. One, by J. M. Jones, F.L.S., is marred by some rather gross errors in classification and nomenclature, yet it contains some interesting information. The other, by Dr. J. Rein, was prepared with greater care, and contains 128 species of introduced and indigenous flowering plants and ferns, besides upwards of 100 algæ. Altogether Mr. Moseley collected 162 species of plants. In addition to these is a considerable number sent to Kew by Sir J. H. Lefroy during his governorship of the islands, making a total of about 320 species that occur in a wild state. These may be classified as follows: indigenous, 130; probably indigenous, 57; certainly introduced, 133. The last number would be higher if we included solitary waifs of other species.

Next in order of the *Challenger* collections come those of St. Paul's Rocks and the island of Fernando Noronha, in which Mr. Moseley collected about sixty species, including a new species of *Oxalis*, one new Asclepiad, and one fig, &c. Had permission to collect objects of natural history not been withdrawn after the first evening, there is no doubt this collection would have been an important one.

Proceeding southward and taking the other islets on our way, we have Ascension, St. Helena, Trinidad (off the coast of Brazil, in about 20° 30′ S. lat.), Tristan d'Acunha, and the neighbouring islets Inaccessible and Nightingale; and thence southward and eastward, Gough Island, Lindsay and Bouvet Islands, Prince Edward and Marion Islands, the Crozets, Kerguelen Island, the Heard group, St. Paul and New Amsterdam. With the exception perhaps of Kerguelen Island, the published accounts of the botany of these oceanic islets are all most imperfect and scattered. We are unaware of any complete enumeration of the exceedingly meagre indigenous flora of Ascension. St. Helena has fared better; but the fifty or so indigenous species are lost amongst the 1000 species of introduced plants enumerated in Mr. Melliss's book "St. Helena," the botanical value of which consists chiefly in the figures of the endemic plants. Moreover Mr. Melliss did not elaborate the synonymy of the flora, and some of the Cyperaceæ were undetermined, whilst a few, we believe, were omitted.

The island of Trinidad is rather farther from the coast of Brazil than the Bermudas are from North Carolina, and very little is known of its vegetation. On the outward voyage of Sir J. Ross's Antarctic Expedition, Sir Joseph Hooker and some of the other officers landed on a small rocky cove, where they were unable to scale the barrier cliffs, so they could not penetrate to the interior of the island, and they brought away only one fern (*Polypodium lepidopteris*) and one sedge (*Fimbristylis*, sp.), though there were tree-ferns and other trees, in sight from the ship, on another part of the island. In 1874 Dr. Ralph Copeland, of the Dunecht Observatory, who accompanied one of the transit expeditions, landed on the east side of the island, and succeeded in reaching the elevated centre, where he found several ferns in great luxuriance, and collected a few scraps of plants, including a new tree-fern. The most interesting plant, however, was *Asplenium compressum*, a fern previously known only from St. Helena, though Melliss, by some unfortunate slip, records it from South Africa, Madagascar, &c. Dr. Copeland further states that, although most of the valleys of the north side of the island contained enormous numbers of dead trees, not a single living one was to be seen, except near the highest points. They appeared to have been dead many years and were mostly overturned. He was unable to investigate the phenomenon, but suggests that they may have been destroyed by goats, though he adds not a mammal of any kind was seen.

Tristan d'Acunha itself was explored by Dupetit Thouars in 1793, and he described the plants in a paper which he read before the *Institut* of France in 1803. The next botanist who visited the island was Carmichael, who published an enumeration of the plants he collected in the *Transactions* of the Linnean Society. Mr. Moseley botanised the same island and the neighbouring Nightingale and Inaccessible Islands, and collected not only those previously known, but also some new species of Cyperaceæ. Previously, too, *Gnaphalium pyramidale*, Thouars, was unknown at Kew, or rather a young plant of it collected by Carmichael could not be identified as such with certainty.

We have little space left, so we can merely mention the groups of islets in the Southern Ocean. Mr. Moseley added considerably to our knowledge of the flora of Marion Island and the Heard Group, and Kerguelen Island, whilst the Americans, Germans, and French, of their respective expeditions, investigated the Crozets and New Amsterdam and St. Paul's Islands. Kerguelen Island, the largest by far of all these oceanic islets, being about eighty miles in diameter, has been explored by the naturalists of the English, German, and American transit expeditions, and the results published. One of the most interesting discoveries of late years connected with the vegetation of these islets was made by the late Capt. Goodenough, about ten years ago, when he collected *Phylica arborea* in Amsterdam Island, till then only known in the island of Tristan d'Acunha, separated therefrom by ninety degrees of longitude, which in this latitude are equal to a distance of about 4700 miles. Mr. Moseley also found it abundantly in Inaccessible and Nightingale Islands. *Phylica arborea* is likewise remarkable in being the only plant of these southern islets that is arboreous in habit, though at the outside it is only about twenty feet high in the most sheltered localities.

W. BOTTING HEMSLEY

THE SHAPES OF LEAVES[1]

II.—*Extreme and Intermediate Types*

WHERE access to carbonic acid and sunlight is habitually unimpeded by the competition of other plants in any direction, the leaf of each species tends to assume a completely rounded form; the conditions are evenly distributed on every side of it. Such absolute freedom to assume the fullest foliar perfection is best found on the surface of the water. Hence most water-plants which have leaves lolling on the surface assume a more or less distinctly rounded shape, the venation and other details remaining in accordance with the ancestral habit. Foliage of this character is found in the water-lilies and many other aquatic plants. The little entire lenticular fronds of the common duckweed, *Lemna minor* (Fig. 10), which coats all our small ponds and ditches, form an excellent example of the type in question. Here the shape is almost orbicular; the edge is entire; and the smallness of each separate frond is due to the minuteness of the plant and the obvious necessities of its situation. In the waterlilies we get a similar example on a much larger scale, for these plants recline on broader and more permanent sheets of water, and draw nourishment from their large rhizome, sunk securely in the mud beneath, and annually accumulating a rich store of food-stuffs for the growing foliage.

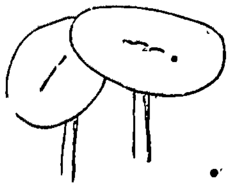

FIG. 10.—*Lemna minor.*

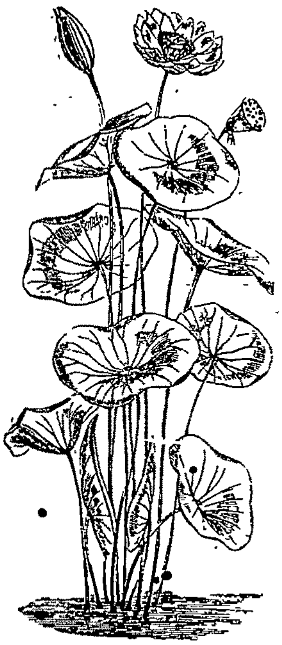

FIG. 11.—*Nelumbium speciosum.*

Mr. Herbert Spencer (by whose kind permission two accompanying diagrams are copied from "The Principles of Biology") points out a distinction between the shapes adopted by such plants, according to their relations to a central axis. In the sacred lotus, *Nelumbium speciosum* (Fig. 11), the leaves grow up on long and independent footstalks, without definite subordination to any such axis; and they therefore assume an almost perfectly symmetrical peltate form. In the *Victoria regia* (Fig. 12) the footstalks, though radiating almost horizontally from a centre, are long enough to keep the leaves quite remote from one another; and here they assume an almost symmetrically peltate shape, but with a bilateralness indicated by a long seam over the line of the footstalk. The leaves of our own white waterlily, *Nymphæa alba* (Fig. 13), are more closely clustered, and have less room to expand transversely than longitudinally; hence they are somewhat longer than broad, and have a cleft where the *Victoria regia* has only a seam. *Limnanthemum* shows the same type on a smaller scale.

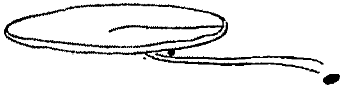

FIG. 12.—*Victoria regia.*

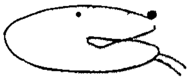

FIG. 13.—*Nymphæa alba.*

Among land plants, the conditions under which leaves can fill out to the full rounded shape occur less frequently than among floating aquatic species; still, even here a very interesting set of gradations may be observed. The best example of all is that given by the common American May-apple, *Podophyllum peltatum*, where the separate radical leaves grow straight up from a stout rootstock on very thick and tall stalks, so as to overshadow all the other vegetation; and they assume a regular, circular, peltate form, exactly like a Japanese parasol. The radical leaves of our own English *Cotyledon umbilicus* (Fig. 14), springing from a perennial rootstock, for the most part on bare walls or unoccupied hedgerows, are able similarly to expand without interference, and catch carbonic acid and sunlight to their hearts' content. Hence they are orbicular and peltate, though they retain the characteristic crenate edge of most flat-leaved Crassulaceæ. But the upper leaves, springing from the flower-stalk, are more bilateral, as shown in the figure, though even these round out to a more or less orbicular form, owing to their exceptional access to air and light. The so-called garden nasturtium, *Tropæolum majus*, with leaves growing out at right angles into open space, has also peltate leaves, as has likewise the usually aquatic *Hydrocotyle.*

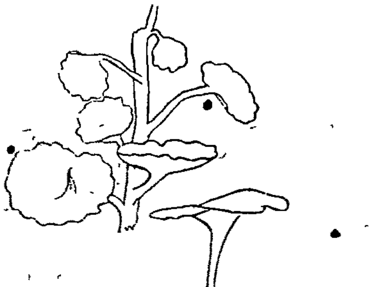

FIG. 14.—*Cotyledon umbilicus.*

When the plant sends up leaves from a rich buried rootstock, so tall as to overshadow the surrounding vegetation, but subordinated to a common centre, they usually assume the reniform shape. This type is particularly well seen in the various coltsfoots—for example, in *Tussilago farfara*, *T. petasites*, and *T. fragrans* (Fig. 15). Similar types occur in *Asarabacca*, and in the marsh marigold, *Caltha palustris*. Extremely similar to the leaf of *Caltha*, though on a smaller scale, is that of one true buttercup,

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[1] Continued from p. 442

Ranunculus ficaria, the lesser celandine, which produces its foliage in early spring from buried tubers, and so anticipates other plants, having the air all to itself for a couple of months, after which it gets overshadowed by later comers. The same type recurs pretty closely in the radical leaves of its allies, *R. auricomus* and *R. parviflorus*, as also somewhat more remotely in the ivy-leaved crowfoot, *R. hederaceus*, which creeps, unimpeded, over soft mud. Many early spring plants have lower or radical leaves at least of this reniform type, because they grow in comparatively unoccupied ground. As an example, take ground-ivy, *Nepeta glechoma* (Fig. 16). The violets represent a closely similar case. Many of these plants, however, produce later on, when foliage grows thicker, much more lanceolate leaves. In the burdocks, docks, &c., this type is persistent.

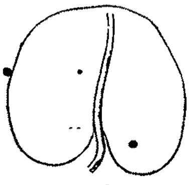
Fig. 15.—Typical leaf of *Tussilago* genus.

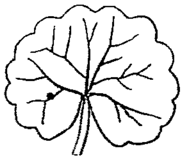
Fig. 16.—*Nepeta glecoma.*

On the other hand, where the distribution of carbonic acid is most scanty, or where the competition is fiercest, or where the competing plants are supplied with no reserve to enable them to send up shoots which overtop their competitors, immense subdivision into leaflets takes place, and these leaflets are often almost or quite filiform. The extent to which leaflets are subdivided depends upon the relative paucity of carbon in their environment; the general resulting form depends mainly upon the inherited type of venation. Among submerged aquatic plants, the filiform condition is habitual, because carbonic acid is so comparatively scarce in water. Among British species, the water violet, *Hottonia palustris*, is a good example. All terrestrial primroses have undivided foliage; but in *Hottonia* the leaves, still preserving the pinnate character of the venation, as in the common primrose, are cut into very deep segments, forming a close mass of narrow, linear, waving threads, more like a *Chara* than a flowering plant at a first glance. *Utricularia* shows the same result with a different ground-plan. In *Myriophyllum*, water milfoil, we have whorls of leaves each minutely subdivided into hair-like pinnate segments, and moving freely through their still ponds in search of stray carbon particles diffused in the water. *Hippuris* has the separate leaves undivided, but attains the same result by crowding its long, thin, linear blades in whorls of ten or twelve, so as closely to resemble an *Equisetum*. Our common *Ceratophyllum* looks at first sight much like water-milfoil, but here the whorled leaves, instead of being pinnately divided, are repeatedly forked into subulate or capillary segments, the result of a branching rather than of a pinnate venation. Other instances will occur at once to every botanist.

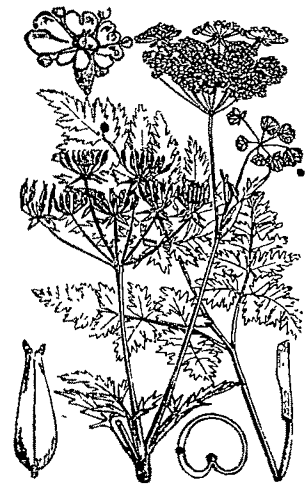
Fig. 17.—*Chærophyllum silvestre.*

On land we get very much the same condition of things in the fierce competition that goes on for the carbon of the air between the small matted undergrowth of every thicket and hedgerow. The common weedy plants, and especially the annuals or non-bulbous perennials, which grow under such conditions, cannot afford material to push broad leaves above their neighbours' heads, and they are therefore compelled to fight among themselves for every passing particle of carbon. Hence they are usually very minutely subdivided, though in a less waving and capillary manner than the submerged species; their leaflets are oftener flat, and definitely exposed on their upper surface to the sunlight. That essentially weedy family, the Umbellates, contains a great number of such highly segmented hedgerow leaves. Common wild chervil, *Chærophyllum silvestre* (Fig. 17), forms a familiar example: other cases are *C. temulum*, *Sison Amomum*, many *Carums*, *Œnanthes*, *Pimpinellas*, *Daucus*, *Caucalis*, &c., all of which belong by habit to greatly overgrown localities. Compare these with the free-growing, almost orbicular, radical leaves of *Astrantia* and *Sanicula*, in

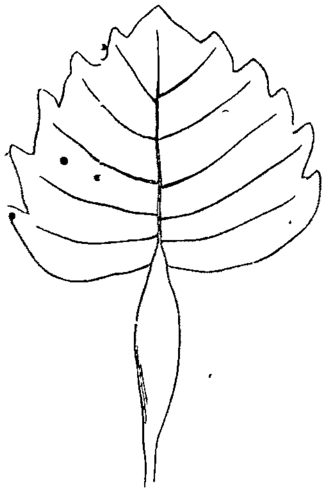
Fig 18.—Floating leaf of *Trapa natans*

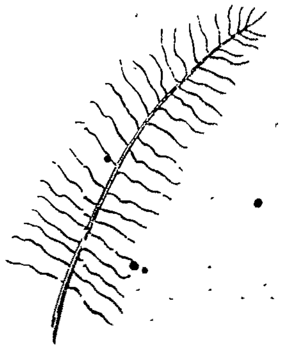
Fig. 19.—Submerged leaf of *Trapa natans*

the same family; or with the still freer peltate leaves of *Hydrocotyle*; or again with the divided but more broadly segmented leaves of those tall open-field species, cow-parsnip, *Heracleum sphondylium*, and Alexanders, *Smyrnium olusatrum*, which have only to compete against the grasses and clovers; or, finally, with the large waterside forms, *Apium graveolens*, *Sium latifolium*, and *Angelica silvestris*. So, too, take the much segmented herb-Robert, *Geranium Robertianum*, of all our hedgerows, growing side by side with the like-minded chervils and carrots, and compare it with that persistent rounded geraniaceous type which recurs, not only in our English *G. molle*, &c., but even in many exotic *Pelargoniums*. Among composites the crowded type is best exemplified by that thicket weed, milfoil, *Achillea millefolium*, with its infinite number of finely-cut, pinnatifid segments; while in the taller but closely-allied sneezewort, *Achillea ptarmica*, growing on high open pastures, we get the same general type in outline and venation, only entire save for the slight serrations along its edge. In tansy, *Tanacetum vulgare*, also a hedgerow plant, the same type as milfoil recurs on a far larger and handsomer scale. Compare these with coltsfoot and burdock, or even with the tall eupatory and the tufted, close-packed daisy. Other good miscellaneous instances of the weedy type are fumitory, *Corydalis*, moschatel, the camomile group, &c.; while among larger cryptogams the majority of thicket ferns display an equally marked subdivision of the fronds and pinnæ. It may be added that highly civilised countries like England are particularly rich in these subdivided types of foliage, owing to the predominance of hedgerows and of tall grasses.

As in the submerged plants, so in the matted terrestrial undergrowth, whorling of linear leaves may practically answer the same purpose as minute segmentation. Some plants solve the difficulty of catching stray carbon in the one way, and some solve it in the other. Each adopts the easiest modification of its own ancestral type. For example, take the stellate tribe. Their tropical allies, the larger Rubiaceæ, have simple, usually entire, opposite leaves, with interpetiolar stipules. In the small, weedy, northern forms however, the interpetiolar stipules have grown out into linear leaf-like foliar organs, forming with the true leaves an apparent whorl of six members. Sometimes, too, the whorl is enlarged to as many as eight leaves, and sometimes reduced to four. These thick whorls of small leaves, always well turned outward to the sunlight, have become practically analogous in their action to minutely segmented leaflets, in our English *Galiums*, *Asperulas*, and *Sherardia*. Two of them at least, *G. mollugo* and *G. aparine*, are extremely common hedgerow plants. Compare them with the broad-leaved free-climbing *Rubia peregrina*, which has only four large members to each whorl.

Among monocotyledons, where (as will be afterwards explained) the type is given by the peculiarity of the cotyledon and governs the venation, minute subdivision is replaced in the matted undergrowth by single, linear, lanceolate blades, which answer the selfsame purpose in the long run. The grasses, sedges, and woodrushes are sufficient examples. Here the numerous leaves, all long and narrow, and all with long thin flower stems, strive to overtop one another, and run up side by side to a considerable height. They may be compared with the large rich leaves of the bulbous lilies, tulips, amaryllids, and orchids. In both cases the type is the same, but the development is different. Plants that consort much with the grasses, as for example ribwort plantain, though wholly unlike in type, are apt to be drawn up and assimilated to them, not merely in general character, but even in venation and mode of fertilisation. Other grass-like dicotyledons are found among the *Polygonums*, *Armerias*, *Bupleurums*, pinks, &c., all under similar circumstances to those of the grasses themselves.

Intermediate types between these two extremes of entire obicularity and minute subdivision occur everywhere. Compare, from this point of view, the common meadow buttercups, which grow in fully occupied meadows, with *Caltha* and the lesser celandine. Compare, again, the mallows on the one hand with the peas on the other, or the docks with the crucifers. Throughout these intermediates, various stages can be easily observed. For example, the South European water-chestnut, *Trapa natans*, beautifully illustrates the gradations which have finally given us our own *Hippuris* and *Myriophyllum* from an Onagraceous or Saxifrage ancestor. It has a number of floating leaves (Fig. 18) supported by bladder-like petioles filled with air, and arranged radially round the stem. Hence, though large and spreading, they are distinctly bilateral, and they do not interfere with one another's food supply. But the submerged leaves (Fig. 19, very diagrammatic) are mere pinnate skeletons of the venation, waving about in the water below. Among monocotyledons, the *Potamogetons* show us some very instructive similar cases, altered in character by the peculiarities of the very persistent monocotyledonous foliar type. In the floating leaves of *P. natans* they come as near the waterlilies as a monocotyledon can reasonably expect to do; in *P. pectinatus*, the wholly submerged leaves look like long blades of grass, proceeding from the thread-like stems.

Less minutely subdivided than the hedgerow plants are a large class of somewhat weedy forms, well typified by our smaller English crucifers. These are often pinnately divided to a considerable extent, as in *Cardamine hirsuta* and *Senebiera didyma*. Compare them with the taller kinds, such as cabbage and charlock. Much the same type reappears in the lowly forms of Papilionaceæ, as for example in *Anthyllis*, *Astragalus*, *Ornithopus*, *Hippocrepis*, &c. On the other hand, in the tall climbing *Vicias*, and still more in *Lathyrus*, the leaflets, having more carbon, more sun, and less competition, fill out rounder, and generally decrease in number, the upper ones being transformed into tendrils. But in the very grass-encumbered clover-like types, *Ononis*, *Medicago*, *Melilotus*, *Trigonella*, and, above all, *Trifolium* itself, the leaflets are dwarfed and reduced to three, the lower members being suppressed, and only the three terminal ones left, so as to raise them on a long footstalk up to the air and sunshine. Compare the very similar leaflets of wood-sorrel. Again, look at the various conditions under which the following Rosaceous plants grow: pear, blackthorn, strawberry, cinquefoil, silver-weed, great burnet, salad burnet, and compare some of them with clover, lady's-fingers, and *Hippocrepis*. The comparison tells its own tale at once.

Finally, we must briefly allude to a large class of tufted plants, usually with entire, ovate, obovate, or ovate-lanceolate leaves, which grow in a rosette from a centre, and insure themselves a good supply of carbon and of light by keeping under all competitors with their close tufts. Of these, our common daisy forms an excellent example: notice the tight way it fits itself against the ground so as to prevent grass from growing beneath it. Another good case in point is *Plantago media*: compare form and habit with those of *P. major* and *P. lanceolata*. To the same class, more or less, may be referred *Arabis thaliana* and many crucifers, London Pride, the common primrose, *Hieracium pilosella*, &c.; and, with more pinnate, lyrate, or prickly leaves, the young thistles, and the radical foliage of many ligulate composites.

The shapes of leaves thus depend upon the average surrounding conditions, modifying a given ancestral type. How these ancestral types themselves were first developed we shall have to inquire in our next paper.

GRANT ALLEN

(*To be continued.*)

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT[1]

III.

THE first important contribution to our knowledge of inhibitory centres in the brain and spinal cord was that of Setchenow. He found that when the cerebral lobes in a frog were removed, voluntary motion was abolished, but reflex action became somewhat more marked. On removal of the optic lobes, the reflex action became very greatly increased, and if, instead of removing them they were stimulated either chemically by a grain of salt laid upon them, or electrically, reflex action in the limbs was greatly retarded or completely abolished.

These experiments were repeated by Herzen, who, like Setchenow, considered that there was no inhibitory mechanism in the spinal cord itself, but disbelieved also in inhibitory centres in the brain. He explained the depression of reflex which occurred on irritation of the optic lobes by supposing that any intense nervous irritation, no matter whether it was central or peripheral, caused great depression of reflex action both when the brain was intact and when it was divided, as in Setchenow's experiments. Setchenow again repeated his experiments, and came to the conclusion that it was uncertain whether the inhibitory mechanism could be excited reflexly from the periphery. He made, also, a sharp distinction between tactile and painful impressions upon the skin. For tactile impressions he considered that there was no inhibitory mechanism in the brain. Further investigations still, showed that both chemical and electrical irritation would excite the inhibitory apparatus, and he, therefore, considered that both excito-motor and depressor fibres were present in the same nerve-trunk[2] Goltz found, in opposition to Setchenow, that there was an inhibitory apparatus for tactile reflexes also in the frog's brain, but this he found in the cerebral lobes,[3] while Setchenow denied any inhibitory function to that part of the brain altogether.

He found also, however, like Herzen, that complete abolition of reflex action could be produced by powerful irritation of any peripheral sensory nerve, and considers that the irritation is conveyed to the reflex centre, and diminishes or destroys its excitability for the original stimulus, without supposing that there is any special inhibitory centre.

Lewisson found that by powerfully compressing the neck, or by squeezing the feet, or some other part of the body of a frog, or by irritation of the cutaneous or muscular nerves, or by electricity, the reflex excitability could be much depressed. He found, however, that unless the irritation was strong it produced stimulation both of the reflex and motor centres of the brain instead of depression.[4]

The general conclusion to which all these experiments, as well as those of Fick,[5] Freusberg, and others lead is, either that the nerves contain both excito-motor and reflex depressing fibres, or that excitement and depression can be produced by the same nerves under different conditions.

Freusberg,[6] who discusses the question of inhibition in an able and thorough manner, comes to the conclusion that all instances of inhibition including the different effects of weak and powerful stimuli applied to the same nerve, and also the inhibitory effects of stimulation of different nerves on each other, are not due to specific inhibitory centres, but to a remarkable property of the central nervous system, which does not allow of its different parts being simultaneously set in action by different causes. This conclusion, although it may be nearer the truth than the hypothesis of separate inhibitory centres, is not satisfactory, for it still leaves us in the dark regarding the way in which the central nervous system comes to possess the remarkable properties which he attributes to it.

Setchenow explains the increased rapidity of reflex action after section of the cord below the medulla oblongata, by supposing that there are two paths along which the stimulus usually passes, from the sensory to the motor tracts. The one goes directly across, and this is the path taken after section. The other goes up to the medulla, and then down the cord. This is the path taken under ordinary conditions; but besides the apparent unlikelihood that the stimulus should take this longer path under normal conditions, an objection has been raised to it by Cyon which seems fatal.

Cyon finds that when the so-called inhibitory centres are stimulated, although reflex contraction of the leg is apparently delayed for a long time, this delay is to a great extent only apparent and not real.[1]

It is true that the vigorous contraction of the muscles which suffices to raise the limb is much delayed, but a contraction of these muscles commences at very nearly the same time that it would do if the inhibitory apparatus were not stimulated. This shortening of the muscle goes on very gradually for a considerable time, and then culminates in a sudden vigorous contraction, the total height of which is greater than that of the contraction which would have occurred without irritation of the inhibitory centres. It is very difficult to explain this result on the ordinary hypothesis, but easy enough on that of interference. According to it we suppose that a stimulus applied to the foot has been transmitted as usual from the sensory to the motor cells of the cord, and thence to the muscles, so as to initiate contraction in them. This stimulus would correspond to the first half wave in the diagram (Fig. 2). The subsequent waves of stimulation which would have proceeded from the motor ganglia have been interfered with by the stimuli passing down from the so-called inhibitory centre, but their times being not arranged so that each wave from the brain should fall half a wave-length behind that in the cord, the stimuli at length cease to interfere, and the contraction, which has gone on gradually increasing as the interference diminishes, at last finishes abruptly.

The part of the brain which ought to correspond in higher animals to the optic lobes in frogs is the corpora quadrigemina, but irritation of these parts has not been found to have any marked inhibitory action upon reflexes in the limbs.[2]

Irritation of the frontal lobes in puppies has, however, been found by Simonoff[3] to exercise an inhibitory action; but, according to Ferrier abolition of the frontal lobes in monkeys does not produce any very obvious effect upon the animal.[4] We know that by an effort of the will, we are able either to increase or diminish reflex action, and it might appear probable that irritation of the motor tracts in the cerebrum might have an inhibitory action on reflexes. Irritation of the cerebral motor areas has not been found to exercise any definite inhibitory action upon reflexes, but on the other hand Exner[5] has found, if a stimulus be applied simultaneously to a motor area in the brain and to an extremity, the two stimuli aid one another, and produce a greater effect than they would separately. As irritation

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[1] Continued from p. 439

[2] Uber die elektr. und chem. Reizung der sensiblen Ruckenmarksnerven des Frosches, 1868. Quoted by v. Boetticher, *op. cit.* p. 6.

[3] Goltz, *op. cit.* p 42.

[4] Lewisson, ",Ueber Hemmung der Thätigkeit der motor. Nervencentren durch Reizung sensibler Nerven," Archiv. f. Anatomie u. Physiol. 1869.

[5] Fick, Verhandlungen der physikalisch medicinischen Gesellschaft zu Wurzburg, April 23, 1870

[6] Freusberg, "Ueber die Erregung u. Hemmung d. Thatigkeit d. nervosen Centralorgane," Pflüger's Archiv. x 174.

[1] Cyon, Ludwig's Festgabe, p. clxviii.

[2] Setschenow Physiologische Studien uber die Hemmungs-mechanismen für die Reflexthatigkeit des Ruckenmarkes im Gehirn des Frosches, p 3 (Berlin: Hirschwald, 1863).

[3] Simonoff, Arch f. Anat. u. Phys. p. 545, 1866.

[4] Ferrier, Functions of the Brain, p. 230 (London, 1876).

[5] Exner, Pfluger's Archiv. xxviii. 487.

of the cerebral motor areas, therefore, does not exercise a definite inhibitory action upon reflexes, but does under certain conditions markedly increase them, one might expect that their removal would diminish reflex action. Such a diminution actually occurs when they are destroyed in disease, but when the brain is removed layer by layer in operations upon animals, it is usually found that the reflex increases in proportion to the quantity removed. When the whole brain is removed, the reflex action is greater than when it is present, and as the cord is cut away layer by layer, the excitability of the segment below appears to be increased; each layer, as has already been mentioned, appearing to have an inhibitory influence on the one below it. But this is not always the case, because we sometimes find on removal of the various parts of the brain or of the spinal cord that the section completely abolishes reflex action for the time.

We are accustomed frequently to cloak our ignorance of the true cause of this abolition by saying it is due to the shock of operation or something of that sort; but looking the facts fairly in the face, we find that sometimes removal of the upper part of the brain or spinal cord causes increase and sometimes diminution of reflex-action in the parts below. At present we have no satisfactory explanation of this phenomenon, but if we suppose in the one case the nervous matter to have been removed in such a way as to cause an interference of the stimuli passing along from cell to cell, and in the other to cause a coincidence, we can readily understand the occurrence of the two different conditions. Moreover, we have said several times, that inhibition or stimulation are only relative conditions depending on the length of path along which the stimulus has to travel, and the rapidity with which it travels. The length of path remaining the same, the occurrence of stimulation or inhibition depends upon the rapidity of passage of the stimulus. The same length of path which is just sufficient to throw successive impulses of a slowly travelling stimulation half a wave-length behind the other, and produce inhibition, may be just sufficient to throw the vibrations of another more rapidly transmitted stimulus a whole wave-length behind, and produce increased instead of diminished action.

If the hypothesis that inhibition is produced by interference be true, we shall be able to test it by seeing whether stimulation of certain nerves which, under the ordinary conditions produce inhibition, do so when the rate of transmission of nervous impulses is altered. The length of path being the same, if we alter the rapidity of transmission it is probable that as the rapidity diminishes, the inhibition will be converted into stimulation, again possibly passing into inhibition, according as the stimuli, which we normally suppose to be half a wave-length behind each other, are thrown a whole wave-length, or a wave-length and a half behind each other. At a certain period, also, the waves of stimulation will be neither a whole nor a half wave-length behind each other, but the fraction of a wave-length. In such cases we shall neither have constant coincidence, nor constant interference, but we shall have rhythmical coincidence and rhythmical interference, the result of which will be that we shall neither get constant motion, nor constant arrest of motion, but alternate motion and rest. In other words we shall neither have complete rest nor tonic contractions, but intermittent or clonic contractions. Now this condition is exactly what we do find when one sciatic of a frog is irritated twenty-four hours after it has been exposed. We have already mentioned that when irritated immediately after exposure it had the effect simply of abolishing reflex action in the other leg; but the same irritation applied in the same manner after many hours, instead of causing arrest in the other leg, causes clonic convulsions.[1]

This occurrence is very hard to explain on the ordinary hypothesis of separate and distinct inhibitory centres, but it agrees perfectly with the hypothesis that inhibition and stimulation are merely relative conditions.

I have repeated Nothnagel's experiments, but I have not got the same results. Irritation of the sciatic nerve indeed caused a certain diminution in reflex at first, but irritation after twenty-four hours caused no clonic convulsions, it merely appeared somewhat to stimulate reflex action in the other leg. The reason of this discrepancy in our results is probably that the temperature was different in the two cases. Nothnagel's results were published in March, and his experiments were probably performed during cold weather, while mine were done during very mild weather. If the effects which he noticed were due to definite inhibitory centres in the spinal cord similar experiments should have had similar results in his hands and mine. If on the other hand the effects simply depend on the rate of the transmission of nervous impulses it is easy to understand why the results were different in the two cases.

There are also certain phenomena connected with the action of drugs on the spinal cord which are almost inexplicable on the ordinary hypothesis, but which are readily explained on that of interference. Thus belladonna when given to frogs causes gradually increasing weakness of respiration and movement, until at length voluntary and respiratory movements are entirely abolished, and the afferent and efferent nerves are greatly weakened. Later still, both afferent and efferent nerves are completely paralysed, and the only sign of vitality is an occasional and hardly perceptible beat of the heart and retention of irritability in the striated muscles. The animal appears to be dead, and was believed to be dead, until Fraser made the observation that if allowed to remain in this condition for four or five days, the apparent death passed away and was succeeded by a state of spinal excitement. The forearms passed from a state of complete flaccidity to one of rigid tonic contraction. The respiratory movements reappeared; the cardiac action became stronger, and the posterior extremities extended. In this condition a touch upon the skin caused violent tetanus usually opisthotonic, lasting from two to ten seconds, and succeeded by a series of clonic spasms. A little later still the convulsions change their character and become emprosthotonic. These symptoms are due to the action of the poison upon the spinal cord itself, for they continue independently in the parts connected with each segment of the cord when it has been divided.

This action may be imitated by a combination of a paralysing and exciting agent such as strychnia and methyl-strychnia. Fraser concludes that the effects of large doses of atropia just described are due to a combined stimulant and paralysing action of the substance on the cord, and that the difference in the relations of these effects to each other, which are seen in different species of animals, may be explained by this combination acting on special varieties of organisation.

T. LAUDER BRUNTON

(*To be continued.*)

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[1] Nothnagel, Centralblatt f. d med. Wiss. March 28, 1869, p. 211.

NOTES

THE Queen has signified her intention of opening the International Fisheries Exhibition, at South Kensington, on Saturday, May 12.

BARON NORDENSKJÖLD writes to us that he has definitely settled to start for the interior from Auleitsivik Fjord on the west coast, and then, in September, to go round Cape Farewell along the east coast to the north.

A MOST interesting letter has been received at Kew Observatory from Mr. Cooksley, of Capt. Dawson's expedition to Fort Rae. They arrived on August 30, started the meteorologica observations on September 1, and the magnetical observations on September 3. Apparently all was well at the date of the letter, December 19, 1882.

Mr. William Henry M. Christie, F.R.S., Astronomer Royal, has been elected by the Committee to be a Member of the Athenæum Club, under Rule 2, which provides for the admission of persons distinguished in literature, science, or the arts, or for public services.

M. Dumas was not able to be present at Monday's sitting of the Academy of Sciences. His recovery is not quite so rapid as it was hoped and expected to be.

In the Civil Service Estimates for 1883-4 the total vote for education, science, and art amounts to £4,748,556, a net increase of £165,531 over the previous year.

The sixth International Congress of Orientalists will be opened at Leyden on September 10 next.

Mr. Milne, who has recently returned to his post in Japan, has suggested to the Japanese Government the great utility of establishing a series of observations for the study of earthquakes; earth-tremors; earth-pulsations; earth-oscillations, or permanent changes of level; terrestrial magnetism; fluctuations of underground water; earth temperatures; eruptive phenomena, &c. We trust that the Japanese Government will see it to be their interest, in a land of earthquakes, as well as the interest of science, to take the advice of Mr. Milne, who has already done so much for seismology. Mr. Milne writes that he is more and more convinced that there are "earthquakes" of so slow period that neither observers nor ordinary instruments record them. The Japanese papers report that a volcano in the Asuma Yuma range has burst out.

Mr. A. H. Keane has been elected Corresponding Member of the Italian Anthropological Society.

Mr. Robert Lindsay has been appointed Curator of the Edinburgh Botanic Garden.

A special general meeting of members only of the Association for the Improvement of Geometrical Teaching will be held at 8 p.m., on March 20, at University College, (1) to authorise the publication of Books i. and ii. of the Elementary Geometry as revised by the committee; (2) to appoint three trustees of the property of the Association.

The Institution of Naval Architects began its annual meeting yesterday, and continues to-day and to-morrow. Among the papers in the programme are the following:—On certain points of importance in the construction of ships of war, by Capt. G. H. Noel, R.N.; The influence of the Board of Trade rules for boilers upon the commercial marine, by J. T. Milton; Sea-going torpedo-boats, by M. J. A. Normand; Some experiments to test the resistance of a first-class torpedo boat, by A. F. Yarrow; On the modes of estimating the strains to which steamers are subject, by Wigham Richardson; On the extinctive effect of free water on the rolling of ships, by P. Watts; A description of a method of investigation of screw propeller efficiency, by H. B. Froude; The speed and form of steamships considered in relation to length of voyage, by James Hamilton; On fog-signalling, by J. MacFarlane Gray; Method of obtaining the desired displacement in designing ships, by R. Zimmermann.

The Royal Commissioners for Technical Education—Messrs. Samuelson, M.P., Woodall, M.P., P. Magnus, and Swire Smith—accompanied by Mr. G. R. Redgrave (secretary), visited Birmingham on March 8, and devoted several days to a careful inspection of the Mason College, Midland Institute, &c. The Commissioners were much interested in the system of practical science instruction which is being carried on in the Board Schools under the direction of Mr. Jerome Harrison, F.G.S., and both heard lessons given in the new Icknield Street Schools, and examined the newly built laboratory, &c. We hope shortly to present to our readers an account of the system by which about 2500 of the elder boys and girls in the Birmingham Board Schools are now receiving lessons in elementary science, at, practically, little or no extra cost to the town of Birmingham.

It is proposed to establish the new Professorship of Physiology at Cambridge in the ensuing Easter term. The appointment of a Professor of Pathology is also declared by the General Board of Studies to be urgent. The Medical Board has recently unanimously reported that the appointment of a Professor of Surgery is urgently necessary; and Prof. Humphrey has offered to resign the Professorship of Anatomy and accept the Professorship of Surgery for the present, without stipend.

The death is announced of William Desborough Cooley, the author of a History of Geographical Discovery, a Physical Geography, and other geographical works, and who at one time wrote largely on theoretical African geography.

The half-yearly General Meeting of the Scottish Meteorological Society will be held to-day. The business before the meeting is: (1) Report from the Council of the Society; (2) Address by Prof. Piazzi Smyth, at request of the Council, on Rainband Spectroscopy; (3) the Meteorology of Ben Nevis in 1882, by Clement L. Wragge.

The *Réforme*, the new Paris paper, which has established telegraphic communication with London, publishes daily a translation of the previsions issued by the Meteorological Board of London, which is read by the French public at the same time as in England.

M. Lalanne, Member of the Academy of Sciences, has been elected a Life Senator in the Liberal interest. It seems to be becoming almost a constant practice of the French Senate to select its "Irremovables" from among the several classes of the Institute.

An Electrotechnical Society has been formed at Vienna, similar to the one existing and flourishing at Berlin.

The German astronomers who had proceeded to Punta Arenas in Magellan's Straits in order to observe the last transit of Venus have at last returned to Germany.

A meteoric stone weighing a hundredweight fell near Alfianello, near Brescia, on February 16 last. It entered the ground to a depth of two metres, and caused a shock like that of a slight earthquake.

A memoir, for which a gold medal (600 francs) has been awarded by the Belgian Academy, is by Prof. Fredericq, of Liege; it is on the influence of the nervous system on the regulation of temperature in warm-blooded animals. After many experiments, the author affirms that cold acts on the sensitive nerves of the skin, and through them on centres of thermogenesis in the *medulla oblongata*. These centres react, and through centrifugal nerves cause an increase of the phenomena of interstitial combustion, especially in the muscles; but we also fight with cold by a diminution of the losses of heat, the vessels of the skin being constricted, owing to an excitation of the vaso-constrictor centres, through impression of the sensitive nerves of the skin by cold. M. Fredericq considers that the system does not (as most physiologists say) contend against heat by diminishing the production of heat. The regulation of temperature is simply based on increase of the losses of heat, by dilatation of the cutaneous vessels, by acceleration of the outer circulation, increased secretion and evaporation of sweat, and greater admission of air to the lungs. The vaso-dilator nerve centres (sudorific and respiratory) are excited directly by superheated blood.

AN interesting trial of an electrically-moved tramcar took place on Monday at Kew, and, notwithstanding some inevitable hitches, may be regarded as fairly successful. The peculiarity of the application of electricity in the present case lies in the use made of accumulators. The car was constructed at the Electrical Power Storage Company's works at Millwall, and is of the usual dimensions for carrying forty-six inside and outside passengers. It weighs, with its accumulators and machinery, but without any passengers, four and a half tons. Under the inside seats of this tramcar is placed the accumulator, consisting of fifty Faure-Sellon-Volckmar cells, each measuring 13 inches by 11 inches by 7 inches, and each weighing about 80 lbs. This accumulator, when fully charged, is capable of working the tramcar with its maximum load for seven hours, which means half a day of tramway service. From the accumulators the current is communicated by insulated wire to a Siemens' dynamo placed underneath the car, which acts as a motor, the motion being transmitted to the axle of the wheels through a driving-belt. To start the car the current is switched on from the accumulator to the dynamo, the armature of which being set in motion, the power is communicated to the driving wheels. The car can be driven from either end, and the power required can be exactly apportioned to the work to be done by using a greater or lesser number of cells. On a level road, for instance, with a light load, only a comparatively small number of cells will be necessary, but with a heavy load or on a rising gradient greater power will be required, and additional cells must be switched in. The action of the motor, and consequently the direction of the car, can be readily reversed by reversing the current, and the car can also be as readily stopped by shutting off the current entirely and applying the handbrake with which the car is fitted. At night the car is lighted by means of four Swan incandescent lamps, two of which are placed under the roof and one at each end of the car. All the lamps derive their current from the accumulator. The car is also fitted with electric bells, worked from the same source, and is to be run regularly on the Acton tramway line. The Storage Company also had a successful trial on Monday at Kew of a launch fitted with a battery of forty cells and a Siemens' dynamo.

WE learn from the last number of the *Journal* of the Russian Chemical and Physical Society (1883, fascicule 1) that, at a recent meeting of the Society, Prof. Mendeléeff made a communication on the applicability of the third law of Newton to the mechanical explanation of chemical substitutions, and especially to the expression of the structure of hydrocarbons. If we admit not only the substitution of hydrogen by methyl, but also the substitution of CH_2 by H_2, and of CH by H_3—as must be according to the law of substitutions as deduced from the third law of Newton—we can not only explain, but also predict, all cases of isomerism, without recurring to the usual conceptions as to the connections and atomicities of elements. Thus, benzene can be understood as a normal butane, $CH_3CH_2CH_2CH_3$, or $(CH_3CH_2)_2$, where a double symmetrical substitution of H_3 by CH has taken place, the H_2 having been taken from CH_3 and the third H from CH_2, so that only the CH groups are left; benzene being thus $= \left(\begin{matrix}\mathrm{CHCH}\\ \mathrm{CH}\end{matrix}\right)^2$. It would explain the isomer of benzene, dipropargyl the formation from acetylene, and the substitution and addition products from benzene.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythræus* ♂) from India, presented by Mr. C. F. Henshaw; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. F. C. H. Dadswell; a Herring Gull (*Larus argentatus*), British, presented by Miss Ella Vicars; three Common Swans (*Cygnus olor*), British, presented by Mr. J. Hargreaves; four Prairie Grouse (*Tetrao cupido*) from Iowa, North America, presented by Mr. Henry Nash; a Daubenton's Curassow (*Crax daubentoni* ♀) from Venezuela, presented by Mr. Rowland Ward, F.Z.S.; a North American Turkey (*Meleagris gallo-pavo* ♂) from North America, presented by His Grace the Duke of Argyll, K.T., F.R.S.; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, deposited; a Gaimard's Rat Kangaroo (*Hypsiprymnus gaimardi* ♀), three Coypu Rats (*Myopotamus coypus*), born in the Gardens.

GEOGRAPHY OF THE CAUCASUS

OF the several branches of the Russian Geographical Society, the Caucasian and the East Siberian are well known for the amount of valuable geographical work they have done during the thirty years or so of their existence. The high scientific interest connected with the exploration of the Caucasus is obvious. The scientific exploration of the Alps has revealed to us a new world; but the highlands of the Caucasus, with the high plateaux of Transcaucasia, afford a still greater variety of geological and physico-geographical features than the Alps; besides, situated as they are on the boundary between the moist climate of the west and the dry one of the east, between the deeply-indented coasts of Europe and the deserts and plateaux of Asia, between the young civilisations of the west and the old civilisations of the east, the Caucasian highlands afford such a variety of climatic, botanical, zoological, and ethnological features as hardly can be met with in any other country of the world. Very much remains to be done to bring these highlands within the domain of scientific knowledge. In what has been done up to the present, the Caucasian branch of the Russian Geographical Society has always had a good share, either by direct exploration, or by bringing to the knowledge of the scientific world such explorations as otherwise would have remained unknown in the archives of different Government offices, or by giving a scientific character to such explorations as were made for military or diplomatic purposes. Besides, the activity of the Caucasian Geographical Society is not limited to the Caucasus. Closely connected with the General Staff of Tiflis, it extends its explorations to the Trans-Caspian region, to Asia Minor, and to Persia; and closely follows the Russian military expeditions, surveyors, and diplomatists who eagerly visit these countries.

Unfortunately the publications of the Caucasian branch—the *Zapiski* or *Memoirs*, and the *Izvestia* or *Bulletin*—are but very insufficiently known abroad, *Petermann's Mittheilungen* being nearly the sole channel through which they are brought to the notice of the scientific world. The following summary, therefore, of the last publications of the Society will be of some use to scientific geographers. Without attempting to review all the volumes of the *Memoirs* and *Izvestia* which have appeared, we shall limit this paper to a review of the two last of each, the chief results of the papers contained in former volumes being already embodied in Elisée Reclus's "Géographie Universelle."

Several papers of the sixth volume of the *Isvestia* are devoted to the geodesy of the Caucasus and adjacent countries. During the war of 1878 a considerable amount of geodetical work was done in the province of Kars and in Asia Minor, and M. Kulberg gives the latitudes and longitudes determined. The longitudes of Kars, Erzerum, and Mysun were determined by means of telegraphic signals (the accuracy of this method being such as to reduce the probable error between Pulkova and Vladivostok, on the Pacific, to 0″·14, that is, to 50 yards on a distance of 7000 miles). Other longitudes were determined by chronometer. A trigonometrical network was extended to Erzerum, and numerous surveys were made. The longitudes of several points at Constantinople were determined with great accuracy by General Stebnitzky, as well as that of Batum by M. Kulberg.

The same volume contains also a list of latitudes and longitudes determined on the banks of the Emba and on the Mangishlak peninsula.—M. Kulberg contributes also an interesting paper on the results of determinations of lengths of the pendulum on the Caucusus, in order to determine the increase of gravity caused by the Caucasian chain. The observations were made at

Tiflis, Elizabethpol, Dushet, Gudaur, and Vladikavkaz with the same pendulums that were used for a similar purpose in Russia and afterwards in India. It results from the observations that in all the above-named localities, the lengths of the seconds pendulum are less than the calculated ones, namely, by 0·0037 Paris lines at Batum, 0·0455 at Elizabethpol, 0·0445 at Tiflis, 0·0476 at Vladikavkaz, 0·1171 at Dushet, and 0·1226 at Gudaur. Thus, the geoid (or the true figure of the earth's surface, as determined by the directions of the pendulum) nearly corresponds with the spheroid on the shores of the Black Sea; it rises above it by 1587 feet at Tiflis, and by 1622 feet at Elizabethpol. It rises further north, reaching 4175 feet at Dushet, and 4371 at Gudaur, but soon falls, and has at Vladikavkaz, on the northern slope of the main chain, nearly the same height as at Tiflis, that is, 1697 feet above the spheroid.

The purely geographical papers are numerous:—M. Bakradze contributes a paper on the Batum province,—the Saatabago of antiquity,—and the basin of the Chorokh River, inclosed by mountains 10,000 feet high, and often of volcanic origin. The vegetation of the province is perhaps still more luxuriant than in other parts on the coasts of the Black Sea, where it altogether develops with a prodigious strength, owing to the great amount of rain; vines cover the trees in the coast district. But the country is thinly peopled. The old Georgian population is forgetting its language, and is disappearing from the upper parts of the basin of the Chorokh; the Lazes occupy only nineteen hamlets; the Armenians number no more than 570 houses; the Abhazes and Circassians, who have immigrated from the Caucasus, and Kurds are also scarce.—Another paper, by M. Levashoff, gives a detailed description of the mountains on the left bank of the Chorokh, between Batum and Artvin; these mountains are spurs of the Anti-Taurus chain which terminates close by the Chorokh in the peak Kvahid, 10,390 feet high. The left affluents of the Chorokh flow in narrow gorges, the bottom of which, and sometimes the slopes, are occupied by hamlets of Mussulman Gurians. Each of these gorges has its own individuality, and communication between them is very difficult. The small villages of each gorge are quite isolated from those of a neighbouring gorge. The fields of Indian corn and rice are often scratched on the small terraces on the slopes of mountains, often at a height of 3000 feet above the sea-level, and close by ruins of old small fortresses, each of which has its own legend. The tributaries of the Chorokh become wild streams after each rain, and the avalanches are dangerous enemies. The forests, which cover the mountains from top to bottom, are peopled with bears, wolves, and foxes. Further down, towards the sea-coast, the gorges become wider, and their bottom is covered with gardens. The Chorokh itself has a breadth of twenty-five to fifty yards, and runs with such rapidity that the *kayouks*, or local boats, managed with great skill through the rapids, pass the distance from Artvin to Batum (more than fifty miles) in four or five hours.—We notice also in the same volume a paper on the villayet of Trebizond, translated from the German; the letter of Mr Gifford Palgrave on vestiges of glacial action in North-eastern Anatolia, translated from a former volume of NATURE; the account of a party who undertook to climb the Elbrouz, but stopped 3500 feet short of its summit; and a notice on Western Daghestan.—M. Chernyavsky gives a detailed description of periodical phenomena in the life of plants at Sukhum-kaleh, during the autumn, winter, and spring of the years 1871 to 1875.

M. Seidlitz contributes a note on goitre and cretinism on the Caucasus. It is spread in several valleys of the main chain, especially in the Upper Svanetia; in the valley of the Tzhenis-tzhali many cases of cretinism were noticed. Altogether the small people of Svanets, which numbers only 12,000 souls, seem to be in a state of degeneracy, and ought to have an infusion of fresh blood from without. The goitre was noticed also in adjacent parts of the upper basin of the Rion river, among the Osets. On the northern slope of the Caucasus, west of the Kazbek peak, as well as in the basin of the Kuban, the goitre was not noticed; but it is known in Western Daghestan and in the valleys of the Andian Koysou ridge. It is cured by the waters of springs containing carbonic acid. Women are more subject to this disease than men. Another disease, of hysterical character, endemic to the same locality, is worthy of notice. The men and women affected bark like dogs, and the aborigines consider it as the result of bewitching, in which the "barking grass," as the Avars say (a kind of *Orchis*), is used by the bewitchers. In the Anti-Caucasus goitre was noticed in the Nakhichevan district and in the Batum province. It is always endemic, and never takes an epidemic character, as was the case in 1877 at Kokan, in Turkestan, where 9 per cent. of the soldiers and officers were seized with this disease after a year's stay at Kokan.

The ethnography of the Caucasus occupies a large place in this volume of the *Izvestia*. M. Zagursky contributes a note on the supposed kinship of the Osets with the Etruscans, and shows that it would be rather difficult to establish this kinship on account of a want of likeness between the Osetian language and the little we know about the language of the Etruscans.—Prof. Patkanoff contributes a valuable paper on the place occupied by the Armenian language among other Indo-European languages. He concludes that, and shows why, the question still remains open. Several linguists consider the Armenian language as decidedly belonging to the Iranian group, whilst others classify it with the European group. Lagarde distinguishes in it three elements: the Haikan, the Arkasid, and the Sassanid elements; the two latter are Iranian, but the Haikan element belongs to a family of languages the oldest of which is the Zend. Hubschmann concludes that it occupies an intermediate place between the Iranian languages and the Slavo-Lithuanians; and Fr. Muller, a partisan of its Iranian origin, admits that it has some kinship with the Slavo-Lithuanian languages. Prof. Patkanoff concludes that it occupies an intermediate place between these two, and is a representative of an extinct group of Indo-European languages, which formerly was spread perhaps in Asia Minor.—We notice also several notes on the dolmens of the Maykop district; on the descriptions of the first physical training given to children by different Caucasian peoples (these interesting descriptions, comprising nearly all Caucasian peoples, were sent to Moscow to Dr. Pokrovsky); on archæological discoveries in the province of Kuban, &c.

The *Izvestia* contain also many interesting short notices on the scientific work done on the Caucasus by other Societies and private persons; and bibliographical notices on different works dealing with the Caucasus. Elisée Reclus's description of the Caucasus in the "Géographie Universelle" is considered as the best that has yet appeared, and it is proposed to translate it into Russian, with notes and additions.

The Appendix contains several valuable papers, namely: a note on the Bosphorus and Constantinople, by M. Stebnitzky (with a map), containing some new information on currents in the Bosphorus and on the mean temperature at Pera, according to new observations of M. Kumbari (14°·3 Cels.); a note on the Aysors of the province of Erivan; a note on the population of Turkish Armenia, by M. Eritsoff (1,162,957, out of which 214,350 are Turks, 357,577 Kurds, 498,007 Armenians, 41,682 Kizilbashes, 25,516 Greeks, and 17,400 Aysors); and several translations.

The geodetical part is represented in the seventh volume of the *Izvestia* by a paper by M. Kulberg, on the influence of the oscillations of the supporting disc of the pendulum of the Russian Academy on the measured length of the seconds pendulum. The correction due to this cause was found to be equal to +0·0650 Paris lines, which correction closely corresponds to the difference between the Russian pendulum and that of Cater, which was found at Kew to be equal to 0·0056 inches, or 0·0631 Paris lines. The corrected lengths of the seconds pendulum at the above-named localities (at 13° Réaumur, and reduced to the sea-level) would be thus: 440·2734 Paris lines at the Tiflis Observatory, 440·3279 at Vladikavkaz, 440·2126 at Gudaur, 440·2018 at Dushet, 440·3172 at Batum, and 440·2364 at Elizabethpol.—A biographical notice of the late Gen. Khodzko gives an account of the immense work he performed for the triangulation of the Caucasus. He began this work in 1847 with the Anti-Caucasus, always taking for himself the most difficult parts of the work, such as the measurements on the summit of Alaghoz (13,436 feet high), or of Ararat (16,916 feet), 6000 feet above the snow line, and of other high summits. On June 28, 1851, he observed an eclipse of the sun on the summit of Galavdur, at a height of 10,380 feet, and noticed the protuberances which were doubted at that time as belonging to the atmosphere of the sun. The geodetical determination of 1386 points in Trans-Caucasia was terminated in 1854, but that of Northern Caucasus was begun only in 1860, and was connected with those of Russia in 1864. The accuracy of this immense work and its importance for geodesy and physical geography are well known.

The same volume contains several valuable geographical papers and maps. Among the latter the first place belongs to those of the frontier between Russia and Persia, from the Caspian to Babadurmaz, and of the frontier between Russia and Turkey, from the Black Sea to Ararat; both are accompanied with maps.—General Stebnitzky contributes a most valuable sketch of all that is known about the Pontian range, which follows the southern coast of the Black Sea from the Yeshil-irmak to the Chorokh.—M. Stepanoff contributes an interesting paper on the province of Kars, recently annexed to Russia; and M. Bakradze one on the ethnography of the same province. The province consists of three different parts: the lowlands of the basin of the Olti River, covered with clay hills intersected with irrigation canals, and offering great advantages for gardening; the 5000 to 6000 feet high plateau of Kars, 50 miles long and 35 miles wide, bordered with mountains the highest of which reaches 9700 feet. It is covered with lavas and basalts, deeply cut by rivers; the mountains are devoid of wood; agriculture is carried on on this plateau, notwithstanding its great height. The third part of the province is again a plateau, 6000 to 7000 feet high, where agriculture becomes impossible, but covered with good pasture-land, and dotted with lakes. The population of the province has suffered much from wars. In the basin of the Olti and in the north-east it was formerly Georgian, who have become Mussulmans; the Kurds make one-sixth of the population. The basins of the Araxes and Kars rivers were formerly occupied by Armenians. The capital of Armenia, Ani, now in ruins, was situated here. After 1830, no less than 90,000 Armenians emigrated into Russian dominions, whilst Turks, Turcomans, Karapakhs, and Caucasian emigrants (Kabards and Osets) occupied their place, forming thus a most mixed population. Presently the Mussulmans emigrated back from the province (no less than 65,447 souls during two years), and 7100 Russian Nonconformists have occupied their place, as well as 10,000 Greeks and about 4100 Armenians. The migration of whole populations is thus still going on in our times, as it was going on formerly after the great wars of the past. It is easy to foresee that the country contains most remarkable Armenian antiquities, such as churches built in the ninth and tenth centuries.

Since the year 1880 the director of the Tiflis Observatory, M. Milberg, has undertaken a series of measurements of the temperature of the ground, together with measurements of temperature by a black-bulb thermometer suspended 1·5 metres above the ground, and M. Smirnoff analyses the results of these measurements. The blackened thermometer has given a somewhat higher average temperature for the year than the usual thermometer suspended in shade (12°·7 Celsius, instead of 11°·6); the same was observed, as is known, in England. At the same time its maxima are obviously higher and its minima are lower than those of the usual thermometer in shade, its range being from −14°·5 to +42°·9, instead of −12°·0 to +37°·6; whilst the range of average temperatures of different months was 28°·6 instead of 27°·5 in the shade. The underground thermometers were placed at depths of 1, 2, 5, 12, 20, 41, and 79 centimetres, and were observed, the six former every hour, and the last each three hours. Two other thermometers, placed at depths of 1·6 and 3·5 metres, were observed once a day. The whole series of observations is published in the *Memoirs* of the Caucasian Agricultural Society, and the *Izvestia* give the monthly averages, as well as a *résumé* of the results. We shall add to this *résumé* that the observations at Tiflis show well the retardation of seasons at a depth of 79 centimetres, the coldest and warmest months being February and August, instead of January and July. The frosts at the spot where the observations were made do not penetrate deeper than 40 centimetres.—M. Maslovsky gives some observations of temperature at Askhabad, in the Akhaltekke oasis, during the summer months; the moisture in May was but 31 to 33 per cent., falling as low as 17 per cent., and reaching sometimes 59 per cent.—M. Chernyavsky gives the Abkhaze, Mingrelian, and Georgian names of different plants.

Several papers deal with the population of the Caucasus: M. Zagursky has contributed a paper on the ethnographical maps of the Caucasus, and, after having sharply criticised the works of M. Rittich, recommends as the best ethnographical map of the Caucasus, that which was published by M. Seidlitz in *Petermann's Mittheilungen*, and in which M. Zagursky has embodied the results of the little-known but remarkable linguistic works of the late General Uslar. Still this map leaves much to desire and ought to be accompanied by an explanatory memoir.—The much-debated question as to the number of Armenians in the Russian dominions is discussed by M. Eritsoff, who comes to the conclusion that it must be (taking into account the increase of population until 1881) 860,456 on the Caucasus, and 56,536 in European Russia.—M. von Eckert gives the results of anthropological measurements he has made, according to the instructions of Virchow, on 30 Adighes, 7 Ingushes, 11 Georgians, 14 Osets, 14 Armenians, 9 Aderbijan Tartars, and 80 Little-Russians from the Government of Kharkoff. They proved to be all brachycephalic, the average indexes being 80·7 for the Osets, 80·9 for the Tartars, 81·9 for the Ingushes, 82·0 for the Adighes, 82·2 for the Little-Russians, 83·3 for the Georgians, and 86·5 for the Armenians. The percentage of broad faces (*chamäprosop* faces, that is, those where the breadth between the cheek-bones is less than 89·9 per cent. of the length of the face, measured from the upper part of the nose to the lowest part of the chin) is 44 for Tartars, 64 for Armenians, 71 to 77 for Osets, Georgians, and Adighes, 86 for Ingushes, and 90 for Little-Russians.

The same volume contains several notes: on the Charjui; a list of heights in the Aderbijan; on the Scotch colony at Kuras and many others; and a bibliographical notice, by M. Stebnitzky, of Elisée Reclus's description of the Caucasus, which is spoken of in high terms.—The Appendix contains the translation, with notes, of the memoir, by Major Trotter, on the Kurds in Asia Minor, and of the Consular Report of W. Gifford Palgrave on the provinces of Trebizond, Sivas, and Kastamuni.

The eleventh volume of the *Memoirs* of the Caucasian Geographical Society contains three papers by M. Petrusevitch: on the Turcomans between the Uzboy and the northern borders of Persia; on the north-eastern provinces of Khorassan; and on the south-eastern coast of the Caspian and the routes to Merv. Some of these papers are already known to English geographers; and the others probably will be translated in full. They are accompanied by a map of the Russian Trans-Caspian dominions and of Northern Persia.

The twelfth volume of the *Memoirs* contains the first part of a large work, by the late General Uslar, on the ancient history of the Caucasus. It deals with the oldest traditions about the Caucasus, and is a most remarkable attempt at a *scientific* inquiry into the remotest history of this country. It is accompanied by a biographical notice of General Uslar, by M. Zagursky, his collaborator and follower. It is certain that M. Uslar, who pursued for many years the truly scientific exploration of Caucasian languages (undertaken first by Sjögren), has done in this branch far more than anybody else. But his works—which were only lithographed in a few copies, and each of which is not only a serious study of separate languages, but also a thorough description of the nation it deals with—are very little known, and this only from the short reports that were made on them by the late Member of the Russian Academy of Sciences, M. Schiefner. The few pages in which M. Zagursky gives an account of the work of Uslar, of the methods he followed, and of the results he arrived at, ought to be translated in full, as surely they would be most welcome to all those in England who are interested in the study of ethnology. They deserve much more than a short notice.

P. K.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Prof. Moseley and Prof. Burdon Sanderson have been appointed *ex officio* Members of the Board of the Faculty of Natural Science.

Prof. Clifton has been elected a Member of the Hebdomadal Council in place of the late Prof. Smith.

The Professorship of Archæology and Art, founded by the late Commissioners out of the revenues of Lincoln College, has been in abeyance owing to the proposed statute not having received the Queen's assent. The College now proposes to endow the professorship, and a statute will be promulgated at the beginning of next term, providing for a Professor of Classical Archæology and Art, "who shall lecture on the arts and manufactures, monuments, coins, and inscriptions of classical antiquity, and on Asiatic and Egyptian antiquities, or on some of those subjects."

Mr. G. A. Buckmaster, B.A., and late Natural-Science Demy of Magdalen College, has, after examination, been elected to the Radcliffe Travelling Fellowship. Mr. Buckmaster also obtained the Burdett Coutts Scholarship for proficiency in geology in 1882. The Fellowship is of the annual value of 200*l*., tenable for three years. The candidate must declare that he intends to graduate in medicine in the University of Oxford, and to travel abroad with a view to his improvement in that study. A Fellow forfeits his Fellowship by spending more than eighteen months within the United Kingdom

SCIENTIFIC SERIALS

Journal of the Franklin Institute, February.—An account of certain tests of the transverse strength and stiffness of large spruce beams, by G. Lanza —The abstraction of heat by mechanical energy, by J. Rowbotham.—On the application of the principle of virtual velocities to the determination of the deflection and stresses of frames, by G. F. Swain.—Cone pulleys, by H. W. Spangler.—Dust explosions in breweries, by C. J. Hexamer.—A summary of progress in science and industry, 1882.

THE January number of the *Revue d'Anthropologie* (Premier Fasc., 1883), contains the first part of a valuable memoir—unfortunately left incomplete by Paul Broca at the time of his death—on the cerebral convolutions of the human brain, as shown by casts. Broca, having found from long experience that it is almost impossible to obtain specimens of a normal cerebrum in which both hemispheres are symmetrical, devoted his attention to the preparation, for the special use of students, of exact models of the convolutions divested of the secondary folds, whose extreme variability makes it difficult to determine their true character. The memoir now first printed supplies an exhaustive description of the brain at every stage from fœtal to senile life, with explanations of the significance of the different colours used in the preparation of the models, which have been completed under the superintendence of M. S. Pozzi.—"Buffon Anthropologiste" is the title of a paper by M. P. Topinard, in which he has reprinted the main part of a lecture previously addressed to his class in the École d'Anthropologie. The object of the address is to show that Buffon was the precursor of Darwin and Lamark, both as to the theory of development from one, or at most a few original types, and in his belief in the survival of the fittest. His undoubted contradictions M. Topinard ascribes to the necessity of the times, which compelled him to respect the opinions of the clergy so far as to address to the Faculty of Theology a written retractation of fourteen propositions contained in his "Histoire Naturelle," which that body had condemned. This curious document is here given *in extenso*.—M. C. Sabatier, a former *juge de paix* in Kabylia, in an article on "La femme kabyle," explains the nature of the enactments by which the French Government is endeavouring to ameliorate the condition of women among the Kabyles, who till the present time have virtually been slaves, being treated alike by their fathers and husbands as the least valued of chattels. As the result of long discussions with the heads of the tribes, two new "kanouns," or laws, have been agreed to and put into force, which M. Sabatier believes to be decisive steps towards the social regeneration of the men as much as of the women, one of these enactments restricting the rights of the father to give his daughter in marriage before she has reached a fixed age, and the other freeing a wife from the control of her husband under certain conditions of desertion and neglect.—MM. Corre and Roussel's report of their observations of 200 crania of criminals preserved in the Anatomical Museum of Brest is supplied with various tables exemplifying their precise cranial characteristics, the nature of the crimes committed, the birth-place of the criminals, &c. The general conclusions are in complete accord with those of Bordier, Broca, &c.

Archives des Sciences Physiques et Naturelles, January 15.—On a refractometer for measuring the indices of refraction and the dispersion of solid bodies, by M. Soret.—Theoretical and experimental study of a rapid vessel, by M. Pictet.—On the apparent forces arising from the terrestrial motion, by M. Cellérier.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 12, 1882.—Considerations on the stratigraphic relations of the psammites of Condroz and the schists of the Famenne properly so-called; also on the classification of these Devonian deposits, by M. Mourlon.—Second note on the dynamo-electric machine with solenoid inductor, by M. Plucker.—Determination of the general law ruling the dilatability of any liquid chemically defined, by M. de Heen.—On the aurora borealis of November 17, 1882, by M. Terby.—Reports on prize competitions, &c.—The great discoveries made in physics since the end of last century (lecture at public *séance*), by M. Montigny.—Dwarfs and giants (lecture), by M. Delbœuf.

The Proceedings of the Linnean Society of New South Wales; vol. vii. Part 2 (April–June, 1882); Part 3 (July–September, 1882). The chief contents are, *Botanical:* Botanical notes on Queensland. No. 2, the tropics; No. 3, the Mulgrave River; No. 4, Myrtaceæ.—On a coal-plant from Queensland, by Rev. J. E Tenison-Woods.—Half-century of plants new to South Queensland, by the Rev. B. Scortechini.—Forage-plants indigenous to New South Wales, by Dr. Woolls.—On *Myoporum platycarpum*, a resin-producing tree of the interior of New South Wales, by K. H. Bennett.—Botanical notes in the neighbourhood of Sydney, by E. Haviland.—*Zoological:* On a new Gobiesox from Tasmania; on two new birds from the Solomons; on a new Coris from Lord Howe's Island, by E. P. Ramsay.—Australian Micro-lepidoptera, No. 7, by E. Meyrick.—On a reported poisonous fly from New Caledonia; new species of fish from New Guinea and Port Jackson; on an insect injurious to the vine, by Wm. Macleay.—On a new species of Allopora, by Rev. J. E. Tenison-Woods.—On Australian freshwater sponges; on the brain of *Galeocerdo rayneri;* monograph of Australian Aphroditea (Plates 6 to 11); notes on anatomy of pigeons, by W. A. Haswell.—Some new Queensland fishes; on a new species of squill from Moreton Bay, by W. de Vis, B.A.—Habitat of *Cypræa citrina*, of Gray, by J. Brazier.—New variety of *Ovulum depressum*, found at Lifou, by R. C. Rossiter.—On a breeding place of *Platalea flavipes* and *Ardea pacifica*, by K. H. Bennett.—*Geological:* Physical structure and geology of Australia, by Rev. J. E. Tenison-Woods.

Journal of the Asiatic Society of Bengal, vol. li. Part 2, Nos. 2 and 3, 1882 (December 30, 1882) contains:—Some new or rare species of Rhopalocerous Lepidoptera from the Indian region, by Major G. F. L. Marshall, R.E. (Pl. 4).—On an abnormality in the horns of the Hog-deer (*Axis porcinus*), with an amplification of the theory of the evolution of the antlers in ruminants, by John Cockburn.—On the habits of a little-known lizard (*Brachysaura ornata*), by John Cockburn.—Second list of butterflies taken in Sikkim in October, 1882, by L. de Nicéville.

Morphologisches Jahrbuch, eine Zeitschrift fur Anatomie und Entwickelungsgeschichte, Bd. 8, Heft 3, contains:—The nasal cavities and lachrymo-nasal canals in amniotic vertebrata, by Dr. E. Legal.—The structure of the hydroid polyps, by Dr. Carl F. Jickeli (Plates 16–18).—The tarsus in the birds and Dinosaurs, by G. Baur (Plates 19 and 20).—Contribution to a knowledge of the development of the vertebral column in Teleostians, by Dr. B. Grassi.—On an hypothesis concerning the phylogenitic derivativion of the blood system of a portion of the Metazoa, by Dr. O. Butschli.

Reale Istituto Lombardo di Scienze e Lettere Rendiconti, vol. xv. fasc. xx.—Reports on prize-awards; announcements of prize-subjects, &c.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 15.—"Description of an Apparatus employed at the Kew Observatory, Richmond, for the Examination of the Dark Glasses and Mirrors of Sextants." By G. M. Whipple, B.Sc., Superintendent.

In the *Proc. Roy. Soc.* for 1867, Prof. Balfour Stewart described an apparatus designed and constructed by Mr. T. Cooke for the determination of the errors of graduation of sextants. This instrument has from that date been constantly in use at the Kew Observatory, and since the introduction of certain unimportant improvements has been found to work very well.

No provision was made, however, for its employment in the determination of the errors of the dark shades used to screen the observer's eyes when the sextant is directed to the sun or moon, and it has been found that errors may exist in the shape of want of parallelism in these glasses, sufficiently large to seriously affect an observation accurate in other respects.

It has also been found that sextant makers are desirous of having the shades examined before proceeding to fit them into their metal mountings, and also to have the surfaces of the mirrors tested for distortion before making the instruments up. With a view to the accomplishment of these ends, for some time past the Kew Committee have undertaken to examine both dark glasses and mirrors, and to mark them with a hall-mark when they are found to answer the requirements necessary for exactitude.

For these purposes the following apparatus has been devised by the author, and brought into use at the Observatory.

A telescope of $3\frac{1}{4}$ inches aperture and 48 inches focal length, a pair of collimators of $1\frac{1}{4}$ inch aperture and 10 inches focal length, and a heliostat, are firmly fixed to a stout plank, so that their axes may be in the same horizontal plane. The eyepiece of the telescope carries a parallel wire micrometer.

In order to adjust the instrument, the telescope is directed to the sun, a shade being fitted to the eyepiece and then placed in its Y's focused for parallel rays. The collimators are then fixed on their table with their object-glasses opposed to that of the telescope, the eyepieces and wires having first been removed, and a metal plate with a sharply-cut hole in its centre fitted to their diaphragms.

Light is next reflected down the collimator by the heliostat, and the aperture in the diaphragm being viewed through the telescope, is carefully focused by moving the object-glass of the collimator to and fro by means of its rack and pinion.

The diaphragm aperture is next collimated by rotating the collimator in its bearings.

Both collimators being thus adjusted, they are placed side by side, so that their illuminated sights can be viewed simultaneously in the telescope, appearing as superimposed bright disks 12′ in diameter. They are next separated so that the disks remain merely in contact at the extremity of their horizontal diameters.

The instrument is now ready for use, and the examination of the shades is performed in the following manner:—

The glass to be tested is fixed in a rotating frame in front of the object-glass of one collimator, a corresponding shade being placed between the heliostat and diaphragm of the other collimator. The sun is now directed on to the diaphragms. The coloured disks are viewed through the telescope, when, if the sides of the shade, placed between the collimator and the object-glass of the telescope, are perfectly parallel, the relative position of the disks is unchanged; if, however, the shade is not ground true, the disks will appear either separated or to overlap. In the first case the amount of separation is measured by the micrometer, and serves to indicate the quality of the glass. In the case of overlapping images the shade is rotated through 180°, and separation produced which can be measured. A second examination is then made, the shade having been turned through 90°.

If in no position a separation of images is found to exist to the extent of 20″, the glass is etched K.O. 1; if more than 20″ but less than 40″, the mark is K.O. 2; with greater distortion than this, the shade is rejected and not marked.

To examine the quality of the mirrors, a small table, on levelling screws, is put in front of the object-glass of the telescope. The mirror to be tested is placed on its edge on this table, and turned until a distant well-defined object is reflected down the tube of the telescope. The object-glass of the telescope having previously been stopped down to an aperture corresponding to the size of the mirror, the reflected image is contrasted with that seen directly, and if the definition is unchanged the mirror is marked K.O. with a writing diamond, and returned to the maker; if the object appears distorted, its unfitness for use is similarly notified. A small fee is charged for the examination.

Geological Society, February 7.—J. W. Hulke, F.R.S., president, in the chair.—G. D'Arcy Adams, Prof. Ferdinand Moritz Krausé, and the Rev. Alfred William Rowe were elected Fellows, and Dr. Karl A. Zittel, of Munich, a Foreign Correspondent of the Society.—The following communications were read:—On the metamorphic and overlying rocks in parts of Ross and Inverness shires, by Henry Hicks, F.G.S., with petrological notes by Prof. T. G. Bonney, F.R.S. In this paper the author described numerous sections which have been examined by him in three separate visits made to the north-west Highlands. In some previous papers, sections in the neighbourhood of Loch Maree had been chiefly referred to. Those now described are to the south and south-east of that area, and occur in the neighbourhoods of Achmashellach, Strathcarron, Loch Carron, Loch Kishorn, Attadale, Strome Ferry, Loch Alsh, and in the more central areas about Loch Shiel and Loch Eil to the Caledonian Canal. In these examinations the author paid special attention to the stratigraphical evidence, to see whether there were any indications which could in any way be relied upon to prove the theory propounded by Sir R. Murchison that in these areas fossiliferous Lower Silurian rocks dip under thousands of feet of the highly crystalline schists which form the mountains in the more central areas. On careful examination he found that in consequence of frequent dislocations in the strata the newer rocks were frequently made to appear to dip under the highly crystalline series to the east, though in reality the appearance in each case was easily seen to be due to accidental causes. Evidences of dislocation along this line were most marked; and the same rocks in consequence were seldom found brought together. He recognised in these eastern areas at least two great groups of crystalline schists metamorphosed throughout in all the districts examined, even when regularly bedded and not disturbed or contorted; and they have representatives in the western areas, among the Hebridean series, which cannot in any way be differentiated from them. These he called locally by the names, in descending order, of Ben-Fyn, and Loch-Shiel series. The former consist, in their upper part, of silvery mica-schists and gneisses, with white felspar and quartz; in their lower part, of hornblendic rocks, with bands of pink felspar and quartz, and of chloritic and epidotic rocks and schists. The Loch-Shiel series consists chiefly of massive granitoid gneisses and hornblendic and black mica-schists. Thirty-three microscopical sections of the crystalline schists and the overlying rocks are described by Prof. Bonney, and he recognises amongst them three well-marked types. In No. 1 he includes the Torridon Sandstone, the quartzites and the supposed overlying flaggy beds on the east side of Glen Laggan. These are partially metamorphosed, only distinct fragments are always easily recognisable in them in abundance. In No. 2, the Ben-Fyn type, the rocks are crystalline throughout, being typical gneisses and mica-schists. In No. 3, the Loch-Shiel series, he recognises highly typical granitic gneisses of the Lower Hebridean type. Dr. Hicks failed to find in these areas at any point the actual passage from group 1 to group 2; neither did the same rocks belonging to group 1 meet usually the same rocks belonging to group 2. The evidence everywhere showed clearly that the contacts between these two groups were either produced by faults or by overlapping. Group 3, placed by Murchison as the highest beds in a synclinal trough, supported by the fossiliferous rocks, the author regarded as composed of the oldest rocks in a broken anticlinal. They are the most highly crystalline rocks in these areas; and the beds of group 2 are thrown off on either side in broken folds. These, again, support the rocks belonging to group 1. The author therefore feels perfectly satisfied that the crystalline schists belonging to groups 2 and 3, which compose the mountains in the central areas, do not repose conformably upon the Lower Silurian rocks of the north-west areas with fossils, and that these highly-crystalline rocks cannot therefore be the metamorphosed equivalents of the comparatively unaltered, yet highly disturbed and crumpled, richly fossiliferous Silurian strata of the southern Highlands, but are, like other truly crystalline schists examined by him in the British Isles, evidently of pre-Cambrian age. In an Appendix by Prof. T. G. Bonney, F.R.S., on the lithological characters of a series of Scotch rocks collected by Dr. Hicks, the author stated that he observed in the above series, as he had done in other Scotch rocks lately examined by him, three rather well-marked types—one, where, though there is a certain amount of metamorphism among the finer constituents forming the matrix, all the larger grains, quartz, felspar, and perhaps mica, are of clastic origin; a second, while preserving a bedded structure and never likely to be mistaken for an igneous rock, being indubitably of clastic origin, retains no certain trace of original fragments; while the third, the typical "old gneiss" of the Hebridean region, seldom exhibits well-marked foliation. It is sometimes difficult to distinguish between the first and second of these; but this the author believed to be generally due to the extraordinary amount of pressure which some of these Scotch rocks have undergone, which makes it very hard to determine precisely what structures are original. Even the coarse gneiss is sometimes locally crushed into a schistose rock of comparatively modern aspect. The least altered of the above series the author considered to be the true "newer-gneiss" series of the Highlands, but both of the others to be much older than the Torridon Sandstone.—On the Lower Carboniferous rocks in the Forest of Dean, as represented in typical sections at Drybrook, by E. Wethered, F.G.S., with an appendix by Dr. Thomas Wright.

Chemical Society, March 1.—Dr. Gilbert, president, in the chair.—The following gentlemen were elected Fellows:—A. C. Abraham, G. Board, C. N. Betts, E. Bevan, F. J. Cox, A. Collenette, S. Dyson, W. T. Elliott, H. B. Fulton, C. G. Grenfell, B. F. Halford, W. D. Hogg, D. Hooper, J. J. Knight, H. F. Lowe, T. H. Leeming, J. E. Marsh, W. Newton, C. Rumble, F. Scudder, J. O'Sullivan, S. A. Vasey, T. D. Watson, R. M. Walmsley, C. S. S. Webster, F. Watts.—The following papers were read:—On some derivatives of the isomeric $C_{10}H_{14}O$ phenols, by H. E. Armstrong and E. H. Rennie. Lallemand stated that a trinitro-thymol was produced by the action of a mixture of nitric and sulphuric acids on dinitrothymol. The authors find that a trinitro body is formed, but that it has the constitution and properties of trinitrometacresol. The authors could not obtain a trinitro body from carvacrol. When thymolsulphonic acid is treated with nitric acid, paranitrothymol is formed, the sulpho group being displaced. When bromothymolsulphonic acid is treated with chromic acid, an amorphous quinone is formed, but when permanganate is used, no quinone is produced. The authors have also studied the action of nitric acid on bromisobutylsulphonic acid.—Chemico-microscopical researches on the cell-contents of certain plants, by A. B. Griffiths. The author has grown cabbage plants on soils containing ferrous salts: the plants are larger, and their ash contains a considerable quantity of oxide of iron. In sections under the microscope crystals are visible which belong to the monoclinic system and give a blue colour with potassium ferricyanide and an opacity with barium chloride. The author concludes that they consist of ferrous sulphate.—The phenates of amido bases, by R. S. Dale and C. Schorlemmer. The authors have satisfied themselves that, when aurin is heated with ammonia, pararosanilin is at once formed. When aurin is heated with common rosanilin and alcohol, a solution is produced which on concentration yields a crystalline powder of rosanilin aurate; similarly by heating anilin and phenol in molecular proportions, anilin phenate is obtained in glistening plates melting at 29°·5, boiling 184°·5.

Anthropological Institute, February 27.—Prof. W. H. Flower, F.R.S., president, in the chair.—The election of Mr. C. Fountaine Walker was announced.—Dr. Garson exhibited and described a series of photographs of cases of hypertrichosis.—Mr. A. Tylor read a paper on the homological nature of the human skeleton. He finds that in the skull of all vertebrate animals, including man, a general resemblance to the trunk and limbs is carried out—for instance, variations in the limbs are accompanied by variations in the jaws, and the occiput varies with the pelvis, the sternum with the palate, and so on throughout the skull and body. This is due to mechanical causes. Bones, like the parts of plants, consist of stalks and leaves; the stalk-element is shown in the vertebræ and the long bones, and the leaf-element in the apophyses, the plate-bones of the skull, such as the parietals, &c. The elemental shaft-bones always bulge at the extremities where pressure is exerted, hence the peculiar form of all such bones. This form is a mechanical necessity, and, in accordance with the known laws of correlation and repetition of parts, helps us to understand the singular relations subsisting between the skull and the rest of the skeleton.

Institution of Civil Engineers, March 6.—Mr. Brunlees, president, in the chair.—The first paper read was on the productive power and efficiency of machine tools, and of other labour-saving appliances, worked by hydraulic pressure, by Mr. Ralph Hart Tweddell, M.Inst.C.E.—The second paper read was on stamping and welding under the steam-hammer, by Mr. Alexander McDonnell, M.Inst.C.E.

Sydney

Linnean Society of New South Wales, December 27, 1882.—Dr. James C. Cox, F.L.S., president, in the chair.—The following papers were read:—Occasional notes on plants indigenous in the neighbourhood of Sydney, No. 2, by Edwin Haviland. This paper treats chiefly of the construction and habits of *Utricularia dichotoma*.—Description of a new *Belideus* from Northern Queensland, by Charles W. De Vis, B.A.—A paper by the same author describing two new Queensland fishes (*Callionymus achates* and *Mugil nasutus*).—By the Rev. Dr. Woolls, on the species of Eucalyptus first known in Europe. Of the twelve species described by Willdenow, eleven are from the immediate neighbourhood of Sydney, and one only from Tasmania. This tree, the Tasmanian Stringy Bark (*E. obliqua*), was the first Eucalypt known in Europe, the specimen having been collected during Furneaux's voyage. On it L'Héritier founded the genus, 1788. The early descriptions are, as it may be supposed, very vague and imperfect, and their identification has been a matter of much difficulty and hesitation, now happily removed.—On some new species of tubicolous annelides, by William A. Haswell, M.A., B.Sc.—On new species of *Agaricus* discovered in Western Australia, by the Rev. C. Kalchbrenner.—On some points in the anatomy of the urogenital organs in females of certain species of kangaroos, Part 1, by J. J. Fletcher, M.A., B.Sc.—The Rev. J. E. Tenison-Woods read a paper on a species of *Brachyphyllum*, which was found in the Tivoli coal mine. In many respects this species resembled the well-known *B. mamillare* of the British and Continental Oolite, but lest any confusion should arise from a doubtful identification, and as the stems and leaves of this specimen were much thicker, and the leaves more fleshy than in *B. mamillare*, the author distinguished it as *B. crassum*. He considered that the discovery of this specimen served to place the Jurassic age of the Ipswich (Queensland) coal beds beyond much doubt.—A note was read by Dr. H. B. Guppy, of H.M.S. *Lark*, on the cocoa-nut eating habit of the *Birgus* of the Solomon Islands. Dr. Guppy had no doubt from what he had observed that the Robber-Crab is in the habit of breaking open the shells of the cocoa-nuts with its powerful chelæ.—Mr. Haswell stated that he had much pleasure in announcing to the Society that, thanks to the intelligent inquiries made by Mr. Morton of the Museum, while recently in Queensland, he had hopes that they were on the way towards learning something of the embryology of the *Ceratodus*. Mr. Morton had ascertained that the *Ceratodus* spawns in the Burnett River during the months of June, July, or August, the spawn being deposited in a slight excavation formed in the bed of the river at a depth of eight or ten feet, the male and female remaining in close attendance on it until hatched. Arrangements had been made by which it was hoped that a supply of the spawn might be obtained for observation next season.

Paris

Academy of Sciences, February 26.—M. Blanchard in the chair.—The death of Baron Cloquet, Member in Medicine and Surgery, was announced.—The following papers were read:—Note on various points of celestial physics, by M. Janssen. At Meudon Observatory they are studying movements of photospheric matter with the aid of series of images obtained with the "photographic revolver"; they are also working at photographic photometry, the principle being that the intensities of two light-sources are in the inverse ratio of the time they take for the same photographic work (*e.g.* producing the same tint on two quite similar plates). The method will be applied to data of the comet of 1881, the full moon, &c. M. Janssen further hopes to present soon a complete study of the spectrum of aqueous vapour.—Results of a new series of experiments on the apparatus for transport of mechanical work installed on the Chemin de fer du Nord, by M. Deprez: note by M. Tresca (see p. 422).—On the heat of formation of chromic acid, by M. Berthelot.—Rain in the Isthmus of Panama, by M. de Lesseps. A table of observations of rainfall by Mr. John Stiven, for 1879–1882, shows that 1879 was an extraordinarily rainy year (2·152 m.), a large excess occurring in November. The rain-season lasts nearly six months, from May to November, excepting an interruption of a few weeks in June and July. This is explained by the behaviour of the ascending body of air which accompanies the curve of maxima in its annual oscillation on either side of the thermal equator, which movement is connected with the annual movement of the sun. The trade-winds north and south also affect the phenomena.—On the bronze tools used by miners in Peru, by M. Boussingault. A bronze chisel found in an old quarry of trachyte near Quito, evidently served in working the trachyte (softened by water); it contains copper 95, tin 4·5, with minute quantities of lead, iron, and silver.—Nebulæ discovered and observed at Marseilles Observatory, by M. Stephan.—Exhalation of nitrogen in a gaseous state during respiration of animals, by M. Reiset. MM. Petenkoffer and Voit negated such exhalation (affirmed by the author). Recent experiments by MM. Seegen and Nowak confirm M. Reiset's view.—Direct and rapid attenuation of virulent growths by the action of heat, by M. Chauveau. The method may be applied to liquids of artificial cultivation with much better success than to the natural humours of the system, and it may be graduated at will according to the degree of attenuation desired.—Contribution to the study of refrigeration of the human body in hyperthermic diseases, especially typhoid fever, by M. du Montpellier. He indicates the useful effects of his cooling apparatus.—Researches on the division of acids and bases in solution by the method of congelation of the solvents, by M. Raoult.—On the relations between covariants, &c. (continued), by M. Perrin.—On the theory of uniform functions, by M. Goursat.—Note on a point of the theory of continuous periodic fractions, by M. de Jonquières.—Remarks on a communication of M. de Chardonnet on the vision of ultra-violet radiations, by M. Mascart. He thinks the conclusions too absolute; he showed some years ago that ordinary sight habitually perceives the whole ultra-violet solar spectrum as lavender grey, and some eyes see even further.—On the increase of intensity of scintillation of stars during auroras, by M. Montigny. (Already noted elsewhere.)—On the production of apatites and of bromised Wagnerites with lime base, by M. Ditte.—Researches on the action of zinc-ethyl on amines and phosphines; new method of characterising the nature of these bodies, by M. Gal.—On the products of decomposition by water of fluoborised acetone *a*, by M. Landolf.—On neutralisation of glycolic acid by bases, by M. de Forcrand.—On a new base of the quinoleic series, phenol-quinoleine, by M. Grimaux.—Derivatives of strychnine, by M. Hanriot. He describes a new dinitro-strychnine, also diamido-strychnine.—On sulphocyanacetone, by MM. Tcherniac and Hellon.—Chloronitrated camphor, by M. Cazeneuve.—On the ice plant, by M. Heckel. His observations some years ago agree with those of M. Mangon.—Researches on the chromatophores of the *Sepiola Rondeletii*, by M. Girod. He regards the protoplasm of the pigmentary cell as the agent of extension; the basilar cell producing contraction.—On the disease of saffrons known as *Tacon*, by M. Prillieux.—On an inversion of temperature observed at a point of the Alps on December 27, 1882, by M. Henry. M. Broch noted a similar case near Christiania, where a rich banker has a châlet at a height of 408 m. In winter the temperature there is often about zero, while at Christiania it is 10 or 15 degrees below zero.—M. Daubrée indicated the contents of a new publication from Lima, *Anales de Construcciones civiles y de Minas del Perú*.

March 5.—M. Blanchard in the chair.—The following papers were read:—Observations of the satellites of Neptune, of Uranus, and of Saturn, with the equatorial of the eastern tower of Paris Observatory, by MM. Henry; Note by M. Mouchez. A new objective having been put in the instrument (acquired in 1849, under Arago) renders it the best instrument the Observatory has ever had.—Nebulæ discovered, &c. (continued), by M. Stephan.—The prolific power of virulent agents that are attenuated by heat, and the transmission by generation of the attenuating influence of a first heating, by M. Chauveau. The attenuation does not involve any alteration of the vitality or prolific power of the agents deprived, by heat, of their infectious properties. It is also shown that the influence is not merely individual, but may appear in the properties of new agents arising through proliferation of the protoplasm which has been directly subject to it.—M. de Lesseps stated that he was about to go to the region of the North African Chotts for a month, to consider the investigations of M. Roudaire.—A letter from M. Nordenskjöld referred to his intended departure for Greenland in August. He believes that vast regions covered with perpetual ice are a physical impossibility on our globe south of the 80th degree of N. lat., and goes to the interior of Greenland to test this view.—On the importance of the *rôle* of inhibition in therapeutics, by M. Brown-Séquard. A morbid activity will disappear suddenly, or nearly so, on irritation at some point (to be sought) more or less distant from that at which the activity prevails.—Practical use of sulpho-carbonate of potassium against Phylloxera in the south of France, by M. Culeron.—On the perturbations of Saturn due to the action of Jupiter, by M. Gaillot.—Observations of the great comet of September, 1882 (II. 1882), made at the Observatory of the Transit of Venus Mission at Martinique, by M. Bigourdan.—Observations of the new comet (Brooks and Swift) made at Paris Observatory (equatorial of the western tower), by the same.—Observations of the same comet at Lyons Observatory with the 6-inch Brunner equatorial, by M. Gonessiat. The comet appeared as a bright, nearly round nebulosity, with nucleus well condensed. In a clear sky, a straight tail of about 13′ long was observed. (M. Bigourdan estimates the brightness as about that of a star of 6th or 7th magnitude.)—On the approximation of sums of numerical functions, by M. Halphen.—On the series of polynomes, by M. Poincaré.—On the trajectories of different points of a connecting-rod in motion, by M. Léauté.—On the theory of electromagnetic machines, by M. Joubert. He calls attention to the loss of work in continuous-current machines through change of direction of the current in the coils of the ring.—On a new collimator, by M. Thollon. The slit is made to take any direction, while its image remains fixed. This is effected by means of a total-reflection prism placed behind the slit, with its hypothenuse face parallel both to the axis of the collimator and to the slit.—Dissociation of the bromhydrate of phosphuretted hydrogen, by M. Isambert.—On sulphuric chlorhydrate, by M. Ogier.—On chloride of pyrosulphuryl, by the same.—Heat of formation of solid glycolates, by M. de Forcrand.—On the hydrocarbons of peats, by M. Durin. From an examination of fresh mosses, he thinks it probable (with M. Dumas) that the hydrocarbons of peat are not formed during vegetal decomposition, but that they existed already in the mosses which formed the peat.—Experiments proving that sanguineous concretions, formed at the surface of an injured part of vessels, begin with a deposit of hematoblasts, by M. Hayen.—On the chromatophores of Cephalopoda, by M. Blanchard. He holds that they do not differ at all in general structure from those of fishes, batrachians, and especially saurians (chameleon). The chromatophore is a sort of amœba charged with pigment, living for itself and independent of the skin which imprisons it; it is, however, under the influence of the nervous system. The radiating fibres are mere fibres of connective tissue, and M. Blanchard has never (like M. Girod) found them to vary in form with the chromatophore.—On a flagellate Infusoria, ectoparasite of fishes, by M. Henneguy. This was observed on trout. The name *Boda necator* is given provisionally.—On the *Gnetaceæ* of the coal formation of Rive-de-Gier, by M. Renault.—Selenetropism of plants, by M. Musset. Plants of phototropic sensibility were grown from seeds in pots in a very dark place; then, on three nights, exposed at a window to direct moonlight; the stems bent over towards the moon, and followed it in its course.

BERLIN

Physical Society, February 16.—Prof. Kirchhoff in the chair.—Prof. Krech described at length a spectrophotometer which he had made in 1872, and with which, in the years 1873, 1874, 1875, and 1876, he had made a large number of observations for verification of the theory of the apparatus and determination of its errors. The theory of the instrument and the improvements proposed were fully gone into; the experiments had been made before Herr Glau had described his spectrophotometer.

CONTENTS

PAGE
THE ZOOLOGICAL STATION IN NAPLES. By J. T. CUNNINGHAM 453
EPPING FOREST. By Prof. G. S. BOULGER 455
PERRY'S "PRACTICAL MECHANICS." By Dr. J. F. MAIN 456
OUR BOOK SHELF:—
  Friele's "Der Norske nord-hass-expedition, 1876-1878."—Dr. J. GWYN JEFFREYS, F.R.S 457
  Penhallow's "Tables for the Use of Students and Beginners in Vegetable Histology" 458
LETTERS TO THE EDITOR:—
  The Matter of Space.—Prof. A. S. HERSCHEL (*With Diagram*). 458
  Terrestrial Radiation and Prof. Tyndall's Observations.—Dr. A. WOBIKOF 460
  Diurnal Variation in the Velocity of the Wind.—E. DOUGLAS ARCHIBALD 461
  The Large Meteor of March 2, 1883.—W. F. DENNING; J. L. J. 461
  On the Movements of Air in Fissures and the Barometer.—A. STRAHAN 461
THE PITT-RIVERS COLLECTION 461
JOHN RICHARD GREEN 462
THE BOTANY OF THE "CHALLENGER" EXPEDITION. By W. BOTTING HEMSLEY 462
THE SHAPES OF LEAVES, II. By GRANT ALLEN (*With Illustrations*) 464
ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT, III. By Dr. T. LAUDER BRUNTON, F.R.S. 467
NOTES 468
GEOGRAPHY OF THE CAUCASUS 470
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 472
SCIENTIFIC SERIALS 473
SOCIETIES AND ACADEMIES 473

THURSDAY, MARCH 22, 1883

PATHOLOGICAL ANATOMY

A Text-Book of Pathological Anatomy and Pathogenesis. By Ernst Ziegler. Translated and Edited for English Students by Donald MacAlister, M.A., M.B., St. John's College, Cambridge. 8vo. (London: Macmillan and Co., 1883.)

FOR some years the student of medicine has felt the want of an English manual of modern Pathological Anatomy. He has been compelled either to trust entirely to his teacher, or to consult works and memoirs little adapted for beginners. This felt want the English edition of Ziegler's Pathology, when completed, will in great part meet. The author believing "that the learner gains a readier grasp of his subject when it is first presented to him as a uniform and coherent system of doctrine," has, by avoiding "much matter of controversy," succeeded in making a clear and concise statement of each subject treated. In this the author has been well seconded by the editor, who, by carefully revising and amending the original, by adding numerous references to English and French memoirs, and by otherwise with characteristic ability adapting the work for English readers, has greatly enhanced its value.

Although the authors have kept the student chiefly in view in preparing this manual, a glance at the small print and the numerous references given, will at once prove that those desirous of gaining an exhaustive knowledge of the subject, and those engaged in special investigations, have not been neglected. It seems to us that this is by far the best plan for a text-book. It is to be regretted that students at the present day read so little. In many instances they content themselves with "learning" in order afterwards to retail what they purchase from their teachers; or what is worse, when they are unfortunate enough not to have their teacher as one of their examiners, they "get up" an endless number of often useless facts, derived from all possible sources, before presenting themselves for examination. This waste of time and energy in great part results from the want of good text-books. The books available are generally too large, they are often quite beyond the grasp of the beginner, and at the same time not a little out of date. In order to be able to utilise fully the opportunities now offered for gaining a practical knowledge of pathology, and other allied subjects, lectures are not enough; there must be something to fall back upon, by means of which the impressions received from the teacher may be tested, something that will form a foundation on which an intelligent knowledge of the subject may be built. We believe that the work before us will serve this purpose, and that it will be equally useful to the teacher by enabling him to take for granted that the fundamental facts of his science can be again and again referred to as the student requires, and by providing short, concise statements which he can modify at will, and to which he can add much that is of historical interest, or that is too recent for any manual, however complete, to contain.

The volume now published deals with General Pathological Anatomy. It is divided into seven sections. Those on Malformations, Inflammation, Tumours, and Bacteria deserve especial mention. In treating these subjects the authors have been careful to avail themselves of all the recent investigations, not only in Pathology, but also in Embryology and other branches of Biology, and by making free use of small print and giving abundant references, they have succeeded in drawing up a more complete account than exists in any other English manual.

In a very suggestive introductory chapter some of the special terms used by pathologists are defined, and the functions of pathological anatomy indicated. In the section on the Formative Disturbances of Nutrition the researches of Strasburger and Flemming on the changes in cells and nuclei during subdivision are considered, and a diagram showing indirect cell-division is introduced. In speaking of cell-multiplication it is pointed out that the proposition, "The stronger the external stimulus the greater the proliferation," cannot be accepted; that "one can at most admit that very slight stimuli, sufficient merely to excite the cell without injuring it, may perhaps call into play its power of multiplication; but nothing has been experimentally established concerning the nature, the action, or the mode of application of such stimuli"; further, that "when the nutritive and formative activities of a cell are morbidly increased, the effect is due to augmentation of the physiological stimuli or diminution of the physiological resistances to growth, or the direct influence of external stimuli"; the factors probably favouring proliferation being (1) an increased capacity in the cell to assimilate nutriment, (2) an increased supply of nourishment, (3) the removal of the normal checks to growth. In the same chapter there is an account of the origin of epithelium, fibrous and adipose tissue, and of new blood-vessels; and, in the chapter immediately following, an account of the origin of pus-corpuscles and of the mode in which tissues are regenerated.

Tubercle and other allied diseases, such as lupus, leprosy, and glanders, are spoken of as "Infective Granulomata." A tubercle is defined from a histological point of view as "a non-vascular cellular nodule which does not grow beyond a certain size, and at a certain stage of its development becomes caseous"; but it is afterwards pointed out that when Koch's recent investigations are taken into consideration it must be spoken of as "a cellular nodule containing within it the specific tuberculous virus, the bacillus tuberculosis."

Among these infective granulomata we have the new disease known as "actinomycosis," which is associated with the presence of the peculiar fungus *Actinomyces*. In this disease the infection probably starts from the mouth, and results in the formation of granulations and fibrous tissue and in suppuration.

The classification of tumours has long been a puzzle to pathologists. Later writers have more and more recognised their relation to the embryonic layers, and now we have, we believe for the first time in an English text-book, a purely embryological arrangement, tumours being divided by the authors into: (1) those derived from the mesoblast,—the connective-tissue tumours; (2) those containing elements derived from epithelial cells—the epithelial tumours. This classification, which commends itself by its simplicity, is likely to be generally adopted.

The consideration of the different kinds of tumours is followed by a chapter on their ætiology, in which Cohnheim's embryonic hypothesis is discussed at some length, and the objections to its general acceptance pointed out. In answer to the question, How does the tumour assume properties distinct from those of its surroundings? there is as a reply, "We believe that the phenomenon is ultimately due to some change affecting individual elements of a tissue whereby they are rendered dissimilar to their neighbours." The change is manifested especially in this—that the normal checks to the indefinite growth of the proliferous cells are inoperative or inadequate, either because the formative and productive energy is increased, or because the restraining influence of the surrounding structures is diminished, or from both causes together.

The last section of the present volume is devoted to Parasites. On comparing the German account of animal parasites with the English, we note very considerable additions and improvements. The chapter contains a sufficiently complete account of the structure and life-history of the ordinary parasites for all practical purposes. The chapter on Bacteria is extremely valuable. The editor has been careful to incorporate in the text all the important recent discoveries, and references are given to all the memoirs that the student or investigator is likely to require to consult. We thus have in a connected form the results of numerous inquiries into the nature of the organisms which for some time have been claiming not a little of the attention of biologists and physicians. In describing the bacteria, reference is made to the influence of temperature and of the surrounding medium on their growth and development, also to the influence they exercise on the nutrient liquid, and to their presence without and within the living body.

In reference to the existence of bacteria within the body we read:—

"Bacteria are perpetually entering the body with the food we eat and the air we breathe. They must, therefore, be at times found in the tissues, especially in places where access is direct. The fact that they are not easy to demonstrate is readily explained. It must be only a small number that are able to multiply in the tissues they have penetrated; the majority must quickly perish." Bacteria are described as pathogenous and non-pathogenous, the latter being harmless unless the normal secretions undergo some alteration, or the bacteria develop to an unusual extent. Under such conditions, inflammation may be set up, or the whole system may be influenced by the absorption of the soluble products of decomposition, some of which are extremely poisonous, and capable according to Hiller, of altering or even destroying the tissues exposed to them. "The pathogenous bacteria have the power of settling, not merely in the ingesta and secretions, or in dead tissue, but also in living tissue. This happens chiefly in the mucous membranes and in the lungs. The uninjured skin is protected against invasion by the horny epidermis."

"Many of the bacteria can settle in perfectly healthy mucous membranes. In the case of others we must imagine that they do not find a proper soil for their development, unless the mucous membrane is injured or altered. Of course injury or alteration of this kind may seem to make the outer skin or any other accessible tissue the starting-point of a bacterial invasion (wound-infection). All that is necessary is that a bacterium should reach a spot that affords the conditions of its development. If this occurs, it multiplies and forms colonies or swarms. These may, according to the species of the fungus and the nature of its soil, remain in aggregation, forming heaps or masses, or may spread through the tissues. Such a settlement is never without effect on the affected tissues. The bacteria may force their way into the substance of the constituent elements, and especially into the tissue-cells, which are sometimes found to be crammed with bacteria."

All that is necessary is that a bacterium should reach a spot that affords the conditions for its development, *i.e.* "the temperature of the body must be such as favours its development; it must be able to abstract fit nutriment from the tissues in which it settles; it must nowhere encounter substances which check or injure it." When in the tissues, the increase of the bacteria may be arrested by the aggregation of living cells resulting from the inflammation they set up, assisted by the regenerative action of the fixed tissue-cells. If this does not happen, they spread into the surrounding tissues, usually reaching the lymphatics and blood-vessels, some to perish, others rapidly to multiply.

The bacteria are supposed to lead to disease by withdrawing nourishment, setting up chemical changes—partly by their direct action on the nutrient material, and partly by the action of the unorganised ferments they form; and finally, as a result of these changes, by producing poisonous matters. In doing this they enter into conflict with the tissue-cells, influencing their nutritive activity, changing them or even leading to their destruction. Whether it is a change in the fermentive action of the cells, or a disturbance of the functions of the central nervous system which leads to fever, has not been determined. Neither is it known whether the unsusceptible condition of the tissues which usually follows when the bacteria have been eliminated, results from "a modification in the chemical constitution of the tissues, or to a change in the vital activity of the cells."

In referring to the relation of bacteria to infective diseases it is stated "that among the infective diseases there are certainly some which are due to the invasion of a microphyte, and that it is highly probable the others have a like origin." This chapter further gives a short account of the various diseases which have been described as resulting from the influence of bacteria, and concludes by discussing the burning question of the present moment—the mutability of bacterial species. It is well known that Naegeli, Buchner, and others believe "that both the morphological and the physiological characters of the bacteria are mutable"; that "a given bacillus does not invariably produce bacilli of the same structure, and does not always pass through the same developmental stages." "A bacterium which, under given conditions, gives rise to a definite kind of fermentation, may lose this property when cultivated under different conditions." Koch and others believe that bacteria do not alter in their properties, and that "even when the nutrient medium is altered from time to time no recognisable differences are produced."

The authors point out that "at present we are unable to draw any certain conclusion regarding the relation of non-pathogenous to pathogenous bacteria. Clinical experience would indicate that the activity of the infective virus may vary within certain limits. And we must apparently admit that the infective bacteria have not always possessed their noxious qualities, but have acquired them somehow in the course of ages. But this is not enough to convince us that harmless bacteria can acquire infective properties rapidly. . . . We may therefore provisionally conclude that the transformation of innocuous into noxious bacteria can occur but rarely, and under special conditions."

Recent work both in this country and on the Continent seems to go against the mutability theory, and in all probability it will soon be made clear that Buchner's experiments are capable of another interpretation from that hitherto adopted.

Enough has been said to indicate that the English edition of Ziegler's Pathology will not only prove of immense help to the student, but that it will also be invaluable to the practitioner. It is to be hoped that the second part, on Special Pathological Anatomy, will soon appear, and that it will equally commend itself to English readers.

The numerous woodcuts with which the work is illustrated are beautifully distinct, the type and paper are everything that could be desired, and so successful has the editor been that there is no evidence of the greater part of the work being a translation.

ENSILAGE

Ensilage in America. By James E. Thorold Rogers, M.P. (London: W. Swan Sonnenschein and Co., 1883.)

PROFESSOR ROGERS has contributed a most interesting little book on Ensilage in America. He has no doubt been serviceable to his country in drawing public attention to a subject of importance; but like most persons who focus their eyes upon a single point, he has lost the due proportion in which it stands to its background, foreground, and surroundings. Perhaps this may be forgiven as a common fault, or it may be the secret of strength, in all propagandists. Be this as it may, it is a marked feature in the volume before us. Ensilage is to be the temporal salvation of the farmer. The Professor appears to have been carried away on the full tide of American enthusiasm, buoyed up by a certain youthful airiness scarcely consistent with the gravity of an Oxford Don. He has forgotten the salt, and those who read his book (and we trust they may be numbered by thousands) must add it for themselves.

Ensilage is the preservation of green fodder in its natural succulent condition in pits or *Silos*. These pits must be airtight and watertight, and the fodder must be so well trampled into them and weighted on the top as to arrest fermentation. The theory of the process is that, in the case of fodder so treated, heat is generated and fermentation commences. The small amount of oxygen held in the interstitial air is speedily absorbed, and its place taken by carbonic acid gas. Just as a lighted candle extinguishes itself in a bath of choke-damp of its own making when burnt in a closed vessel: so the fermentation and its accompanying heat are arrested in the mass of closely packed fodder which is in fact immersed in a bath of carbonic acid, and thus securely protected from ordinary atmospheric action. Well preserved ensilage comes out of the pit almost as green and fresh as when it was first put into it, and has acquired a pleasant vinous smell and slightly acid flavour, which has given it its name of sourhay in Germany, Austria, and Hungary. The process is at once simple and effective, but is no doubt expensive when carried out upon the scale which a successful experiment demands. Thus the larger the pit the more assured the success, as all the conditions are more perfectly attained. At p. 22 we read: "M. Havemeyer's silos were four—two fifty-nine feet long and fourteen feet wide; and two thirty-five feet long and twelve feet wide, each pair being twenty-five feet deep. They are under the same roof as the feeding barn, where there is standing-room for ninety-eight cows." The pits are bricked and cemented, or built with concrete walls, and they may be carried up higher than the level of the ground, or may be built entirely from the surface. When the ground is naturally dry and of a clayey or close texture the silo need not be lined. It is recommended that a drain should if possible be carried from the bottom of the silo to take off superfluous water. Simple as these directions undoubtedly are, they point to a heavy initial expenditure, only to be recommended after very mature consideration. On the other hand silos of smaller size, as, for example, 22′ × 9′ × 15′ deep and other dimensions, are also mentioned. Still the fact remains that in small silos there is more waste and greater uncertainty. Also that for practical purposes a small silo would be of little value. The process of storing the fodder is very easy to understand. It is, in the case of green maize, cut up with a powerful chaff-cutter, trampled into the pit by men or horses, and when the space is filled it is covered with boards and weighted down with boxes of stone or earth to a pressure of about 100 lbs. per square foot. The fodder settles down under pressure, and is found after several months to be perfectly palatable and fresh.

Such is the process which Prof. Rogers now lays before the British public with the strongest possible recommendations. Not only so, but with threatenings or at least warnings also, for we are told that "if the New Englanders and New Yorkers succeed in extending their ensilage system, they will strive to find a foreign market for their increased produce." This process, it is urged, is entirely to revolutionise agriculture. It is to be a new point of departure, a "new dispensation." "Is there not a bonanza (a mining term for peculiarly rich ore) in the farms with this new enterprise? Will it not give the farmer such profits, with less labour, as will enable him to be more independent? Is it not going to create new interests with our sons, when they can find a more profitable employment, with less hard labour than can be found in any business in our cities?" It is to double the population of "our New England cities," and indeed appears to be a veritable *El Dorado* for farmers.

In thus introducing ensilage to the attention of his countrymen, Prof. Rogers is scarcely cautious in the manner in which he discounts the value of scientific and especially of chemical opinion upon this subject. "Ensilage is to be the food of the future for pigs and poultry as well as for horses and bullocks." But it is only grass after all, and we can hardly believe that it can be superior to the herbage from which it was made. Pigs and poultry will graze in pastures it is true, but the digestive systems of these animals demand more concentrated foods. There is an evident tendency to "forget the rock from which it was hewn," if we may apply such words to the process of ensilage. It is green fodder *preserved* until winter. Well! if preserved until winter it cannot be eaten in summer. If eaten in summer it surely would also have been realised.

It may be better than hay, but we cannot expect from ensilage such superlative results above what might reasonably be expected from hay. Such high-flown anticipations as are embodied in Prof. Rogers's book are usually doomed to disappointment, and the process of ensilage will probably take its place in American and English agriculture as it has already taken its place in the agriculture of the Continent of Europe, among other improvements of the nineteenth century, but without overtopping any of them.

Prof. Rogers does not appear to have informed himself as to the state of our knowledge in England upon this topic. The process was fully described by the present writer for the Royal Agricultural Society in 1874. He also drew attention to it in two letters to the *Times* in 1875, when it evoked considerable discussion, under the title of "Potted Hay." The process was also both described and illustrated by drawings in the *Agricultural Gazette* at the same period. Since then it has been repeatedly tried, but in all cases without marked success. We are ready to allow that this want of success has been due to the experiments having been conducted upon a small scale and probably with too much regard to economy of outlay. The process is too generally successful in many countries to be capable of being challenged. So late, however, as 1875 Prof. Tormay of Pesth wrote to us that practical men were greatly divided as to its value. No doubt the making of sourhay deserves further trial, and there is as little doubt that it will be largely experimented upon during the coming summer. It must, however, be remembered that it may be as profitable to eat the herbage when growing, as to preserve it *in any form* for the winter. Also that our turnips, swedes, and mangels give us a means of producing meat in these countries which is not possessed by American agriculturists. Turnips and hay are probably a better combination of succulent and dry food for winter feeding than turnips and ensilage would be. In this matter we prefer to suspend judgment for a while upon the uses of ensilage to the British farmer. At the same time we cordially agree that it is likely to be particularly useful in the Eastern States of America, where the soil and climate are unfavourable to the growth of roots and favourable to the growth of maize. On this point we have abundant testimony in Prof. Rogers's book. A special attraction towards ensilage is that it can be carried out without delay in any weather, and that it saves the anxiety of haymaking.

Those who have tried it in this country complain that it is very difficult to keep the pit good when it has been once opened. Still the process is worthy of more extended trial, and if carried out without too much fear of the initial expense and risk of failure, may be shown to be of service to English agriculturists.

JOHN WRIGHTSON

OUR BOOK SHELF

Another Book of Scraps, principally relating to Natural History. With thirty-six Lithographic Illustrations from Pen and Ink Sketches of Wild Birds. By Charles Murray Adamson. (Newcastle-on-Tyne: Reid, 1882.)

MR. ADAMSON has been so much amused by the preparation of his first "Book of Scraps" that he has prepared another, and invites our opinion upon it. We cannot say that we have derived much information from looking through his letterpress, although we perfectly agree with him that the study of natural history, which he advocates, "opens out a wide field for the profitable employment of the mind." But the thirty-six illustrations which form the main portion of the book certainly show that the author has studied the forms and habits of wild birds to some purpose, although in an artistic point of view, perhaps, it would not be difficult to criticise the surroundings amongst which he places them. The drawings are a little rough, as Mr. Adamson himself confesses, but no naturalist can turn them over without recognising at once the species which are intended to be portrayed. We have seen pictures in the Royal Academy of which the same remark could not truthfully be made. Mr. Adamson is evidently most at home on the sea-shore. His sea-birds are best. With his woodcocks and partridges we are not so well satisfied. But "Another Book of Scraps" will make a nice addition to a drawing-room table.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Incubation of the Ostrich

I HAVE received the following letter from Mr. J. E. Harting, and with his permission send it to you for publication. I do so partly in justice to Mr. Harting himself—the letter having been originally written to the *Spectator* in vindication of his own accuracy, and having been rejected by the editor—and partly because I think it desirable that the point in natural history which it discusses should be definitely cleared from the erroneous views which, as I shall presently show, are still prevalent with regard to it.

ANIMAL INTELLIGENCE

To the Editor of the Spectator

SIR,—I have just read in your issue of February 3 a letter from Mr. G. J. Romanes to which a long editorial note is appended, and which raises an interesting question relating to the incubation of the ostrich. As my name is mentioned as having written something on the subject, perhaps you will allow me to offer a few remarks.

Briefly stated, the point under discussion is this: Mr. Romanes, in his recently-published work on "Animal Intelligence," has observed that in the case of the ostrich *the task of incubation is shared by both the sexes.*

In reviewing this work your critic alleges that "female ostriches take *no* part in the duty of incubation"—that is, they do not assist the male.

Whereupon Mr. Romanes cites his authorities for the statement made by him, and refers amongst other sources to my book on ostriches, published in 1876, wherein (at p. 41) I remark that "the males are polygamous, each associating with three or four hens, all of which lay their eggs in one large nest scooped out in the sand, and relieve each other by turns at incubation. Le Vaillant purposely watched an ostrich's nest, and during the day saw four hens sit successively on the same eggs, a male bird coming late in the evening to take his turn at incubation." A little further on, I added: "Incubation lasts six weeks, the cock-bird taking his turn at sitting like the hens."

Your reviewer, still sceptical, replies: "The passage in Mr. Harting's book is based on the statement of Le Vaillant, whose observations, except when confirmed by later experience, are justly discredited by the best-informed naturalists of the present day, as he was notoriously so often unworthy of belief."

Permit me to point out that in making the statement above quoted, I by no means relied *solely* on Le Vaillant. I had before me the evidence of several modern observers on the subject, whose publications are referred to in my "List of Works quoted," at the commencement of my volume. At p. 189 I have alluded to the experiments made at San Donato, near Florence, in 1859 and 1860, by Prince Demidoff, who says that "the female ostrich began to sit as soon as the first egg was laid, and sat for three hours daily, leaving the male for the rest of the time."

At p. 196, quoting a report forwarded in 1873 by a resident of experience in South Africa to the Council of the Zoological and Acclimatisation Society of Victoria, who were then contemplating the introduction of the ostrich into that colony, I find this distinct statement: "The process of hatching is performed by the male and female sitting alternately, one keeping a vigilant look-out as sentry, as well as procuring food."

Again, in a Report by Dr. W. G. Atherstone of Grahamstown, based on observations made by himself and friends on different ostrich farms in the neighbourhood of Grahamstown, and quoted by me *in extenso*, the following passage occurs on p. 202 of my book:—"They sit alternately, the male at night grazing and guarding the females. During the daytime, the time of the male bird going on the nest varies during the period of incubation, as also does the time between the female leaving the nest and the male taking her place, the exposure and cooling being probably regulated by the temperature of the incubation fever at different stages."

In addition to the evidence of these observers I had before me the testimony of Mr. F. Denny of Grahamstown, which is too long to be quoted here, but which will be found embodied in an interesting note published in the *Zoologist* for 1874 (p. 3916); so that I felt perfectly justified in asserting in effect, as Mr. Romanes has done, that *the task of incubation with the ostrich is shared by both the sexes*. It would be easy to adduce further evidence on the subject if necessary, but I will not occupy space further than to observe that if your reviewer will turn to p. 107 of Douglass's "Ostrich Taming in South Africa," published by Messrs. Cassell and Co. in 1881, he will see a full-page illustration thus lettered, "*Hen bird sitting*. From a photograph taken at Heatherton Towers."

Admirers of Le Vaillant will be glad to learn that in this case at least his assertions (to quote your reviewer) "have been confirmed by later experience," and are therefore not to be discredited.—I am, Sir, your obedient servant,

22, Regent's Park Road, N.W. J. E. HARTING

After such a battery of evidence it seems almost needless to adduce more; but as the point is an interesting one to ornithologists, I shall briefly add some corroborative proof from other sources.

In the *Spectator*, besides referring to the above, I gave a reference to two articles published by Mr. E. B. Biggar on the ostrich-farms of the Cape Colony, and also to the recently published work by Mr. Nicols; from each of these sources I shall now quote brief passages. Mr. Biggar writes as follows:—

"Some will sit throughout with the most solicitous maternal instinct; . . . others manifest such anxiety, that when the hen has been a little late in taking her morning turn upon the nest, he has gone out, and, hunting her up, has kicked her to the nest in the most unmanly manner. Some are very affectionate over their young, others the reverse; thus do individuals differ even among ostriches. As a rule the cock bird forms the nest, sits the longest, and takes the burden of the work of hatching and rearing. Contrary to what has been currently understood, and what is still stated even in recent colonial accounts, the cock bird sits at night, not the hen. In this peculiarity the hand of Providence may be seen, for the worst enemies of the nest appear at night, and the cock, being stronger and braver, is better able to resist them; moreover, the feathers of the cock being black, night sitting would not expose him to that exhaustion from the sun's rays which would ensue if he sat during the day; while at the same time the grey feathers of the female are less conspicuous while she sits during the day."—*Field*, August 21, 1880.

And again, "After turning the eggs over one by one with her beak, she will sit perhaps for hours with her head stretched flat and snake-like on the ground, and her body as motionless as a mound of earth. Occasionally, on hot days, she may be seen with her body lifted slightly out of the nest to admit a current of air over the eggs; and sometimes she will even leave the nest for two or three hours, till instinct tells her that the lowering temperature requires her return" (*Century*, January, 1883).

Mr. Nicol's work, entitled "Zoological Notes," repeatedly states that the hen bird assists the cock in the process of incubation, and on my writing to him to ask whether he had witnessed the fact, he answers that although he has not done so himself, a well-educated friend "who had passed some time in visiting ostrich-farms in South Africa" had done so; and, in answer to his express inquiry on the subject, wrote, "that the female took part in the task, though not nearly to so great an extent as the male," adding that he was surprised to hear there should be any question concerning a fact so well known to the ostrich farmers.

Lastly, having recently been to Florence, I took the opportunity of calling upon the superintendent and proprietor of the Zoological Gardens there, and obtained all the particulars of the case alluded to by Mr. Harting in the above letter as having occurred at San Donato. I found that two broods of young had been raised in successive years by the same pair of ostriches, and that on both occasions the female assisted the male to incubate the eggs: "que le male et la femelle couvent alternativement," in the words of the published report ("Guide du R. Jardin Zoologique de Florence," p. 81, 1868). Here, however, as in all the previously-mentioned cases, the fact which I stated in "Animal Intelligence" was apparent, viz. that the cock bird undertook the whole duty of sitting during the night.

Now when all this evidence is taken together it appears to me impossible to doubt that the female ostrich assists the male in the process of incubation. Yet from the fact of this evidence not having been clearly focused, an old error on the subject still appears to be prevalent. This error arose some twenty years ago from the observations of M. Noel Suchet (? or Suquet) on a pair of ostriches kept in confinement. Thus, in 1863, Dr. Sclater wrote:—"We now know with certainty from the observations of M. Noel Suchet, Director of the Zoological Gardens at Marseilles, that the normal habits of the ostrich (as regards incubation) do not differ materially from those of its allies of the same family" (*Proc. Zool. Soc.*, 1863, p. 233); and Mr. Darwin, following the judgment formed by Dr. Sclater, wrote in the "Descent of Man" (p. 479) that the male bird "undertakes the whole duty of incubation." Again, my reviewer in the *Spectator*—who, although curiously weak in his logic, appears to be strong in his ornithology—pins his faith entirely to this single observation of M. Suchet. Lastly, Prof. Newton in his article on "Birds" in the "Encyclopædia Britannica" (p. 771), relying, I presume, on the same observation, writes:—"A band of female ostriches scrape holes in the desert sand, and therein promiscuously dropping their eggs, cover them with earth, and leave the task of incubation to the male, who discharges the duty thus imposed upon him by night only, and trusts by day to the sun's rays for keeping up the needful fostering warmth."

Thus it appears that the influence of M. Suchet's observations has been very disproportionate to its merits, and has misled some of our principal ornithologists concerning the normal habits of ostriches.[1] Possibly Prof. Newton, with his extensive knowledge of the literature of such matters, and writing since the appearance of most of the counter-evidence which I have given, is cognisant of some other observations on which he rests his statement. But, if so, it becomes desirable that he should supply his references, as otherwise his statement appears to rest, as my reviewer in the *Spectator* would say, "simply on the survival of the old belief." GEORGE J. ROMANES

March 12

Difficult Cases of Mimicry

I HAVE received from Mr. Thos. Blakiston, of Tokio, Japan, a communication to the *Japan Mail* by himself and Prof. Alexander, commenting on my article in NATURE, vol. xxvi. p. 86, and pointing out some errors as to the estimated advantage derived by the mimicking butterflies. On referring to my article, I find that I have, by an oversight, misstated the mathematical solution of the problem as given by Dr. Fritz Müller and confirmed by Mr. Meldola, and have thus given rise to some confusion to persons who have not the original article in the *Proceedings of the Entomological Society* to refer to. Your readers will remember that the question at issue was the advantage gained by a distasteful, and therefore protected, species of butterfly, which resembled another distasteful species, owing to a certain number being annually destroyed by young insectivorous birds in gaining experience of their distastefulness. Dr. Müller says: "If both species are equally common, then both will derive the same benefit from their resemblance—each will save half the number of victims which it has to furnish to the inexperience of its foes. But if one species is commoner than the other, then the benefit is unequally divided, and the *proportional advantage* for each of the two species which arises from their resemblance is *as the square* of their relative numbers." This is undoubtedly correct, but in my article I stated it in other words, and incorrectly, thus: "If two species, both equally distasteful, resemble each other, then the number of individuals sacrificed is divided between them in the proportion of the square of their respective numbers; so that if one species (*a*) is twice as numerous as another (*b*), then (*b*) will lose only one-fourth as many individuals as it would do if it were quite unlike (*a*); and if it is only one-tenth as numerous, then it will benefit in the proportion of 100 to 1."

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[1] I may observe that Mr. R. B Sharpe, writing in "Cassell's Natural History" (vol. iv. p. 228), has not been thus misled, for he says distinctly that the cock and hen "relieve each other by turns."

This statement is shown by Messrs. Blakiston and Alexander to be untrue; but as some of your readers may not quite see how, if so, Dr. Müller's statement can be correct, it will be well to give some illustrative cases. Using small and easy figures, let us first suppose one species to be twice as numerous as the other, *a* having 2000 and *b* 1000 individuals, while the number required to be sacrified to the birds is 30. Then, if *b* were unlike *a* it would lose 30 out of 1000, but when they become so like each other as to be mistaken, they would lose only 30 between them, *a* losing 20, and *b* 10. Thus *b* would be 20 better off than before, and *a* only 10 better off; but the 20 gained by *b* is a gain on 1000, equal to a gain of 40 on 2000, or four times as much *in proportion* as the gain of *a*. In another case let us suppose *c* to consist of 10,000 individuals, *d* of 1000 only, and the number required to be sacrificed in order to teach the young birds to be 110 for each species. Then, when both became alike, they would lose 110 between them, *c* losing 100, *d* only 10. Thus *c* will gain only 10 on its total of 10,000, while *d* will gain 100 on its total of 1000, equal to 1000 on 10,000, or 100 times as much *proportional gain* as *c*. Thus, while the gain in actual numbers is inversely proportional to the numbers of the two species, the *proportional* gain of each is inversely as the *square* of the two numbers.

I am, however, not quite sure that this way of estimating the *proportionate* gain has any bearing on the problem. When the numbers are very unequal, the species having the smaller number of individuals will presumably be less flourishing, and perhaps on the road to extinction. By coming to be mistaken for a flourishing species it will gain an amount of advantage which may long preserve it as a species; but the advantage will be measured solely by the fraction of *its own numbers* saved from destruction, not by the proportion this saving bears to that of the other species. I am inclined to think, therefore, that the benefit derived by a species resembling another more numerous in individuals is really in inverse proportion to their respective numbers, and that the proportion of the squares adduced by Dr. Müller, although it undoubtedly exists, has no bearing on the difficulty to be explained. ALFRED R. WALLACE

MR. A. R. WALLACE has been so good as to forward me the extract from the *Japan Mail* above referred to, together with his reply. The article in question bears the title, "Protection by Mimicry—a Problem in Mathematical Zoology." The authors, while admitting the broad principles involved in Dr. Fritz Müller's theory, fail to see why the advantage derived by the mimicking species, in cases where the latter is less numerous than the model, should be as the square of the relative numbers. They admit that "the ingenious explanation seems perfectly satisfactory," but the proportional benefit appeared to them exaggerated. Mr. Wallace has now, I think, cleared up the misunderstanding with reference to this part of the question, but it may be of use in assisting towards the further discussion of the problem if I here give the simple algebraical treatment adopted in the original paper.

Let a_1 and a_2 be the numbers of two distasteful species of butterflies in some definite district during one summer, and let n be the number of individuals of a distinct species which are destroyed in the course of a summer before its distastefulness is generally known. If both species are totally dissimilar, then each loses n individuals. If, however, they are undistinguishably similar, then the first loses $\frac{a_1 n}{a_1 + a_2}$ and the second loses $\frac{a_2 n}{a_1 + a_2}$. The absolute gain by the resemblance is therefore for the first species, $n - \frac{a_1 n}{a_1 + a_2} = \frac{a_2 n}{a_1 + a_2}$; and in a similar manner for the second species, $\frac{a_1 n}{a_1 + a_2}$. This absolute gain, compared with the total numbers of the species, gives for the first (A_1), $\frac{a_2 n}{a_1(a_1 + a_2)}$, and for the second ($A_2$) $\frac{a_1 n}{a_2(a_1 + a_2)}$. We thus have the proportion, $A_1 : A_2 = a_2^2 : a_1^2$.

With reference to Mr. Wallace's concluding paragraph, I may point out that the advantage of the mimic is "measured solely by the fraction of *its own members* saved from destruction." Thus, taking his last example, the species *c* saves only 1/1000 of its whole number, and *d* saves 1/10 of its whole number by the resemblance to *c*. The fact that these numbers stand to one another in the ratio of $1 : 10^2$, whilst $c : d = 10 : 1$, is a mathematical necessity from which I do not see how we can escape. As the numerical disproportion betwen the species increases, the advantage derived by the more abundant insect is practically a vanishing quantity; whilst, on the other hand, if the two species are equal in numbers, it is obvious that they both derive the same advantage, each losing only half the number that it would if there was no resemblance between them.

It must not be forgotten in considering the question of mimicry between two nauseous species that the foregoing calculations apply only to the case where the resemblance is perfect, *i.e.* so exact that the insects are absolutely undistinguishable by their foes. The initial steps may be hastened in these cases by the near blood-relationship of the species, and it is a remarkable circumstance that large numbers of species belonging to different distasteful genera have a close similarity of wing-pattern, although the distinctness of the genera has never been called in question. But the genera concerned, although distinct, are very closely related; and this is quite in accordance with the views here advocated.

The general question as to the persecution of distasteful butterflies by young inexperienced birds, &c., is certainly one on which much work remains to be done, and very great service could be rendered if naturalists residing in the tropics would undertake some systematic experiments in this direction. My friend, Mr. W. L. Distant, the author of the "Rhopalocera Malayana," has already given reasons in these columns (vol. xxvi. p. 105) for disbelieving in any such want of experience, and I have discussed this phase of the question with him elsewhere (*Ann. and Mag. Nat. Hist.*, December, 1882).

R. MELDOLA

On the Value of the "Neoarctic" as One of the Primary Zoological Regions

IN the *Proceedings of the Academy of Natural Sciences of Philadelphia* (December, 1882) Prof. Angelo Heilprin has an article under the above title, in which he seeks to show that the Neoarctic and Palæarctic should form one region, for which he proposes the somewhat awkward name "Triarctic Region," or the region of the three northern continents. The reasons for this proposal are, that in the chief vertebrate classes the proportion of peculiar forms is less in both the Neoarctic and Palæarctic than in any of the other regions; while, if these two regions are combined, they will, together, have an amount of peculiarity greater than some of the tropical regions.

This may be quite true without leading to the conclusion argued for. The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups, and geographical individuality. The absence of the families of hedgehogs, swine, and dormice, and of the genera *Meles, Equus, Bos, Gazella, Mus, Cricetus, Meriones, Dipus,* and *Hystrix,* among mammals; and of the important families of flycatchers and starlings, the extreme rarity of larks, the scarcity of warblers, and the absence of such widespread genera as *Acrocephalus, Hypolais, Ruticilla, Saxicola, Accentor, Garrulus, Fringilla, Emberiza, Motacilla, Yunx, Cuculus, Caprimulgus, Perdix, Coturnix,* and all the true pheasants, among birds, many of which are groups which may almost be said to characterise the Old World as compared with the New, must surely be allowed to have great weight in determining this question.

The geographical individuality of the two regions is of no less importance, and if we once quit these well-marked and most natural primary divisions we shall, I believe, open up questions as regards the remaining regions which it will not be easy to set at rest. There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions. But even after these two are united, there will remain discrepancies of almost equal amount among the rest, since in some groups the Neotropical, in others the Australian, far exceed all other regions in their speciality. The temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate, and have thus not been able to preserve so many ancient and specialised forms as the more uniformly warm areas. But, taking this fact into account, it seems to me that the individuality of the Neoarctic and Palæarctic regions is very well marked, and much greater than could have been anticipated; and I do not think that naturalists in general will be induced to give them up by any such arguments as are here brought forward.

ALFRED R. WALLACE

A Remarkable Phenomenon.—Natural Snowballs

I TAKE the liberty of inclosing a copy of an account of natural snowballs which I furnished to the *Courant* newspaper in this place. It may be well to state that the distance from Long Island Sound to Massachusetts is some seventy miles, and that the Connecticut Valley Railroad is about fifty miles long, and runs close to the bank of the Connecticut River for some forty miles; the rolls of snow on the frozen river are said to have been very large and handsome.

SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

On Tuesday evening a light but damp snow fell upon the crust that had formed over the snow of Sunday's storm; and the south wind, which arose at a later hour, produced an unusual phenomenon. Wednesday morning the college campus, the park, and vacant lots everywhere hereabouts were seen to be strewn with natural snowballs, some of them resembling spheres with diameters of from one to nine inches or more, and others looking very much like rolls of light cotton batting, having a cylindrical shape, but in nearly every case with a conical depression at each end reaching nearly or quite to the middle. It was easy to see how the balls had been formed, as it is easy to see how boys roll up the snow for their forts. The wind had in each case started a small pellet of the moist snow, and it had rolled along until it grew so large that the wind could move it no further. The ball not only increased in diameter as it rolled, but also grew gradually in length as a little more of the snow stuck to it on each side, and thus the snow was formed into the peculiar shape described—that of a cylinder with a hollow at each end, as if a long isosceles triangle were rolled up, beginning at its vertex. The largest of the cylinders measured on the college campus had a diameter of twelve inches and a length of eighteen inches, while others in the fields in the neighbourhood seemed much larger. The path of the balls could in many cases be readily traced for a distance of twenty-five or thirty feet. The snow, it should be added, was not at all closely packed, but lay together very lightly and yielded to a slight touch, so that it was impossible to move a ball without breaking it.

Observers in other parts of the city report that some balls were seen of the size of a barrel which left tracks behind them for more than sixty feet. From East Hartford it is reported that they studded the fields thickly, especially in places where the wind had a long range, and were of every size to that of a half bushel or larger. Similar balls were seen yesterday morning in many places from the Sound north to Massachusetts. All along the line of the Valley Railroad they appeared on every rod of ground, and at some places they had left tracks showing that the wind had blown them in every direction, even in some cases up hill.

This interesting phenomenon, though quite unusual, has been noticed before in different places in this country and elsewhere, the most striking instance on record being one which was observed in New Jersey in 1808; this was in the daytime, when the whole process could be watched. On this occasion some of the masses of snow which were rolled up by the wind attained a diameter of three feet. They appear to have been seen, however, over an area of only some four hundred acres, whereas the snowballs yesterday were spread thickly over many square miles.

[We have received a communication on the same subject from Prof. Brocklesby of Hartford.—ED.]

The Late Transit of Venus

I AM told that, in referring to the observations on the late transit of Venus which were made from a station on our college grounds by the astronomers of the German Imperial Commission, you speak of them as using the photographic process. This is not correct; besides contact observations they restricted themselves to the use of the heliometer. The first and the second contacts were not seen by reason of clouds; but four half sets and six full sets of heliometric measurements were made—128 in all. The third and the fourth contacts were observed by the German astronomers and by myself.

SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

Rankine's "Rules and Tables"

I DO not know upon what authority your reviewer of Rankine's "Rules and Tables" bases his dictum that the r in the rule for the extension or compression of a spiral spring should be to the second power instead of to the third power. Prof. Rankine's view was that it should be r^3. I would refer your reviewer to vol. xviii. of the *Transactions of the Institution of Engineers and Shipbuilders in Scotland*, where he will find, amongst other results of an experimental committee's investigations upon the important question of the loading of safety-valves by such springs, that the *third* power of the radius or diameter of the spring is also used.

W. J. MILLAR

Glasgow, March 10

[The formula given by Mr. Millar is, the writer of the notice informs us, perfectly correct, and the error is his.—ED.]

Meteors

ABOUT five minutes past seven this evening I saw the most beautiful "*shooting star*" I have ever witnessed. It was moving from east to west directly over this town, and disappeared at an apparent distance of ten or twelve miles, after traversing an arc of about 75° as I saw it. It was visible whilst one might count ten or twelve at the usual rate of speaking. In its course it not only left a most unusually long train of light behind, but whole pieces kept *dropping*. What appeared is thus best described. These pieces followed the original for a space, leaving perceptible lines of light. Probably ten or a dozen such pieces were broken off during the time I was looking. Some idea of it may be gathered from the fact that for a time I thought it was a rocket. The light was remarkably white, the brilliance much above that of Venus at any time, and its rate of motion slow. The most remarkable feature, however, was the continuous breaking away of pieces, which left in turn visible trains of light.

THOMAS MASHEDER

The Grammar School, Ashby-de-la-Zouch, March 17

IN NATURE, vol. xxvii. p. 434, reading somewhat hastily, I took the brilliant meteor there mentioned to be one I myself saw. Reading more carefully, however, in last week's issue, I see that both day and hour and direction differ. On March 4, about 8.45 p.m., a very large and bright meteor passed at a low altitude from south to north. It was of a greater apparent size than Venus, quite as bright, but with a greener light. The motion was slow, no train; it only became incandescent during a short part of its transit, and passing behind the roofs of some houses was immediately lost to sight. HENRY CECIL

Bregner, Bournemouth, March 20

P.S.—If a line be drawn north and south, the meteor became visible at a point due east, which direction I was facing.

THE BRITISH CIRCUMPOLAR EXPEDITION[1]

THE journey to Fort Rae, though long, was full of interest and variety. Our party, consisting of myself, two sergeants, and an artificer, of the Royal Artillery, left Winnipeg on June 9 by steamer for Fort Carlton, on the Saskatchewan, *viâ* Lake Winnipeg. We were detained a day in that lake by ice, but reached the mouth of the Saskatchewan on the 13th, where we were delayed four days trans-shipping cargo to the river steamer, which lay three miles off at the upper end of the rapids; a tedious voyage of eight days took us to Carlton, a stockaded port on the south bank of the river. For the first three days the country seemed one immense swamp, with numerous shallow lakes; then the banks gradually grow higher, till at "the Forks" (the confluence of the north and south branches of the Saskatchewan) they are about 150 feet above the river. Here the soil seems very rich and fertile, and about the new settlement of Prince Albert, a day higher up, the country is quite English in appearance—undulating, covered with rich grass, with woods here and there—a far more attractive-looking country than the flat, treeless prairie near Winnipeg.

From Carlton, after a day or two spent in hiring transport carts, we started on the 30th with a train of ten carts, containing our provisions and baggage. The country was very pretty, well wooded and watered, with duck, snipe, and prairie chicken in abundance; it was at times difficult to believe one was not in an English park. But the most vivid imagination cannot picture the swarms of mosquitoes that at times attacked us: they came against our faces like flakes in a heavy snowstorm, and though we found our veils and gloves a good protection whilst travelling, yet, when mealtimes came, veils had to be laid aside, and the wretched insects seized the opportunity of taking their meal too.

On the third day of our journey, on reaching the crest of some rising ground, an extended view opened before us, ridge behind ridge, a sombre sea of pinewood stretching away in the distance. It was the great sub-Arctic forest which extends northwards to the barren grounds at the Arctic circle and east and west to the Atlantic and the Pacific. On entering the woods the mosquitoes were not quite so bad, but our unfortunate animals became the prey of an enormous horsefly, which settled on them in thousands, biting them till they were streaming with blood. Fortunately they only came out during the heat of the day, and we were sometimes obliged to make a halt and light fires so that the animals might stand in the smoke, which they were very willing to do; indeed they often put a newly-lighted fire out by rolling in it.

The road through the woods was very bad, and breakdowns were numerous, but at last on July 9 we reached Green Lake, which we left by boat on the 11th for Ile à la Crosse. Our conveyance was now one of the Hudson Bay Company's inland boats, with a crew of eight Indians. As we had the stream with us, we were able to drift all night, only landing when we required to cook; so we reached Ile à la Crosse early on the 14th.

We left it the same evening with a crew of eight Chipewyans, the best crew we ever had. I think they must have pulled sixty miles on one day, the day after we left the fort. On the evening of that day we had an aurora shortly after sunset, which is unusually early in the evening for one. This one appeared to be remarkably close, from its rapid motion and from its being between us and a cirro-cumulus cloud. It was accompanied by a distinct swishing noise like the sound of a sharp squall in a ship's rigging, or the noise a whip makes in passing through the air. I have not heard it since, though there have been plenty of auroras, but from what I have been told by those who have passed their lives in the country, I am of opinion that this sound is occasionally, though rarely, heard, and that it would be heard oftener were it not that the aurora is generally at too great a height.

Two more days brought us to Portage la Loche, a track of some fourteen miles across the watershed dividing the basin of the Arctic Ocean from that of Hudson's Bay. It is fairly level till the last mile, when the edge of the valley of the Clearwater River is reached, some 600 feet above the stream. From this point the view of the valley is very fine, and it strikes one the more from the monotonous nature of the scenery hitherto. The river flows between two ranges of hills, from 800 to 1000 feet in height, and here and there in rapids between limestone cliffs. The first "portage" (where the boats have to be hauled some distance overland) is particularly picturesque, but the whole valley abounds in bits that would delight an artist's eye.

On July 28 we reached the Athabasca River, a fine stream, half a mile or more in width, and the strong current, aided by a fair wind, took us to the lake in a couple of days. There are several springs of naphtha and one of sulphur on the banks.

On crossing Lake Athabasca to Fort Chipewyan, there is a complete change in the character of the country. On the south side the banks are nearly level with the water, all reeds and mud; on the north side is a savage wilderness of Laurentian rock. From a hill at the back of the fort is an extensive view of this strange and desolate country. To the west the lake stretches away to the horizon; on the other side is a mixture of lake, island, and river, and to the north the land, a wilderness of rock in low rounded hills, with a few stunted pines in the valleys, all pretty enough, but so lonely looking.

We were detained a fortnight at Fort Chipewyan till the arrival of the Mackenzie River boats. The heat was at times extreme—as much as 90° in the house.

The Slave River, or Mackenzie, as it really is, is a magnificent river, especially after its junction with the Peace River, which is at least as big as the Athabasca. The united stream is often a mile in width. About half way to Slave Lake are the rapids, where the scenery is very fine. There are four portages, over three of which the boats had to be hauled, so it was two days' work getting through them. We had a sharp frost on the morning of the 19th, the buckets, &c., that were left with water in them had a quarter of an inch of ice on them in the morning.

On the next evening, while running down the rapid to the last portage, the "Portage des Noyés," after sunset, a bright parhelion made its appearance, some 10° or 12° above the horizon. It was of a bright red colour, and threw a brilliant reflection in the water, remaining visible for about twenty minutes, when it changed into a crimson column, that gradually died away.

On August 22 we reached Fort Resolution—a wretched-looking place on a flat muddy coast—and the same evening we left for Fort Rae. At sunset the pilot of the boat insisted on stopping for the night at a small rocky island at the mouth of the Slave River. I thought it a pity to stop as we had a fair wind, but the natives of the country have a great dread of lakes, and certainly Great Slave Lake is a stormy place. At midnight a heavy swell suddenly arose, and our boat was stove in and sunk in a very few minutes. It was a pretty wet job to land all the baggage and stores, which of course were all saturated with water; but fortunately the instruments all escaped unhurt, and nothing was lost but a pair of boots and a couple of hats, and all our salt and most of our sugar, which the water dissolved.

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[1] Letter from Capt. Dawson, R.A., in command of the Expedition See p. 243.

For the next two days we were employed repairing the boat, it blowing a gale and raining hard the whole time, so that we could dry nothing; and when at last we started, almost constant head-winds and frequent gales made our journey a slow one. Fortunately our course lay among islands, so that we enjoyed a certain degree of shelter from the wind, and harbours of refuge were always at hand in case of necessity. These islands are all of rock and well wooded, but destitute at this season of the year of game, which was unfortunate for us, as our provisions were getting short, and our crew were reduced to a pound of flour per diem, with a little tea and sugar. There were not even fish to be caught, though they are usually abundant, but I suppose the rough weather had driven them into the deep water. At last we shot some seagulls, and we were all glad enough to eat them.

At length, on the 30th, we reached Fort Rae, which lies in lat. 62° 38′ N. and long. 115° 25′ W., half way up a long gulf that runs for about 100 miles in a north-west direction from the mouth of the Yellow Knife River. The fort is situated at the foot of a rocky hill that rises some 200 feet above the lake, which is about four miles wide at this point. The Indians who resort here for trade hunt for the most part in the "barren lands" near the Coppermine River, whence they bring quantities of skins and beef from the musk-ox, which seems to be very abundant. Deer too are very plentiful, and in the winter they migrate in great herds from the barren lands to the country between the arm of the lake on which Fort Rae lies and the Mackenzie. Sometimes these herds pass quite close to the fort, and take two or three days in passing. Their numbers must be very great; a single band has been known to kill over 15,000 in an ordinary season.

This year the deer have passed at some distance, but the Indians are now bringing in fresh meat daily.

These Indians are of the "Dog-rib" tribe—T'akfwel-ottiné, they call themselves—a quiet, inoffensive race, like all the wood-Indians. They are almost all Roman Catholics, the missionaries of that religion being very numerous in the country, and they are certainly very devoted and hard-working. There are also Protestant missionaries, but they do not appear to have made any converts.

The Dog-ribs are a branch of the Chipewyan family which occupies all that portion of the continent between the Rocky Mountains and Hudson's Bay to the north of the parallel of 55°. They are unprepossessing in appearance, and their language is almost unpronounceable by a European. Their alphabet, if they had one, would contain no less than seventy-one letters, that being the number of distinct sounds. I believe the language is allied to the ancient Mexican—at any rate the Navajo is the nearest to it of existing languages—and the combinations of letters that one sees in Mexican names (*tl*, for instance) are common in this language. The Dog-ribs have the remarkable peculiarity of a national habit of stammering, which is most marked in those who seldom come in to the fort. They treat their women with more kindness than is usual among the American Indians.

Fort Rae is one of the windiest and cloudiest places I have ever seen, but I am told this is an exceptional year. It is certainly a very late autumn; the lake was not frozen till November 1, and it is only within the last day or two that the cold weather has really set in. Last night the thermometer was at − 34°.

My space is at an end, but by the next mail I hope to give you an account of our winter here.

Fort Rae, December 1

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT[1]

IV.

A CONDITION very nearly similar to that caused by atropia is produced by morphia. When this substance is given to a frog its effects are exactly similar to those produced by the successive removal of the different parts of the nervous system from above downwards. Goltz has shown that when the cerebral lobes are removed from the frog it loses the power of voluntary motion and sits still; when the optic lobes are removed it will spring when stimulated, but loses the power of directing its movements. When the cerebellum is removed it loses the power of springing at all; and when the spinal cord is destroyed reflex action is abolished.

Now these are exactly the effects produced by morphia, the frog poisoned by it first losing voluntary motion, next the power of directing its movements, next the power of springing at all, and lastly reflex action. But after reflex action is destroyed by morphia and the frog is apparently dead, a very remarkable condition appears, the general flaccidity passes away and is succeeded by a stage of excitement, a slight touch causing violent convulsions just as if the animal had been poisoned by strychnia.[2]

The action of morphia here appears to be clearly that of destroying the function of the nerve centres from above downwards, causing paralysis first of the cerebral lobes, next of the optic lobes, next of the cerebellum, and next of the cord. But it seems probable that the paralysis of the cord first observed is only apparent and not real, and in order to explain it on the ordinary hypothesis we must assume that during it the inhibitory centres in the cord are intensely excited so as to prevent any motor action, that afterwards they become completely paralysed, and thus we get convulsions occurring from slight stimuli.

On the hypothesis of interference, the phenomena produced both by atropia and by morphia can be more simply explained. These drugs, acting on the nervous structures, gradually lessen the functional activity both of cells and of fibres; the impulses are retarded, and thus the length of nervous connection between the cells of the spinal cord, which is calculated to keep them in proper relation in the normal animal, just suffices at a certain stage to throw the impulses half a wave-length behind the other, and thus to cause complete inhibition and apparent paralysis.

As the action of the drug goes on, the retardation becomes still greater, and then the impulses are thrown very nearly, but not quite, a whole wave-length behind the other, and thus they coincide for a short time, but gradually again interfere, and therefore we get on the application of a stimulus, a tonic convulsion followed by several clonic ones, and then by a period of rest. This explanation is further borne out by the fact observed by Fraser, that the convulsions caused by atropia occurred more readily during winter, when the temperature of the laboratory is low and the cold would tend to aid the action of the drug in retarding the transmission of impulses.[3]

The effect of strychnia in causing tetanus is very remarkable; a very small dose of it administered to a frog first renders the animal most sensitive to reflex impulses, so that slight impressions which would normally have no effect, produce reflex action. As the poisoning proceeds, a slight stimulus no longer produces a reflex action limited to a few muscles, but causes a general convulsion throughout all the body, all muscles being apparently put equally on the stretch. In man the form assumed by the body is that of a bow, the head and the heels being bent backwards, the hands clenched, and the arms tightly drawn to the body.

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[1] Continued from p. 468.

[2] Marshall Hall, Memoirs on the Nervous System, p vii (London, 1837) Witkowski Archiv fur exper Path. und Pharm, Band vii. p 247.

[3] Transactions of the Royal Society of Edinburgh, vol. xxv p. 467.

My friend Dr. Ferrier has shown that this position is due to the different strengths of the various muscles in the body. All being contracted to their utmost, the stronger overpower the weaker, and thus the powerful extensors of the back, and muscles of the thighs keep the body arched backwards and the legs rigid, while the adductors and flexors of the arms and fingers clench the fist and bend the arms, and draw them close to the body.[1] The convulsions are not continuous, but are clonic; a violent convulsion coming on and lasting for a while, and then being succeeded by an interval of rest, to which after a little while another convulsion succeeds. The animal generally dies either of asphyxia during a convulsion, or of stoppage of the heart during the interval.

When the animal is left to itself the convulsions—at least in frogs—appear to me to follow a certain rhythm, the intervals remaining for some little time of nearly the same extent.

A slight external stimulus, however, applied during the interval—or at least during a certain part of it—will bring on the convulsion. But this is not the case during the whole interval. Immediately after each convulsion has ceased I have observed a period in which stimulation applied to the surface appears to have no effect whatever.

It is rather extraordinary also, that although touching the surface produces convulsions, irritation of the skin by acid does not do so.[2]

The cause of those convulsions was located in the spinal cord by Magendie in an elaborate series of experiments.

Other observers have tried to discover whether any change in the peripheral nerves also took part in causing convulsion; but from further experiments it appears that the irritability of the sensory nerves is not increased.[3]

According to Rosenthal, strychnia does not affect the rate at which impulses are transmitted in peripheral nerves; according to him, however, it lessens the time required for reflex actions. Wundt came to the conclusion that the reflex time was on the contrary increased.

In trying to explain the phenomenon of strychnia tetanus on the hypothesis of interference, one would have been inclined by Rosenthal's experiments to say that strychnia quickened the transmission of impulses along those fibres in the spinal cord which connect the different cells together.

The impulses which normally, by travelling further round fell behind the simple motor ones by half a wave-length, and thus inhibited them, would now fall only a small fraction of a wave-length behind, and we should have stimulation instead of inhibition.

Wundt's results, on the other hand, would lead to the same result by supposing that the inhibitory wave was retarded so as to fall a whole wave-length behind the motor one. On the assumption, however, that the fibres which pass transversely across from sensory to motor cells, and those that pass upwards and downwards in the cord connecting the cells of successive strata in it, are equally affected, we do not get a satisfactory explanation of the rhythmical nature of the convulsions. By supposing, however, that these are not equally affected, but that the resistance in one—let us say, that in the longitudinal fibres—is more increased than in the transverse fibres we shall get the impulses at one time thrown completely upon each other causing intense convulsion, at another half a wave-length behind, causing complete relaxation, which is exactly what we find.

This view is to some extent borne out by the different effect produced by a constant current upon these convulsions, according as it is passed transversely or longitudinally through the spinal cord. Ranke found that when passed transversely, it has no effect, but when passed longitudinally in either direction, it completely arrests the strychnia convulsions, and also the normal reflexes which are produced by tactile stimuli.

Ranke's observations have been repeated by others with varying result, and this variation may, I think, be explained by the effect of temperature.

Near the beginning of this paper I mentioned that the touchstone of the truth or falsehood of the hypothesis of inhibition by interference was to be found in the results of quickening or slowing the rate of transmission of stimuli.

Heat and cold are the two agents regarding whose action in this respect we have the most trustworthy experimental data. In peripheral nerves, heat up to a certain point quickens the transmission of stimuli, and cold retards it. In the spinal cord warmth increases the excitability, and at a temperature of 29 to 30 may of itself cause tetanus.[1] Cold also beyond a certain temperature increases the reflex excitability.

The effect of warmth and cold upon strychnia tetanus is what we would expect on the hypothesis of interference. With small doses of strychnia warmth abolishes the convulsions, while cold increases them. When large doses are given, on the contrary, warmth increases the convulsions, and cold abolishes them.[2]

We may explain this result on the hypothesis of interference in the following manner:—

If a small dose of strychnia retard the transmission of nervous impulses so that the inhibitory wave is allowed to fall rather more than half a wave-length, but not a whole wave-length, behind the stimulant wave, we should have a certain amount of stimulation instead of inhibition. Slight warmth, by quickening the transmission of impulses, should counteract this effect, and should remove the effect of the strychnia. Cold, on the other hand, by causing still further retardation, should increase the effect. With a large dose of strychnia, the transmission of the inhibitory wave being still further retarded, the warmth would be sufficient to make the two waves coincide, while the cold would throw back the inhibitory wave a whole wave-length, and thus again abolish the convulsions.

The effect of temperature on the poisonous action of guanidine is also very extraordinary, and is very hard to explain by the ordinary hypotheses, although the phenomena seem quite natural when we look at them as cases of interference due to alterations in the rapidity with which the stimuli are transmitted along nervous structures. Guanidine produces, in frogs poisoned by it, fibrillary twitchings of the muscles, which are well marked at medium temperatures, but are abolished by extremes of heat and cold. Thus Luchsinger has found that, when four frogs are poisoned by this substance, and one is placed in ice-water, another in water at 18° C., a third in water at 25° C., and a fourth in water at 32° C., the fibrillary twitchings soon disappear from the frog at 0° C., and only return when its temperature is raised to about 18° C. In the frog at 18° C. convulsions occur, which are still greater in the one at 25° C. In the frog at 32° C., on the other hand, no trace of convulsions is to be seen; the animal appears perfectly well, and five times the dose of the poison, which at ordinary temperatures would convulse it, may be given to it without doing it any harm, so long as it remains in the warmth,[3] although when it is cooled down the effect of the poison at once appears.

Another cause of tetanus that is difficult to understand on the ordinary hypothesis of inhibitory centres is the similar effect of absence of oxygen and excess of oxygen. When an animal is confined in a closed chamber, without oxygen it dies of convulsions; when oxygen is gradually introduced before the convulsions become too marked, it recovers. But when the pressure of oxygen is gradually raised above the normal, the animal again dies of convulsions. This is evidently not the effect of mere increase in atmospheric pressure, but the effect of the oxygen on the animal, inasmuch as 25 atmospheres of common air are required to produce the oxygen convulsions, while 3 atmospheres of pure oxygen are sufficient. This effect is readily explained on the hypothesis of interference by supposing that the absence of oxygen retards the transmission of impulses in the nerve-centres; so that we get those which ought ordinarily to inhibit one another, coinciding and causing convulsions. Increased supply of oxygen gradually quickens the transmission of impulses until the waves first reach the normal relation, and then the normal rate being exceeded, the impulses once more nearly coincide, and convulsions are produced a second time.

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[1] Brain, vol iv. p. 313.

[2] Eckbard, Hermann's Handb. d Physiol. Band ii. Tb. 2, p. 43.

[3] Bernstein quoted by Eckhard, *op. cit.* p 40. Walton, Ludwigs rbeiten, 1882.

[1] Cayrade, Recherches critiques et exper. sur les Mouvements Reflexes, p. 48.

[2] Kunde and Virchow quoted by Eckhard, *op. cit.* p. 44; Foster, Journal of Anatomy and Physiology, November 1873, p. 45.

[3] Luchsinger, Physiologische Studien, Leipzig, 1882, p 44.

In discussing the action of the nervous system we have hitherto taken into account only that of the nerve fibrils, and left out of the question the nerve cells. We have assumed that the waves arrived in the reservoir (in our diagram) from a distance, and were simply transmitted along channels, but in the nervous system we have to take into account the origination of the waves in the nerve cells themselves, as well as their propagation along the nerve channels.

There is a great difference between the function of the nerve cell and of the nerve fibre analogous to that which exists between the cell and the wire in a galvanic battery. The particular form of energy which we met with in both cases originates in the cell and is transmitted along the fibre or the wire. In both cases also the energy appears to originate from chemical changes going on in the cell. Material waste of some sort goes on in both, and in both the products of this waste if allowed to accumulate will by and by arrest the action.

We find an indication of the difference between the amount of chemical change which goes on in the nerve cell and in the nerve fibre in the amount of blood supplied to each respectively. The nerve cells are abundantly supplied with blood, and the nerve fibres very sparingly so. The free supply of blood secures to the nerve cells both the supply of fresh material and ready removal of waste products.

Perhaps the best illustration that we can find in physics of the processes which take place in the nervous and muscular systems is however afforded by singing flames in which the sounds and movements are produced by very numerous small explosions: for both in the nervous and muscular systems the tissue change appears to go on as a series of small explosions. The material which yields nervous and muscular energy undergoes oxidation, but the oxygen concerned in the process is not derived directly from the external air. Substances which yield oxygen are contained within the tissues themselves just as nitre is contained along with oxidisable substances in a charge of gunpowder.

In this paper also we have spoken of waves of nervous interference as if they were simple, but it is much more probable that they are very complex, resembling much more the beats of sound produced by two singing flames which are not in unison, than simple waves of water.

The number of nervous discharges which issue from the motor cells of the spinal cord during tetanus and set the muscles in action is, according to Dr. Burdon Sanderson, about 16 per second, but in all probability each of these impulses consists of a large number of small vibrations. In rhythmical actions, such as that of the respiration, we have probably at the very least three rhythms, 1st, exceedingly rapid vibrations in the nervous cells; 2nd, slower vibrations or beats from 16 or 18 per second, which issue from them and excite the muscles to action; and 3rd, a still slower rhythm, of 16 per minute, probably due to interference between groups of cells, which leads to inspiratory movements alternating with rest or with active exspiration. The consideration of these complicated phenomena would, however, at present lead us too far, and they as well as the subject of nervous interference in the heart and rhythmic contraction of muscles, must be reserved for another time.

In this paper I must be content with the attempt to show that inhibition and stimulation in the nervous system are not dependent on special inhibitory or stimulating centres, but are merely relative conditions depending on the length of path along which the stimulus has to travel and the rate of its transmission. The test of the truth or falsehood of this hypothesis is to be found in the effect of alteration in the rapidity of nervous transmission upon inhibitory phenomena. The application of this test appears, so far as our present data go, to support this hypothesis. T. LAUDER BRUNTON

BEN NEVIS OBSERVATORY

IN NATURE, vol. xxvii. p. 39, I gave a brief notice that on November 1—owing to stress of weather forbidding the regular daily ascents of Ben Nevis—I was obliged to discontinue the daily work of the meteorological observing system on the summit and slopes of the mountain. This was in simultaneous connection with my system of observations near the sea-level at Achintore, Fort William. As in the previous summer, I had the honour to organise and carry on the work under the auspices of the Scottish Meteorological Society. The experience gained in 1881, when I first commenced observing on the Ben, enabled me to draw up and submit to the Society a more elaborate plan of mountain observation for the summer and autumn of 1882; and as I have been fortunate enough to carry it through for five months without any hitch, and as I am not aware that anything of the kind had, previous to my first undertaking, been attempted, I am naturally anxious that NATURE should have a more complete account of my last year's operations. My plan was to have *fixed* stations at different altitudes between the main observatories at the base and on the summit of the mountain, so placed in fact that I could observe *regularly* at half-hourly intervals during the daily ascent and descent of the Ben; to extend the number of summit observations to five sets; and to have in every case simultaneous observations taken at the sea-level station—my grand base of operations. All this was with a view to localising disturbances existing in the stratum of atmosphere between the sea-level and the top of Ben Nevis, to furthering meteorological research generally, and so ultimately to gain forecasting material. I arrived at Fort William from Edinburgh on May 25, and at once proceeded to give effect to my plans. During the next few days I was engaged mainly in erecting Stevenson's thermometer screens, and laying out the sea-level station; in establishing a new "midway" observatory at the lake, erecting screen, and building there a granite cairn for a barometer; and in reopening the temporary observatory on the summit of the mountain. It was only by dint of great exertion and a gang of men that I got all in order on the top of the Ben on May 31. I had no occasion, however, to alter the arrangements of the previous summer; and the heavy work of reopening chiefly consisted in digging out from the vast accumulations of snow the barometer cairn, hut, and thermometer cage which here, as a safeguard, incloses Stevenson's screen. The snow, in fact, was nearly four feet deep, and it was necessary to cut out wide areas around the instruments. I also erected another screen to contain Negretti and Zambra's self-registering clock-hygrometer, most kindly placed at my disposal by that eminent firm for the purpose of obtaining 9 p.m. values. I had also to fix a new roof of ship's canvas to the rude shanty that affords some little shelter from the piercing cold and storms. The barometer, a fine Fortin, had been left in its cairn built up during the past winter; and great labour was expended before the north side of the cairn was reopened, the stones being so hard frozen that a crowbar had to be employed. The instrument was found in good condition.

Passing over all other details of arranging the stations and fixing instruments, I may say that I had all in order and commenced work on June 1. I now give a list of the stations, with positions, hours, and elements of observation.[1] The distances in the text are given in right lines from the sea-level station. Fig. 1 at once shows the bearings, and distances by the actual track followed. Fig. 2 is a longitudinal section giving total actual distances.

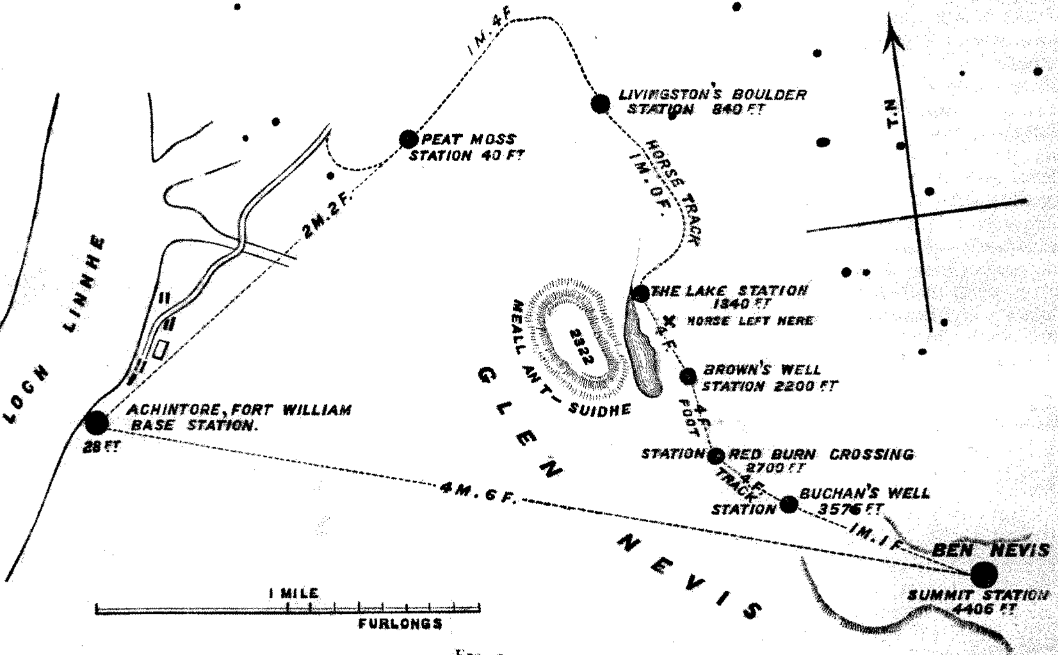

FIG. 1.

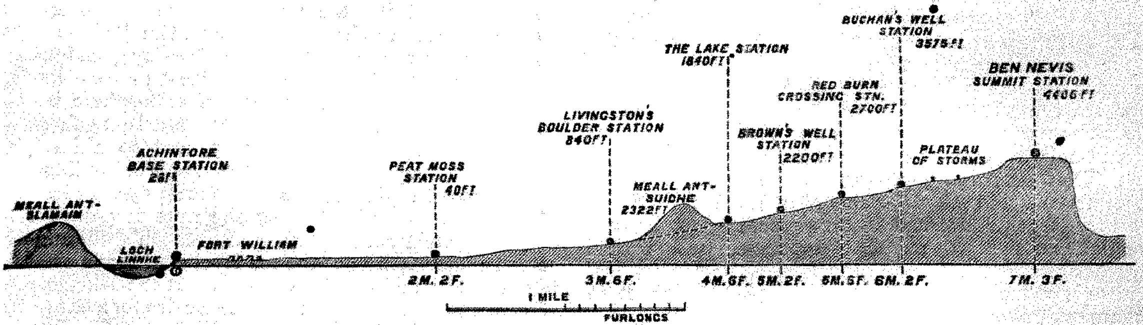

FIG. 2.

ACHINTORE, FORT WILLIAM, BASE OR SEA-LEVEL OBSERVATORY.—*Position:* About 28 feet above sea, on a level sward, perfectly open on all sides, running parallel and immediately adjacent to Loch Linnhe; soil, gravelly loam.

Hours.—5, 5.30, 6.15, 7, 7.30, 7.55, 8.30, 9, 9.30, 10, 10.30, 11, and 11.30 a.m.; and noon, 0.30, 1, 1.45, 2.30, 3, 6, and 9 p.m.

Elements.—Atmospheric pressure by mercurial barometer, temperature of air and evaporation (dry and wet bulbs), direction and force of wind, kind and amount of cloud, and movements and velocities of the various strata of cloud, hydrometeors and remarks in full detail at all the above times. Maximum and minimum shade temperature, solar maximum and terrestrial minimum temperature, earth temperature (1 and 2 feet), and rainfall at 9 a.m. and 9 p.m. Temperature of Achintore well, and subsequently of Loch Linnhe between 9 and 11 a.m.

Ozone for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m.; also for periods of 24 and 12 hours, ending 9 a.m. and 9 p.m. Ozone also for the following periods of exposure:—6 hours ending 1 p.m., and 18 hours ending 7 a.m., and subsequently in addition for 15 hours ending 5.30 a.m., and 9 hours ending 2.30 p.m. [It will be seen later that all these ozone observations (except those for 12 hours ending 9 o'clock) were simultaneous with others on the summit of Ben Nevis, at the Lake, and Peat Moss stations.]

Actinism of the sun's rays and of daylight by Dr. Angus Smith's apparatus for 24 hours ending 10.17 a.m.; comparison-pressure by aneroid at 5 a.m. and 3 p.m. on leaving for and returning from the summit and slopes' stations.

PEAT MOSS.—*Position:* About 40 feet above sea; 2m. 2f.; perfectly open; near the middle of the extensive moss at the foot of *Meall an t-Suidhe;* peaty, swampy soil, with hummocks around.

Hours.—5.30 to 6 a.m. (this was the only hour in the entire system that varied, and extra simultaneous readings were taken at Achintore whenever this was the case), and 2.30 p.m. From August 1 also at 9, 9.30, 10, 10.30, and 11 a.m.

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[1] Cloud movements and velocities were not, however, noted absolutely *every* time.

Elements.—Temperature of air and evaporation (dry and wet bulbs), wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before at all the above times. Pressure by aneroid, 5.30 to 6 a.m., and at 2.30 p.m. Rainfall at 9 a.m. Ozone for 15 hours, ending about 5.30 a.m., and for about 9 hours, ending 2.30 p.m.; also for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m. (simultaneously with the summit and base stations). Temperature of adjacent water-hole subsequently about 5.30 a.m. and 2.30 p.m.

PEAT MOSS CROSSING.—A minor station about 70 feet above sea, 3m. 0f., situated at the burn *Allt Coire an Lochain.*

Hours and Elements.—About 5.50 a.m. and 2.17 p.m.: pressure by aneroid, and temperature of burn.

LIVINGSTON'S BOULDER.[1]—*Position:* 840 feet above sea; 3m. 1f.; close to the burn *Allt Coire an Lochain*, on a level swampy patch; ground around undulating, with large boulders of coarse-grained granite lying adjacent.

Hours.—6.15 a.m. and 1.45 p.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of burn; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

THE LAKE.—*Position:* A plateau-valley 1840 feet above the sea, 3 miles, on swampy ground, fairly level, by the north-east shore of the tarn, *Lochan Meall an t-Suidhe:* granite blocks and hummocks of bog, moss, and dwarf-heather to eastward. Main slopes of Ben Nevis on east-south-east side, and *Meall an t-Suidhe* on west side.

Hours.—7 a.m. and 1 p.m.

Elements.—Pressure by mercurial barometer, com-

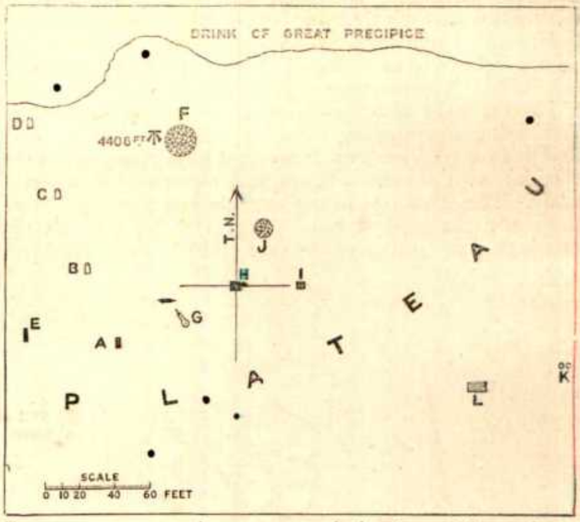
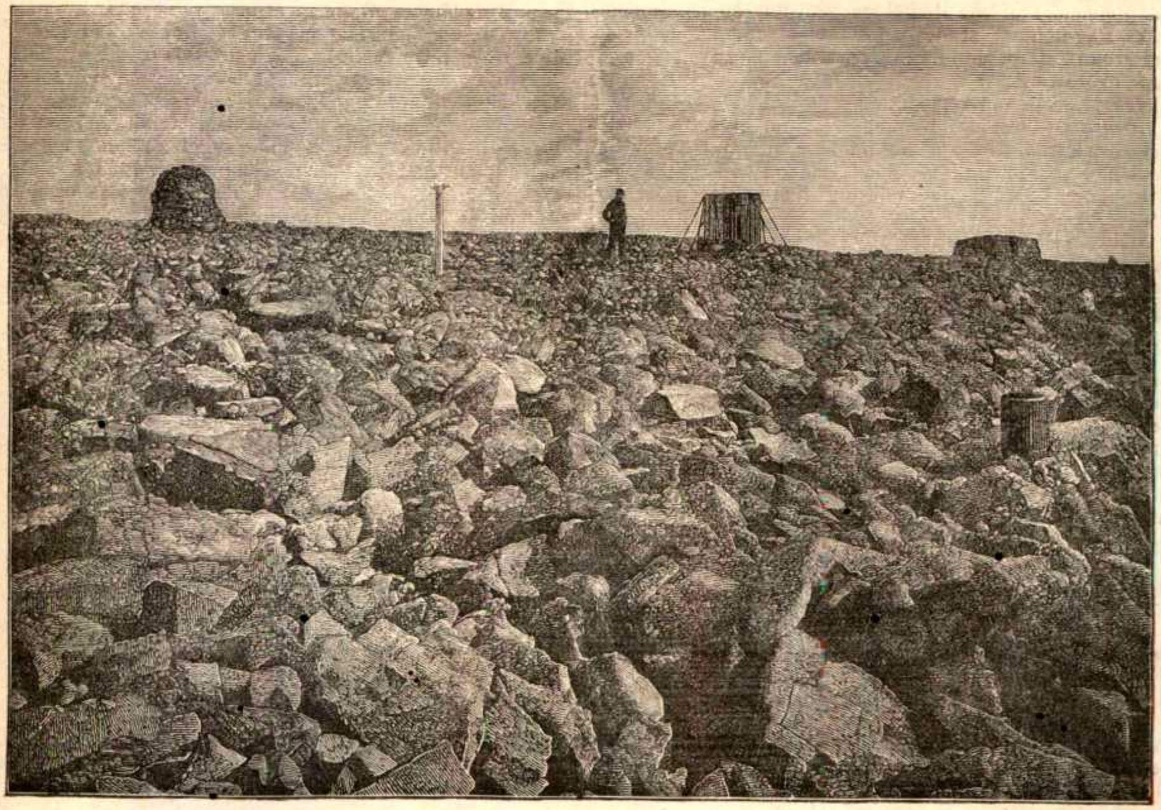


FIG. 3.—A, B, C, D, raingauges; E, notice board; F, Ordnance Survey cairn; G, solar and terrestrial radiation thermometers; H, Stevenson's thermometer cage and ozone tests; I, self-registering hygrometer; J, barometer cairn; K, earth thermometers; L, hut. The apparatus for measuring the actinism of light is near the edge of the precipice N.E. from the hut.



Barometer Cairn. Solar Radiation Thermometer. Thermometer Cage. Hut. Standard Rain-gauge.

FIG. 4.

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[1] The altitude of this station and those of the following intermediate stations, except the Lake, were determined by mean results of aneroid readings, and must be accepted accordingly.

parison-pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), maximum and minimum shade temperature, and ozone for periods of 6 and 18 hours, temperature of ground at depths of 1 and 2 feet; temperature of lake; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time. Rainfall on 1st, 8th, 15th, and 22nd of each month.

BROWN'S WELL.—*Position:* 2200 feet above sea, 3m. 1f., on a grassy patch with springs and swamps, on the main slopes of Ben Nevis. Boulders and stones of fine-grained granite graduating into felsite lie around. Slope to westward estimated at 35°.

Hours.—7.30 a.m. and 0.30 p.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of well; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

RED BURN CROSSING.—*Position:* 2700 feet above sea, 3m. 2f., above the general limit of vegetation in the deep ravine of and close beside the Red Burn; boulders and debris of porphyritic rock on all sides; slope to westward estimated at 40°.

Hours.—7.55 a.m. and noon.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of burn; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

BUCHAN'S WELL.—*Position:* 3575 feet above sea, 3m. 5f., source of the Red Burn: entirely in a region of rocks, fragmentary stones, and debris; completely open, and ground more undulating, with comparatively gentle slope to westward.

Hours.—8.30 and 11.30 a.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of well; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

BEN NEVIS, SUMMIT OBSERVATORY.—*Position:* 4406 feet above sea, 4m. 6f., in the centre of a rough rocky plateau, covered with felstone lavas and volcanic agglomerates (see Figs. 3 and 4).

Hours.—9, 9.30, 10, 10.30, and 11 a.m.

Elements.—Pressure by mercurial barometer, comparison-pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), wind and force; kind of cloud, amount, and velocities of strata; hydrometeors and remarks in fullest detail as at the sea-level and intermediate stations at all the above times.

Maximum and minimum shade temperature, solar maximum and terrestrial minimum temperature, and rainfall by four gauges at 9 a.m.

Temperature of Wragge's Well and of ground at depths of 1 and 2 feet between 9 and 11 a.m.

Ozone for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m., also by two differently exposed tests for 24 hours ending 9 a.m.

Actinism of the sun's rays and of daylight by Dr. Angus Smith's apparatus for 24 hours ending 10.17 a.m.

Hygrometric conditions prevailing about 9 o'clock the previous night by self-registering dry and wet bulbs, were noted at 10.50 a.m.

A moment's consideration, then, will show that the observations at the sea-level station were in every case simultaneous with those at the summit and intermediate stations, and that the hours at the latter were so arranged as to "mean" to the 10 a.m. readings at the base and summit of the mountain, and also at the Peat Moss.

Rainband by Browning's spectroscope was observed at various altitudes, and its indications proved of considerable value. Full notes were taken of the cloud limits, and of any important changes observed between the stations.

Of course my first business was to get the main observations—pressure, temperature, hygrometric conditions, wind, cloud, &c.—into full swing by June 1; and as I felt my way and got my hours and distances well under command I added to my work. Thus the ozone observing-system and the three extra rain-gauges on the summit were added on June 15, and the delicate operations for measuring the actinism of light on July 9. The additional gauges were established to discover if and to what extent the rainfall varies in connection with the wind at different points of the plateau from the centre to the edge of the great precipice.

During June, Stevenson's screens were in use only at the sea-level, lake, and summit; and hence at the other places the dry and wet bulbs had to be swung and the latter moistened afresh from adjacent water at each swinging. But aching wrists and sore fingers soon made me determine to have louvred screens at all the stations, and by July 1 they were in their places and dry and wet bulbs supplied by Hicks and Negretti and Zambra fixed permanently in each. So above all was accuracy the better insured, and the whole system went like clockwork. I left Achintore before 5.30 a.m., and returned about 3 p.m., and the rate of ascending and descending was so regulated as to insure punctuality usually within a few seconds—often to the second—at the various stations.

The new screens were a trifle smaller than the others. I need hardly add that the instruments at all stations were the best observing-standards procurable, and that the arrangements in every respect were those approved by the Meteorological Office and the English and Scottish Meteorological Societies. The condition of the wet bulbs, fixing of ozone tests, clamping self-registering instruments to prevent vibration in gales, levelling rain-gauges, and numerous other matters of important detail required the closest attention. The Beaufort wind and cloud scales were in use, and the ozone tests were Moffat's. Two assistants, educated by Mr. Colin Livingston of Fort William—a sufficient guarantee for their ability—and trained by myself, helped in the work; and relieved me in the ascent of the mountain three times a week, and on these occasions I took the sea-level station. One of the greatest difficulties I had to contend with in the Ben Nevis routine was as to the pony on which I rode to and from the Lake, where it was left to graze and await my descent. Occasionally the stable-boy overslept, and I had to make up for lost time,—no easy matter, as the wretched track leads over deep ruts and treacherous swamps, and the poor brute had a trying time of it. Still more frequently the person to whom it belonged gave me rotten saddlery in spite of all remonstrance; and on commencing the ascent the girth would break, and I had to turn the animal adrift and plod on to the Lake my fastest. This was decidedly hard, inasmuch as I was obliged to climb afoot some 2500 feet from the tarn in less than two hours by a circuitous route and over rough rock stopping to observe at the other intermediate stations. Again, the pony often wandered in his hobbles or having broken the tethering rope had made off to the moss; so also on the homeward journey I had sometimes to leave him and run my hardest over ruts and through swamps, by a short cut, to get my readings at the next station. Other trying parts of the work consisted in the journeys between Buchan's Well and the top in the allotted time, in having the two hours' exposure on the summit in bad weather, and in becoming chilled after profuse perspiration. The rude hut, with its walls full of holes of all shapes and angles through which the wind whistles and the snow-drifts drive, afforded but a poor shelter from the drenching rain and cold, and it was impossible to keep anything dry. My hands often became so numbed and swollen, and my paper so saturated that I had the utmost difficulty in handling keys, setting instruments and entering my observations. Usually so laden was the air with moisture and so very dense and lasting was the cloud-fog that, even when no rain had actually fallen, all the fixings and instruments were dripping; and although, of course, I made a point of wiping the dry bulb, it almost immediately became wet again. Occasionally I timed the interval between wiping and fresh condensation on the bulb, and have found it wet again within *thirty seconds*.

After November 1, then, I had to discontinue the work. The hut had become choked with snow, and the carrying on of the undertaking satisfactorily impossible. I was, however, satisfied; and very pleased that I had secured five months' observations without the break of a single day.

It is not my part to refer in this paper to any results. Such I must in duty leave to be discussed and made known by the Scottish Meteorological Society. But, from what I myself know of the meteorology of Ben Nevis from the experience of two summers and autumns, I do most strongly urge the establishment of a permanent observatory on the summit, firmly believing that most important and unexpected results will accrue to meteorology from continuous observations there. Such, in connection with others at the sea-level, would in my opinion enable the energetic staff at the Meteorological Office, under Mr. Robert H. Scott's able direction, to forecast storms with far greater certainty.

I cannot conclude this account without expressing my best acknowledgments to Dr. Angus Smith for placing at my disposal his apparatus for measuring the actinism of light, which I consider an immense acquisition to a meteorological observatory; to Mr. John Browning for his rainband spectroscope; to Messrs. Negretti and Zambra for their clock-hygrometer; and finally to the Scottish Meteorological Society for the kind encouragement and liberal assistance they have given me.

CLEMENT LINDLEY WRAGGE

HYDROGEN WHISTLES

IT may be recollected by some of the readers of NATURE that a few years ago[1] I contrived a whistle for testing the upper limits of the power of hearing very shrill notes by different men and animals. When properly made, it easily suffices to do this, in the case of men and most animals, but it cannot, neither can any other instrument hitherto devised, emit such notes as it is conceivable that insects may hear. The problem whether any insects can hear notes whose numbers of vibrations per second is manyfold greater than those of the notes audible to men has not yet been fairly put to the test of experiment. I wish to show that this can now be done.

The whistles of which I speak have their lower ends closed with a piston that admits of being inserted more or less deeply, and thus of varying the depth of the whistle and consequently its note; but as a whistle will not give its proper note unless its depth be greater than its width, say, 1½ times as much, and as the depth of a whistle that gives, say, 24,000 vibrations per second is only 0·14 inch, it follows that their bores must be very small, and that a limit of minuteness is soon reached.

Having had occasion lately to reconsider the subject, it occurred to me that I could greatly increase the shrillness of any whistle by blowing a gas through it that was lighter than common air.

The number of vibrations per second caused by whistles is inversely proportional to the specific gravity of the gas that is blown through them; therefore by the use of hydrogen, which is thirteen times lighter than air, the number of vibrations per second produced by a given whistle would be increased thirteenfold.

I have made experiments with most satisfactory results with common coal gas, whose specific gravity, though much greater than that of hydrogen, is not much more than half that of common air, and I have little doubt in consequence that a number of vibrations may be excited by one of my small-bore whistles through the use of hydrogen gas, that very largely exceeds the number attainable hitherto in any other way. They would of course fail to excite the sense of sound in any of ourselves, or perhaps to produce any physical effect that we can appreciate, whether on sensitive flames or otherwise, and the note to those creatures, if any, who could hear it, would be feebler on account of the lightness of the medium in which the vibrations originated, but it would be (so far as I can anticipate) a true note, and ought to be powerful enough to be audible at the short distances at which small creatures may be tested. The whistle I used was made for me by Hawksley, 357, Oxford Street; its bore is 0·04 inch diameter, and it gives a loud note for its size. After some prefatory trials, I proceeded as follows:—I attached the whistle to a gas jet by a short indiarubber tube. Then, without turning on the gas, I retested my range of hearing by setting the piston at various lengths and giving sharp squeezes to the tube as it lay in the hollow of my hand. The effect of each squeeze was to force a little air through the whistle, and to cause it to emit a sharp "cheep." When I relaxed the grasp, air was sucked in through the whistle, and the tube became again filled with air, ready for another squeeze.

My range of hearing proved to be such that when the depth of the whistle was 0·13 inch, I could hear no musical note at all—only a puff; at 0·14 inch I could just perceive a very faint musical note enveloped, as it were, in much puff; even at 0·20 some little puff remained, but before 0·25 the note had become purely musical. This having been established, I kept the whistle set at 0·25 and turned the gas on, giving it abundance of time to expel all air from the tube. Then, turning the stopcock to shut the indiarubber tube from behind, I gave a sharp squeeze as previously, but the whistle, instead of emitting a pure note, gave to me just the same barely perceptible sound that it did when it was set at 0·14. I relaxed my grasp and instantly retightened it, and then the whistle emitted a pure note. A little common air had regurgitated into the whistle when my grasp was relaxed, and it was the reissue of this that gave the note. I repeated the experiment several times with the same result. With a depth of 0·24 I could hear no note at all when using the gas. Then I pulled out the piston to 0·35, and the gas gave a clear musical note; on the second squeeze the note was considerably deepened. The specific gravity of the gas from the jet, as calculated from these data, would be to that of the air at the time, as 14 to 25, or as 0·56 to 1. This happens to be the specific gravity of carburetted hydrogen, but that of common street gas is heavier. Perhaps my measurements were not quite accurate; probably the note given by the gas being really fainter (though not perceptibly so) than that given by air somewhat falsified the judgment. A very slight difference in the data would raise the 0·56 to 0·60 or more.

By the use of hydrogen the little whistle when set at 0·14 inches would produce 312,000 vibrations per second. I know by experiment on others that it will give a true musical note when made much shorter than this, and I see no cause to doubt that it will sound truly at half the above length, and therefore be capable of emitting twice the above enormous number of vibrations per second.

Mr. Hawksley is making for me an apparatus with small gas bag for hydrogen pure or diluted, valves, and an indiarubber ball to squeeze, to enable hydrogen to be used with the whistle when desired. The whistle is fixed to the end of a small indiarubber tube in order to be laid near the insect whose notice it may be desired to attract.

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[1] "South Kensington Conferences, in connection with Loan Exhibition of Scientific Apparatus, 1876," p. 61.

FRANCIS GALTON

PRELIMINARY NOTE ON THE BACILLUS OF TUBERCULOSIS (KOCH)

I. THE absorption and consequent retention of certain stains by this bacillus does not appear to be effected by the hydrates of potassium, sodium, and ammonium and by aniline alone. Sodic phosphate, potassic acetate, vegetable alkaloids, &c., appear to exert a similar action. Further experiments are in progress. I have some very good preparations which were rapidly stained with a *very faintly* coloured stain containing sodic phosphate (sod. phos. cryst. B P.).

II. The sections of tissue shown (by the kind arrangement of Mr. Blaker) at the Brighton meeting of the British Medical Association, in which the bacilli were very distinct, were stained, &c., then floated on to the glass slides, *dried over concentrated sulphuric acid* (or fused CaCl$_2$), and mounted in balsam. Hitherto my attempts to fix the colour of the bacilli, by means of a mordant, in such a way that it might remain unaffected by alcohol, and by oil of cloves, have not proved successful.

III. Treatment with a solution of potassic acetate will probably prove well adapted to free preparations from those last traces of nitric acid which so often cause their ultimate destruction.

From (II.) I should omit a very beautiful and remarkable preparation showing the spores of this bacillus in the lymphatics of the lung. This slide was prepared by Dr. Barron, of University College, Liverpool, and for his kindness in lending it to me and for much invaluable advice I am very grateful.

To Mr. Blaker, M.R.C.S., of Brighton, and to Mr. Black, M.R.C.S., of the Sussex County Hospital, I am under many obligations for their kindly interest and assistance.

J. W. CLARK

THE SHAPES OF LEAVES[1]

III.—*Origin of Types*

THE two most general and distinctive types of foliage among angiosperms are those characteristic of monocotyledons and dicotyledons respectively. They owe their principal traits of shape and venation to the manner in which these two great fundamental classes have been separately evolved from lower ancestors.

Mr. Herbert Spencer has shown that there are two chief ways in which a central axis or caulome may conceivably be developed from an integrated series of primitive stalkless creeping fronds. The *first* way is by the in-rolling or folding of the fronds so as to form a complete tube, often with adnate edges, as represented in the accompanying diagram (Fig. 20), modified by Mr. Spencer's kind permission from the "Principles of Biology." For details of the explanation, the reader must be referred to that work (vol. ii. part iv. chap. iii.); it must suffice here to note that as in such case each frond must envelop the younger fronds within it, the process is there shown to eventuate in an endogenous stem and a monocotyledonous seed—two characteristics found as a matter of fact constantly to accompany one another in actual nature. The *second* way is by the thickening and hardening of a fixed series of midribs, as shown in the next diagram (Fig. 21), also modified after Mr. Spencer; and this method must necessarily result in an exogenous stem and a dicotyledonous seed. The diagrams in Figs. 22 and 23, which represent according to Mr. Spencer (slightly altered) the development of the monocotyledonous and dicotyledonous seedling respectively, will help further to illustrate the primitive characteristics of the two types.

The monocotyledonous type of foliage is for the most part extremely uniform and consistent, in temperate climates at least, for in the tropics the presence of large arborescent forms, such as palms and screw-pines, as well as of gigantic lilies, amaryllids, and grasses, such as the bananas, yuccas, agaves, and bamboos, gives a very distinctive aspect to the *ensemble* of the class. Being in principle a more or less in-rolled and folded frond, every part of which equally aids in forming the caulome or stem, the monocotyledonous leaf tends as a rule to show little distinction between blade and leaf-stalk, lamina and petiole. For the same reason, the free end also tends to assume a lanceolate or linear shape, while the lower part usually becomes more or less tubular or sheathing in arrangement. Again, for two reasons, it generally has a parallel venation. In the first place, since the leaves or terminal expansions are mere prolongations or tips to the stem-forming portion, it will follow that the vascular tissues will tend to run on continuously over every part, instead of radiating from a centre which must in such a case be purely artificial. In the second place it is clear that parallel venation is the most convenient type for long narrow leaves, as is plainly shown even among dicotyledons by such foliage as that of the plantains, descended from netted-veined ancestors, but with chief ribs now parallel. Still better are both these principles illustrated in those cases among dicotyledons where the lamina is suppressed altogether, and the flattened petiole assumes foliar functions, as in *Oxalis bupleurifolia* and *Acacia melanoxylon* (Fig. 24). These phyllodes thus resembling in their mode of development the monocotyledonous type, and continuous throughout with the caulome-portion of the primitive leaf, exhibit both in shape and venation the chief monocotyledonous characteristics. A typical monocotyledon in shape and venation is represented in Fig. 25.

The dicotyledonous type, though far more varied, is equally due in its shape and venation to the original characteristics implied by its origin. Only the midrib instead of the whole leaf being here concerned in the production of the stem, there is a far greater tendency to distinctness between petiole and lamina, and a marked preference for the netted venation. The foliar expansion is not here a mere tip; it becomes a more separate and decided element in the entire leaf. And as the petiole joins the lamina at a distinct and noticeable point, there is a natural tendency for the vascular bundles to diverge there, making the venation palmate or radiating, so as to distribute it equally to all parts of the expanded surface. Fig. 26 shows the resulting characteristic form of dicotyledonous leaf. Its variations of pinnate or other venation will be considered a little later on.

Among monocotyledons, the central type is perhaps best found in the mainly tuberous or bulbous orders, such as the orchids, lilies, and amaryllids. These orders, having rich reservoirs of food laid by underground, send up relatively thick and sturdy leaves; but their shape is decided by the ancestral type, and by their strict subordination to the central axis. Hence they are usually long, narrow, and rather fleshy. Familiar examples are the tulips, hyacinths, snowdrops, daffodils, crocuses, &c. Those which have small bulbs, or none, or grow much among grass, like Sisyrinchium, are nearly or quite linear; those which raise their heads higher into the open, like Listera, are often quite ovate. Exotic forms (bromelias, yuccas, agaves) frequently have the points sharp and piercing, as a protection against herbivores. In the grasses there is generally no large reservoir of food, and their leaves accordingly show the central type in a stringy drawn-up condition. So also in sedges, woodrushes, and many others. But where the general monocotyledonous habit has been more lost, and something like the dicotyledonous habit acquired, the leaves become more like those of the opposite class. Thus the Arums, with their very unlilylike mode of growth, and their long petioles rising high into the open air, have usually a very distinct broad lamina, and have the veins accordingly branched or netted, almost as in dicotyledons. Very much the same type recurs under similar circumstances in *Sagittaria sagittifolia* (Fig. 27). Still more markedly dicotyledonous-looking are the leaves of certain very aberrant Amaryllids, such as *Tamus* and the other Dioscoreideæ, which have taken to climbing, and have therefore acquired broader leaves with netted veins between the ribs. Compare with these the like result in *Smilax;* and then look at both side by side with such dicotyledons as *Convolvulus*. The influence of the ancestral type is here seen in the arrangement of the main ribs; the influence of environment is shown both in the approximation of general shape, and in the netting of the minor veins. Once more, the ovate type of Listera leads on readily enough to the whorled leaves of *Paris* and *Trillium*, where the venation has become similarly netted. A bushy type, like *Ruscus*, develops broad leaf-like peduncles, which closely simulate the true leaves of dicotyledonous bushes with like habit, such as box or privet.

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[1] Continued from p. 466.

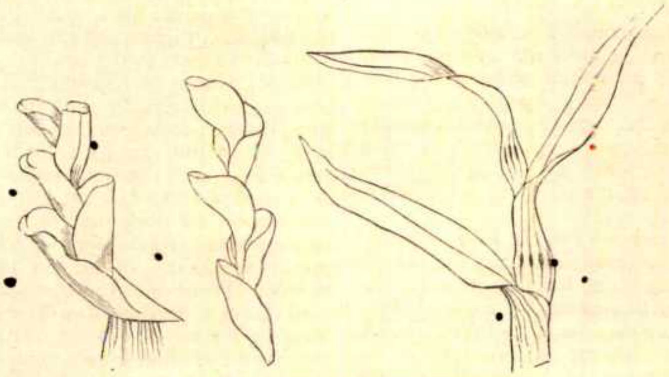


FIG. 20.—Developmen of Monocotyledonous stem.

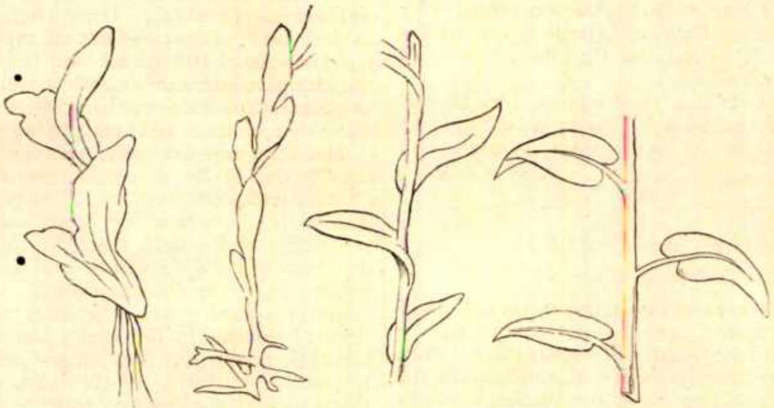


FIG. 21.—Development of Dicotyledonous stem.

But the widest departure of all from the central monocotyledonous type is found in leaves like those of the tropical arborescent forms—the palms, screw-pines, &c. Most of these have long pinnate foliage, whose origin may best be considered when we come to examine the chief dicotyledonous types; meanwhile such forms as the cocoanut or the date-palm may be advantageously compared, as to conditions and general shape, with the tree-ferns in one direction, and the cycads in another. The bananas cast much analogous light upon the origin of these tropical pinnate forms. Where the plant is less arborescent, as in *Chamærops*, the leaf assumes rather a fan-shaped than a pinnate development.

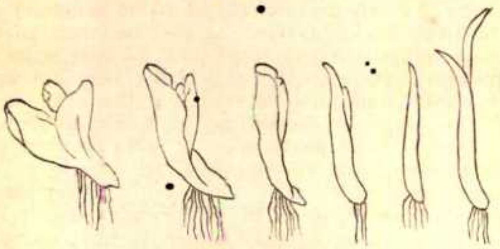


FIG. 22.—Development of Monocotyledonous seedling.

Among dicotyledons it may be fairly assumed that the earliest form of leaf was simple, ovate, and nearly ribless, or with faint digitate venation. This is shown both by the nature of the earliest leaves in most seedlings, and the constant recurrence to such a type wherever circumstances are favourable for its reproduction. Hence, as a whole, digitate venation seems the commonest in most humble dicotyledons; and the only problem is how pinnate venation came to be substituted for it in certain cases. The answer seems to be that wherever circumstances have caused leaves to lengthen faster than they broadened, and so to assume a lanceolate rather than an ovate shape, the tendency has been for the main ribs to be given off, not from the same point, but a little in front of one another. If the technical botanists will pardon such a phrase, the internodes of the midrib, usually suppressed, seem here to have been fully developed. Figs. 28, 29, and 30 show the stages by which such a change may be brought about. Figs. 31, 32, and 33 exhibit a slightly different form of the same tendency.

That this is the real origin of pinnate venation seems pretty clear on a comparison of a good many otherwise closely related forms. Look for example first at the rounded, almost orbicular leaf of *Geranium molle* and its allies, with palmate ribs ; and then look at the long, narrower, doubly pinnate, and pinnately-ribbed leaves of *Erodium cicutarium*. Or again, look at the common cinquefoil, erect and palmate ; and then at silver-weed, long, creeping, closely pressed to the ground, and with numerous pinnate leaflets. Once more, compare *Alchemilla* with *Poterium* and *Sanguisorba*. As a still simpler instance, where we get the difference in its first beginning, contrast *Ranunculus acris* with *R. repens*, or the least compound leaves of the blackberry bramble with its own most compound foliage. As a rule the most pinnate groups, such as the lesser crucifers, the peaflowers, &c., have very long leaves.

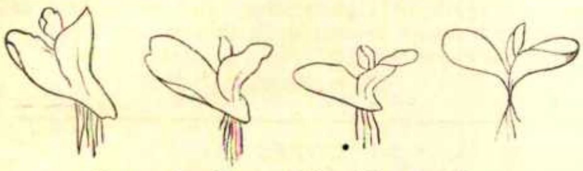


FIG. 23.—Development of Dicotyledonous seedling.

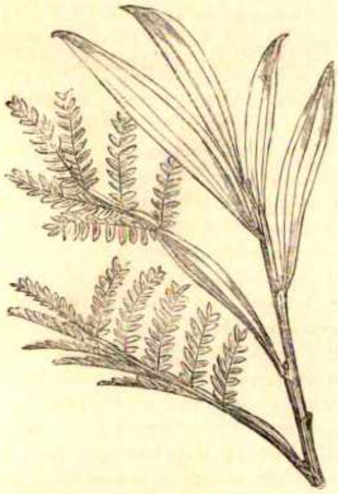


FIG. 24.—*Acacia melanoxylon.*

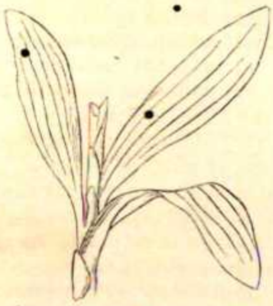


FIG. 25.—Typical Monocotyledonous leaves and venation.

This suggested origin of pinnate venation in dicotyledons becomes even more probable when we look at the pinnate members of other classes. Among monocotyledons the long-leaved arums, though their venation is fundamentally parallel in type, have yet acquired a branching and practically pinnate set of ribs. The plantains and bananas, with very long and broad foliage, carry the same tendency yet further ; for their leaves are pinnately ribbed from a stout midrib. The lower shrubby or bushy palms, like Chamærops, have fan-shaped leaves, with veins diverging in rough parallelism from a common centre ; that is to say, they are in fact palmate ; but in the taller arborescent palms, with their long leaves, the internodes of the midrib (to use the same convenient phrase once more) are fully developed, so that the leaf becomes pinnatifid. In this case the subdivision into leaflets is probably protective against tropical storms. The broad-leaved plantains and the Chamærops, though so much shorter than the pinnate palms, are often torn by the wind, and a plantain leaf so torn into ribbons closely resembles a cocoanut leaf: in the taller palms this disruption between the ribs becomes normal. Compare Zamia and the other cycads among gymnosperms.

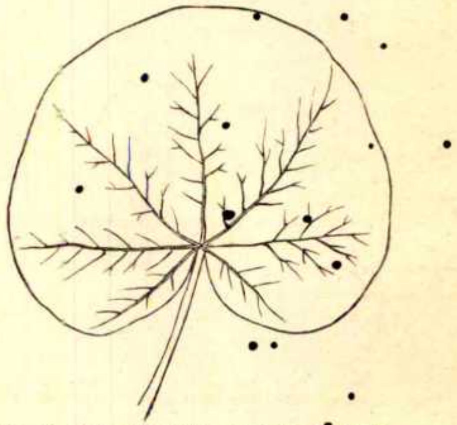


FIG. 26.—Typical Dicotyledonous leaf and venation.

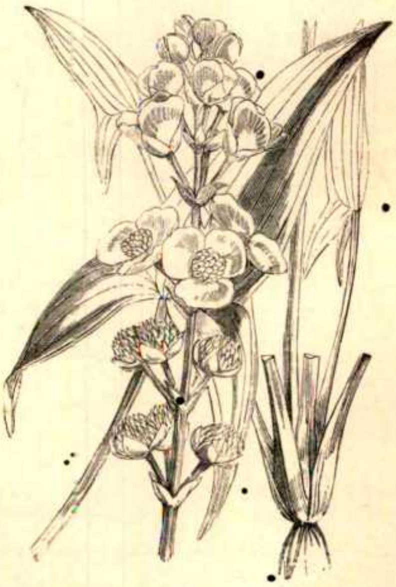
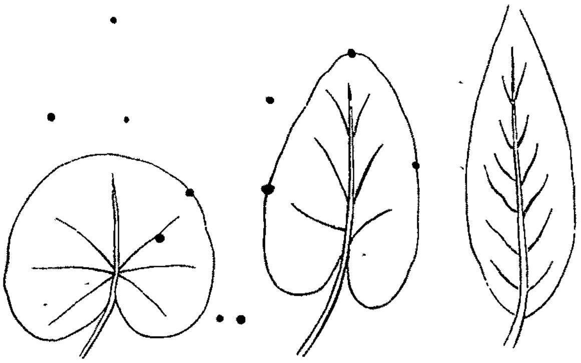


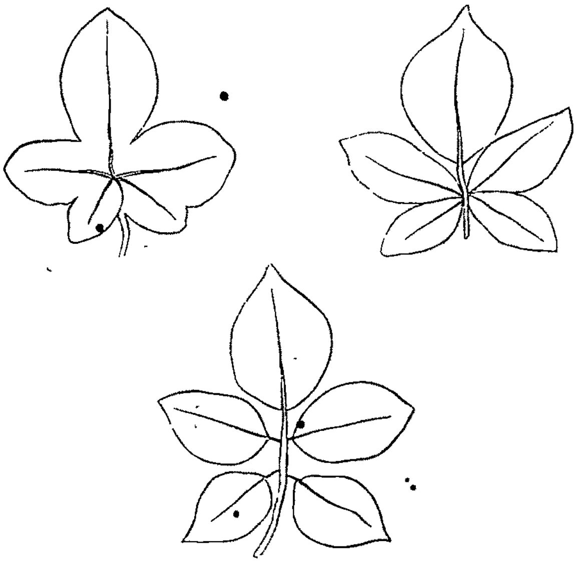
FIG. 27.—*Sagittaria sagittifolia.*

Once more, the ferns are a class with long lanceolate fronds as a rule, and their venation is almost always pinnate ; the only ferns that vary much from the central type being some like the Maidenhairs, which are tufty and rather ovate in general form, and have so modified their venation as closely to approach the herb Roberts and other hedgerow plants in the outer effect. We may fairly conclude, therefore, that pinnate venation is best adapted to very long leaves, both because of the support it gives to the cellular mass and because of the easy manner in which it distributes sap to every part alike.

It seems also probable that pinnate ribs are especially adapted to forest trees. Most of these indeed have their leaves rather long in outline—like the ash, the oak, the chestnut, the walnut, the mountain ash, the laurels, the hornbeam, and the willow—while others in which the primary ribs are palmate—like the horse-chestnut and the plane—have their secondary ribs pinnate and their lobes or leaflets very long, so that the total effect is in the end pretty much the same. But even when the leaf is rather shortened in general outline, as in the elm, the beech, the alder, and the poplar, the venation is still pinnate. Doubtless this form of ground-plan protects the leaves of these exposed trees best against the wind; and where the leaflets are much subdivided, as in the acacias, the subdivision may be regarded as a protection against severe storms.



Figs. 28, 29, 30.—Gradation from palmate to pinnate venation.



Figs. 31, 32, 33.—Gradation from palmate lobes to pinnate leaflets.

The shapes of leaves in each particular species of plant thus depend in ultimate analysis upon two factors: first, the ancestrally-inherited peculiarities of type and venation; and second, the actual conditions to which the species is now habitually exposed. Accordingly, under the same conditions, a monocotyledon and a dicotyledon will tend to assume approximately similar general external forms; but their underlying ancestral peculiarities may generally be perceived through the mere analogical resemblance produced by an identical environment. By the interaction of the two factors we must endeavour to explain every particular form of leaf. To do this throughout the whole vegetable kingdom would be of course an endless task, but to do it in a few selected groups is both a practicable and a useful botanical study. The ground-plan will always depend upon the ancestral type; the outline, degree of segmentation, and minuteness of cutting, will always depend upon the average supply of carbonic acid and sunlight. Grant Allen

(*To be continued.*)

NOTES

Sir John Lubbock did right to ask the Prime Minister on Monday, whether, in remodelling the department of the Lord President of the Council, he would consider the desirability of separating the actual Minister of Education in the House of Commons from that office, and of transferring to him the power of appointing the inspectors and other officers on whom the satisfactory working of the education of the country so greatly depends. As might have been expected, Mr. Gladstone held out no hope of any change being made for a long time; that, however, is no reason why the efforts of the friends of science and education in this direction should cease.

The Grocers' Company have issued a scheme for the encouragement of original research in sanitary science. It consists of two forms of endowment: the one, meant as maintenance for work in progress, in fields of research to be chosen by the worker himself; the other, meant as reward for actual discovery in fields of research to be specified from time to time by the Company. With the former intention the Company establishes three Research Scholarships, each of 250*l*. a year; with the latter intention they appoint a Discovery Prize of 1000*l*., to be given once in every four years. The Research Scholarships are intended as stipends for persons engaged in making exact researches into the causes of important diseases, and into the means by which the respective causes may be prevented or obviated. The Court of the Company propose to appoint to two of the scholarships in May, and to a third in May, 1884. The Discovery Prize is intended to reward original investigations, which shall have resulted in important additions to exact knowledge, in particular sections of sanitary subject-matter. The Court will, once in four years, propose some subject for investigation; and the first subject will be announced in May.

The Annual Report of the City and Guilds of London Institute, taken in conjunction with the Annual Meeting held last week, shows that technical education has taken firm root and is making rapid progress in this country. Though hardly yet so universal as on the Continent, there is every reason to believe that it soon will be, and Lord Selborne, who presided at the Annual Meeting, was justified in congratulating the Institute on its success. As the *Times*, in a sensible article on the Annual Meeting, says: "Lord Selborne did not dwell at length upon the general aspects of technical education. He assumed, and he had good reason to assume, that the need for a systematic development of it is proved beyond question, and is almost universally accepted. No observer now doubts that if the English artisan is to hold his ground in the struggle for existence, he must be kept up to the mark by proper teaching; and no one who has at heart the moral well-being of the working classes doubts the enormous importance of giving them an insight into principles and processes which will raise their work as much as possible out of the mere mechanical groove."

The following are the arrangements for the lectures after Easter at the Royal Institution:—Prof. J. G. McKendrick, ten lectures on physiological discovery; Dr. Waldstein, four lectures on the art of Pheidias; Prof. Tyndall, three lectures on Count Rumford, originator of the Royal Institution; Mr. R. J. Poole, three lectures on recent discoveries in (1) Egypt, (2) Chaldæa and Assyria, (3) Cyprus and Asia Minor; Mr. A. Geikie, six lectures on geographical evolution; and Prof. C. E. Turner, four lectures on historical sketches of Russian social life. The discourses on the Friday evenings will be as follows:—April 6, Dr. Archibald Geikie, F.R.S., The Cañons of the Far West; April 13, Dr. Waldstein, The influence of athletic games on Greek art; April 20, Prof. Bayley Balfour, The island of Socotra and its recent revelations; April 27, Dr. C. William Siemens, F.R.S., Some of the questions involved in solar physics; May 4, Robert H. Scott, F.R.S., Weather knowledge in 1883; May 11, Prof. Huxley, V.P.R.S., Oysters and the oyster question; May 18, Prof. C. E. Turner, of the University of St. Petersburg, Kustarnoe proiezvodstro: or, the peculiar system of domestic industry in the villages of Russia; May 25, Prof. Flower, F.R.S., Whales, present and past, and their probable origin; June 1, Frederick Pollock, LL.D., The sword: its forms and its history; June 8, Prof. Dewar, F.R.S.

IN reference to the course of ten lectures on physiological discovery, to be given at the Royal Institution on Tuesdays, beginning April 3, by Prof. J. G. McKendrick, we may say that the object of the course will be to trace the progress of physiological research from about the beginning of the sixteenth century to recent times, and more especially along those lines which have led to great results. Admitting that the deepest foundation of physiological science is a knowledge of structure, both of organ and of tissue, it will be the aim to show how physiology has gradually attempted to solve some of its problems by the methods of physics and of chemistry, and has thus become a branch of experimental science. The method followed will be to describe briefly the lives of the great discoverers, to indicate the influence of contemporary science on their ideas and opinions, and to show how their labours have brought us to our present position. As far as possible, the fundamental experiments of discoverers will be shown or illustrated, and these will be compared with present methods.

BARON NORDENSKJÖLD, having inspected the Royal Mail Steamer *Sophia*, which the Government have asked the Swedish Parliament to lend for his expedition to Greenland, finds that the vessel is not large enough to carry the quantities of coals and provisions required, although very suitable in other respects. He has therefore decided that a vessel shall be despatched from Denmark with these requisites, and depots established in convenient places on the coast. The *Sophia* will be overhauled and fitted for her voyage in Gothenburg, and as her commander Capt. Nilsson, of that city, has been selected.

THE position of the Lena Meteorological Station is 73° 22′ 30″ N. lat. and 126° 34′ 55″ E. long. The house erected there for the observers is reported to be quite comfortable, and the health of the expedition is satisfactory.

THE group of fishing Chukches, which Baron Nordenskjold has prepared from materials collected in the *Vega* expedition for the coming Fisheries Exhibition, is now on view in Stockholm.

WITH the completion of the buildings in which the varied collection of the great International Fisheries Exhibition is to be housed, the preparatory work of the executive committee is drawing to an end. Not much remains to be done to the buildings which now almost cover the southern half of the Horticultural Society's gardens, and the nature and distribution of the exhibits may now be approximately given. Before handing over to the care of representatives of the colonies and of foreign Powers the places allotted to their countries, the committee on Friday invited members of both Houses of Parliament and their friends to see the buildings. To add to the interest of the aquaria, Lord Walsingham has offered to let off a lake, of about seventy-two acres in extent, on his estate at Merton, in Norfolk, and to send all the fish worth forwarding alive, and besides pike, perch, tench, and other coarse fish, he promises 1000 specimens of the celebrated golden tench. Additional value will be given to the natural history department by the exhibition in a building near the new Natural History Museum of the fine collection of fish preserved in spirit now to be brought from Bloomsbury. In order to make the exhibition as truly popular as could be desired, it will be kept open in the evening, and brilliantly lighted by electricity.

AT the installation of Mr. Bright to-day as Lord Rector of Glasgow University, the degree of LL.D. will be conferred on Dr. Joseph H. Gilbert, F.R.S., and Prof. Fleeming Jenkin.

ON February 26 there was discovered in the snow in several places in Trondhjem Amt, in North Norway, a fine dust, which, it was believed, originated from the Iceland volcanoes, such an occurrence having taken place in 1876. Dr. H. Reusch, of the Mineralogical Faculty of Christiania University, having examined the samples forwarded to him, now states, however, that the dust is not of an eruptive nature, but consists of common sand, fine stones, quartz, hornblende, and talc, mixed with very fine particles of vegetable matter. The phenomenon is nevertheless very remarkable, as the dust must have been carried a very long distance, the whole of the country having for months been covered with deep snow. The dust fell over a district of several degrees. The wind blew strongly from north-north-west.

ON the night of the 4th inst. there was observed in Gestrikeland, in Sweden, a display of aurora borealis, the extent, vividness, and magnitude of which, it is reported, has not been observed in that country for years. An interesting feature of the phenomenon was that the big clouds, which from time to time passed below the aurora, did not in the slightest degree affect the phenomenon.

A TELEGRAM from Messina states that on the afternoon of March 20 a shower of small stones began to fall, proceeding from an eruption of Mount Etna. The atmosphere was thick and dark.

PROF. VIRCHOW has started on a journey to Sicily, whither he goes for archæological purposes. He contemplates returning in two months.

A COMMUNICATION from Dr. Joule, F.R.S., was read at a recent meeting of the Manchester Literary and Philosophical Society, on the use of lime as a purifier of the products of combustion of coal gas. The slaked lime is placed in a vessel the bottom of which, about one foot diameter, is slightly domed and perforated with fine holes. The vessel is suspended about six inches above the burner. It is found that a stratum of four or five inches of lime is sufficient to remove the acid vapours so far as to prevent them from reddening litmus paper. The lime seems in many respects to present important advantages over the zinc previously recommended.

MR. ELLIS LEVER has offered a prize of 500*l.* for a new miners' safety lamp, and has requested the Council of the Society of Arts to appoint one of the judges to award the prize. In accordance with this request, the Council have appointed Prof. F. A. Abel, C.B., F.R.S.

AN enormous aerolite fell on February 16, a little before 3 p.m., in a ploughed field near Alfianello, between Cremona and Brescia, sinking more than one metre in the ground, and producing a rumbling noise, heard twenty kilometres off, and a reeling of the nearest houses as by an earthquake. Unhappily the ignorant country people, when the first fright passed, with mattocks and sticks smashed it and took away the pieces, so that Prof. Calderoni, who directly ran up from Cremona, could obtain only some little fragments for chemical analysis and for scientific cabinets.

A SCHEME is proposed for introducing electric lighting into the Canton of Vaud. The motive force would be derived from turbines of 5000 horse-power at Vallorbes, and the water supply being constant and abundant, it is believed that gas, which is very costly in Switzerland, may be entirely dispensed with throughout the district.

A VERY severe shock of earthquake was experienced in Cyprus on the morning of March 5, at 7.30, lasting about fifty or sixty seconds. At Limassol the houses swayed and rocked in the most appalling manner, and uncemented walls fell to the ground. It was impossible for foot passengers in the streets to keep their balance without assistance. The mules and horses staggered about as though in fits. It was altogether the severest shock which has been recorded for many years.

WE have received copies of the circulars just issued by the Local Scientific Societies Committee of the British Association to 324 societies, for the purpose of obtaining such information as will be useful in suggesting further action. Appended is a list of about 120 local societies which publish Proceedings.

THE Easter excursion of the Geologists' Association will be to Hythe, Romney Marsh, Sandgate, and Folkestone (March 26 and 27). On April 7 the Association will visit Westcombe Park, Greenwich; on April 14 the College of Surgeons; and on April 21 Berkhampstead and Boxmoor.

WE understand that a new weekly journal, devoted to the popular exposition of sanitary matters and to the education of the people in the laws of health, will be shortly issued by Messrs. Wyman and Sons, London. The new journal will be entitled *Health*.

THE former limits of the ice-sheet of the Glacial period appear to be still more and more extended by Russian geologists, in proportion as the post-Pliocene formations of Russia are better explored. We notice in a recent monograph on the Geology of the Volga, by M. Krotoff, that the author, who is well acquainted with this region, considers the glacial formations described by Prof. Miller in the southern parts of the province of Nijni-Novgorod, as due to the action of glaciers, and not of floating ice.

THE young Society for Caucasian History and Archæology, founded in 1881, has already published a first fascicule of its *Bulletin*; the second will soon follow. Prof. V. Miller has published his linguistic "Osetian Studies," containing in the appendix a paper on the religious beliefs of the Osets; and Prof. Patkanoff has published the first part of his "Materials for an Armenian Dictionary," as well as a pamphlet "On the Cuneiform Inscriptions of the Van system discovered in Russia."

THE Administration of Public Instruction of the Caucasus has conceived an excellent idea which cannot be too much recommended for other countries; it is to invite schoolmasters to write descriptions of their localities, and to collect local traditions, folk-lore, &c., and to publish the papers received in the shape of a special collection. It is easy to conceive, indeed, the amount of knowledge which might be gathered in this way, and the attraction which is thrown by a scientific pursuit into the wearisome life of a schoolmaster, who is lost in a small town or village, far from intellectual centres. When he knows that his work will not be lost, and when he is supplied from an intellectual centre with the scientific works he needs, he surely will find interest in his pursuit. This of course applies more to Russia than England. The two first parts of the collection thus started on the Caucasus wholly confirm these previsions; as is seen from an analysis of them published in the *Izvestia*, they contain, indeed, much valuable information. The descriptions of Erivan, Gori, Wakhichevan with its district, and of Chernolyesskoye village are spoken of as very useful work. Two papers, on the formation of Lake Paleostome, and a summary of all places where the Caucasus is mentioned by the ancients, are very elaborate; whilst a series of smaller papers and notes contains a variety of ethnographical sketches, folk-lore, and traditions.

LAMPART AND CO. of Augsburg are issuing in parts a third revised edition of Hellwald's "Kulturgeschichte in Ihrer Naturlichen Entwicklung bis zur Gegenwart." Trubner and Co. are the London publishers. The work will be completed in twenty parts.

AT the last meeting of the Meteorological Society of France, M. Moureaux, physicist to the Bureau Central, read a paper showing that the regimen of the rains south of the Central Plateau was independent of the meteorological conditions on the oceanic side. This communication is considered as an argument in favour of granting to the Bureau Météorologique of Algiers the privilege of being in direct communication with the other offices, and issuing warnings for the northern side of the Mediterranean.

THE additions to the Zoological Society's Gardens during the past week include a Common Seal (*Phoca vitulina*), British Seas, presented by Mr. William Whiteley; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. Campbell; two Prairie Grouse (*Tetrao cupido*) from North America, presented by Mr. Henry Nash; six Common Trout (*Salmo fario*), British fresh waters, presented by Mr. S. Wilson; two Common Seals (*Phoca vitulina*), British Seas, eight Prairie Grouse (*Tetrao cupido*) from North America, deposited; three Common Sheldrakes (*Tadorna vulpanser*), three Common Pintails (*Dafila acuta*), four Shovellers (*Spatula clypeata*), European, four Chilian Pintails (*Dafila spinicauda*) from Antarctic America, two Bahama Ducks (*Dafila bahamensis*) from South America, two Chiloe Wigeons ((*Mareca chiloensis*) from Chili, nine Summer Ducks (*Aix sponsa*) from North America, six Mandarin Ducks (*Aix galericulata*) from China, purchased; an Axis Deer (*Cervus axis* ♂), two Black Swans (*Cygnus atratus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE OBSERVATORY AT MELBOURNE.—The seventeenth annual Report of the Board of Visitors of this establishment, together with the Report of the Government astronomer, Mr. Ellery, for the year ending June 30, 1882, has just been received. The meridian work with the transit-circle was for the most part limited to observations of standard stars, for the ordinary purposes of an observatory and the determination of places of stars used for positions of comets. The 8-inch equatorial had been arranged for the observation of the small planets *Victoria* and *Sappho*, during the last autumn, according to a programme agreed upon with several European and American, and other southern observatories, with the view to another determination of the solar parallax. The large reflector was employed on celestial photography, for sketching a number of Sir John Herschel's smaller nebulæ, for drawings of comet 1881, IV., &c. The nebula about η Argus was examined on three evenings, and was found to agree very closely with the drawing made in 1875. The majority of the smaller nebulæ were found to accord well with Herschel's descriptions. Nos. 57 and 1423, however, were much fainter than Herschel indicated, and Nos. 1655 and 218[illegible] differed considerably from his description. The positions of these nebulæ for 1883·0 with Herschel's notes are as follows:—

		R A. h. m. s.	N.P D. ° ′
No.	57	0 21 43	147 37·7
,,	1423	6 25 6	121 12·3
,,	1655	8 16 27	125 50·9
,,	2181	10 37 27	125 45·0

No. 57.—Pretty bright, small, round, much brighter in the middle.

,, 1423.—Pretty bright; considerably large, round, very little gradually brighter in the middle; 4′.

,, 1655.—A double star = *h.* 4023 in a pretty small nebula, among some seventy stars.

,, 2181.—Pretty faint, small, much extended in 0°±; very suddenly, very much brighter in the middle; the first of three.

The photo-heliograph was used on every fine day possible, and 217 pictures were obtained in the year.

The necessary funds have been voted for a new transit-circle more in accordance with the modern requirements of astronomy, and its construction has been intrusted to Messrs. Troughton and Simms. Mr. Christie, the Astronomer-Royal, was invited to modify the specification sent to England, if he found reason to do so.

THE SUPPOSED VARIABLE μ DORADÛS—A SPURIOUS STAR.—Dr. B. A. Gould has made a very unexpected discovery, from which it appears that μ Doradûs of our catalogues, long supposed to be a variable star, was never observed by Lacaille in the position he assigns it in the Catalogue of the *Cœlum Australe Stelliferum*, and further, that by similar error, five other stars observed by Lacaille on the same day, which are found in the reduced catalogue published by the British Association, have no existence in the positions given. The case is a curious one, and as the *Cœlum Australe* of Lacaille is now a scarce work, we may be excused for transcribing the observations in question as they stand. They were made in Zone XI., 1751, December 16, *in parte inferiore* of Lacaille's rhomboid; the numerals are our own:—

	mag.	h.	m.	s.		mag.	h.	m.	s.
No. 1	6	4	17	38	No. 6	6·7	4	46	27
			24	2				53	34
,, 2	7	4	39	51	,, 7	7	4	51	5
			47	9				54	33
,, 3	7	4	25	23	,, 8	5	4	59	22
			31	0			5	8	28
,, 4	6·7	4	30	16	,, 9	7	6	5	53
			32	38				9	8
,, 5	7	4	41	33					
			43	41					

Lacaille appears to have entered correctly the times of beginning and ending of describing the chord of his rhomboid for Nos. 1 and 2, but instead of 4h. 25m. 23s. for the third star, the time was really 5h. 25m. 23s., and this error of 1h. runs on up to No. 8 inclusive; No. 9 is correct. This will be readily seen by inspecting the above times. The star entered in the Catalogue as μ Doradûs is No. 8, called 5m. in the observations but 6m. in the Catalogue, which gives its place for 1750·0, R.A. 76° 11′ 17″, Decl. −62° 7′ 4″. The place given by the B.A. reductions is R.A. 5h. 4m. 44·3s., N.P.D. 152° 6′ 57″, which is correctly deduced from the transits as printed. With the correction of +1h. to the times, the position for 1750 becomes R.A. 6h. 4m. 44·2s., N.P.D. 152° 6′ 49″, and the star "μ Doradûs" is seen to be identical with Brisbane 1172 = B.A.C. 2000 = Stone 2836, in Pictor. The other spurious stars introduced in the Catalogue by the error which Dr. Gould has brought to light are Nos. 1542, 1554, 1633, 1680, and 1706. The following identifications of the stars really observed may be useful:—

Spurious stars of the reduced Catalogue.					Stars really observed by Lacaille		
No. 1542	Reticulum	7m.	=	Stone 2497,	Dorado	7·6m.
,, 1554	,,	6·7	=	,, 2532,	,,	6
,, 1633	Dorado	7	=	,, 2630,	,,	6·7
,, 1680	,,	6 7	=	,, 2707,	,,	6.7
,, 1706	,,	7	=	Brisb. 1109,	Taylor V. 516	
,, 1766	(μ Doradûs)	5	=	Stone 2836,	Pictor	5m.

Brisbane observed a star close upon Lacaille's erroneous position of his μ Doradûs, and according to his general custom gave it Lacaille's magnitude. Moesta (*Astron. Nach.*, No. 1545) stated that he had observed this star at Santiago de Chile from February, 1860, to January, 1865, and had found it 8½ or 9 of Argelander's scale; he therefore considered it to be variable, and thought the period of variation would prove to be of long duration.

THE COMET OF 1812.—MM. Schulhof and Bossert's sweeping ephemerides for this comet are continued in No. 2489 of the *Astronomische Nachrichten*.

INSECTS VISITING FLOWERS

THE interest arising out of the writings of Darwin, Lubbock, and Hermann Muller relative to the part played by insects in their oft-recurring visits to flowers has of late years attracted much attention. The subject, in fact, has created a taste for observation, and an incentive has been given to watch the frequency of visits of various species to certain flowers, and especially to the insects' choice of colours of flower. While the mere registering of visits may seem a comparatively simple one, the reason why insects should show a preference to alight upon flowers of a certain colour, or choose certain species of plants, is a much more complicated problem than at first sight it would appear. Sir John Lubbock has shown by experiment that blue is the bees' favourite colour; H. Müller avers that in the Alps bees are attracted to the yellow rather than the white flowers. However this may be, certain it is that a much larger number of observations are yet needed before a positive law can be deduced. Two papers read at the last meeting of the Linnean Society (March 1): one by Mr. Alf. W. Bennett, "On the Constancy of Insects in their Visits to Flowers," and the other by Mr. R. M. Christy, "On the Methodic Habits of Insects when Visiting Flowers"—point out that a strict watch and ward is being kept on the movements of the busy bee and its kindred. Mr. Bennett states that butterflies show but little constancy in their visits, citing only a few instances to the contrary; but according to him, to some extent they seem to have a choice of colour. The Diptera exhibit greater constancy, though by no means absolute. The Apidæ, especially the hive-bee, manifest still greater constancy. From these data he infers that the ratio of increase is in proportion to the part performed by the insects in their carrying pollen from flower to flower. As respects preference for particular colours, in a series of observations Mr. Bennett has noted among the Lepidoptera that 70 visits were made to red or pink flowers, 5 to blue, 15 to yellow, and 5 to white; the Diptera paid 9 visits to red or pink, 8 to yellow, and 20 to white; Hymenoptera alighted 303 times on red and pink flowers, 126 on blue, 11 on yellow, and 17 on white flowers. Mr. Christy records in detail the movements of 76 insects, chiefly bees, when engaged in visiting 2400 flowers. He tabulates the same, and concludes therefrom that insects, notably the bees, decidedly and with intent confine their successive visits to the same species of flower. According to him, also, butterflies generally wander aimlessly in their flight: yet some species, including the Fritillaries, are fairly methodical in their habit. He believes that it is not by colour alone that insects are guided from one flower to another of the same species, and he suggests that the sense of smell may be brought into play. Bees, he avers, have but poor sight for long distances, but see well at short distances. Of 55 humble-bees watched, 26 visited blue flowers: of these 12 were methodic in their visits, 9 only irregularly so, and 5 not at all; 13 visited white flowers, whereof 5 were methodic and 8 the reverse; 11 visited yellow flowers, of which 5 were methodic and 6 not; 28 visited red flowers, 7 appearing methodic, 9 nearly so, while 12 were the contrary.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—In the last local examination 17 per cent. of those Juniors who sent up papers in Trigonometry obtained no marks, although some questions were of the very simplest nature. Among the Seniors Hydrostatics produced unsatisfactory answers. Many candidates had no ideas worth the name about pressure at a point, density, specific gravity, and weight. This is due partly to corresponding imperfections in some current text-books, and partly to the habit of teaching Hydrostatics apart from general physics or practical applications. The answers in Statics were the least satisfactory; yet according to the examiner there are few subjects in which good teaching tells more quickly than in elementary mechanics. Thus many who passed did very good papers.

Junior Chemistry obtained a favourable report; but the Seniors displayed lack of reasoning power, with great readiness to reproduce cut-and-dried statements from books. In Heat the results of the examination were encouraging and satisfactory. In Experimental Physics generally there was great lack of practical acquaintance with the subjects. Only one candidate did really well among the Seniors. In Botany the answers were weak throughout, showing great lack of teaching by real objects handled by the students. Zoology appears to have been too much studied by Juniors from older and worthless text-books. The Seniors did better, but spread themselves over too wide a field of work. The knowledge of Physical Geography was better than that of Geology, but neither was good.

The report recently made by Mr. R. D. Roberts of his visits to the centres where local lectures have been established, and on the present state of the local lectures' scheme, contains many most interesting facts regarding the high appreciation with which the intelligent working classes regard the lectures, and the difficulties which the cost of the lectures occasions. Of the results of a course of electricity at Newcastle, the examiner says that the work-done in answer to a long and difficult paper of questions was fully equal to that attained in a scientific University course. The greatest difficulty that occurs is not lack of demand for or interest in education, but the provision of funds to meet the expenses. If a solution of this could be found, the scheme would be taken up largely in towns where it is now out of the question. The people who are eager for knowledge and travel long distances to obtain it, in all kinds of weather over the roughest roads, are just those who, if they must pay for the lectures, must have less bread for their families. This is certainly the case with the Northumberland and Durham miners. Whether the State will in some way assist in providing the knowledge and teaching which are so eagerly desired, must be again made a practical and urgent question.

The following are the lectures in Chemistry, Physics, and Mineralogy for the Easter Term (el. signifies elementary, ad. advanced):—

Elementary Course of Chemistry, by a Demonstrator; General Course, continued, Mr. Main, St. John's College; Non-metals, continued, and Organic Chemistry, el., Mr. Pattison Muir; General Principles, continued, and Organic Chemistry, ad., Mr. Muir, Caius College; Organic Chemistry, el., Mr. Scott (Prof. Dewar's assistant); Demonstrations in Gas Analysis, Mr. Scott; Sound, Lord Rayleigh; Heat, Mr. Trotter, Trinity College; Physics, el., Mr. Glazebrook, Trinity College; Physics, el., Mr. Shaw, Emmanuel College; Physics, ad., papers, Mr. Glazebrook and Mr. Shaw; Chemistry and Physics, el., papers, Mr. Pattison Muir and Mr. Shaw.

Practical Chemistry, University, St. John's, Caius, and Sidney College Laboratories.

Practical Physics, Cavendish Laboratory; Demonstrations in Light and Acoustics; and in Optics and Electricity, el.

Mineralogy, Course by Prof. Lewis, and Demonstrations for both parts of the Natural Sciences Tripos.

The following arrangements have been made by Prof. Hughes for lectures during the Easter term:—Local Stratigraphy, Prof. Hughes; Geology (General Course, continued), by Dr. R. D. Roberts, Clare College; Petrology, Mr. Harker, St. John's College; Palæontology, Mr. T. Roberts, St. John's College. Dr. R. D. Roberts will continue to set papers and superintend the course of reading of students in the Museum.

The Strickland Curatorship being about to become vacant, Mr. Salvin having completed his valuable catalogue, a new code of regulations for the Curatorship has been drawn up. The Strickland Curator is to be appointed by Mrs. Strickland, the foundress, during her lifetime; then by Mrs. Catherine Strickland in case she shall survive the foundress; and, after her decease, by the Superintendent of the Cambridge Museums of Zoology and Comparative Anatomy. In addition to caring for the Strickland Collection, the Curator is to take charge of any University ornithological collections, to catalogue them, to assist scientific visitors in studying the ornithological collections, and to aid and promote the progress of ornithological science.

University College, London.—Twenty lectures on Quantitative Analysis will be delivered by Richard T. Plimpton, Ph.D., on Mondays and Fridays at 3 o'clock, during the third term. The first lecture will be given on April 13.

Prof. Stokes, Lucasian Professor of Mathematics in the University of Cambridge, has been appointed to deliver the first course of lectures on Natural Science under the auspices of the Burnett Literary Fund, Aberdeen.

The Earl of Zetland has given 500*l.* to the Edinburgh Association for the University education of woman to found a bursary for the benefit of its students. This bursary will be known as the Earl of Zetland's Bursary.

SOCIETIES AND ACADEMIES

London

Royal Society, March 8.—"Note on the Order of Reversibility of the Lithium Lines," by Professors Liveing and Dewar.

In their communications on the reversal of the lines of metallic vapours, the authors have several times noticed (*Proc. Roy. Soc.* vol. xxviii. pp. 357, 369, 473) the reversal of the lithium lines, and concluded that the blue line is more easily reversed than the orange line. This, however, does not appear to be really the case. When much lithium is introduced into the arc, a second blue line is developed close to but slightly more refrangible than the well-known blue line. This second blue line produces with the other the appearance of a reversal, which deceived the authors until they became aware of the existence of the second line. The blue line (wave-length 4604) is really reversed without difficulty when sufficient lithium is present, but under these circumstances the orange line is also reversed. The latter line is also the one which first (of the two) shows reversal, and also the one which is more persistently reversed. Hence they place the lines in order of reversibility as follows: red, orange, blue, green, violet.

Mathematical Society, March 8.—Prof. Henrici, F.R.S., president (and subsequently Sir J. Cockle, F.R.S., vice-president), in the chair.—Mr. Alfred Lodge, Fereday Fellow of St. John's College, Oxford, was elected a Member, and Major Allan Cunningham and Mr. H. T. Gerrans were admitted into the Society.—Prof. Henrici feelingly announced, in a few well-chosen sentences, the loss the Society had sustained since its last meeting by the death of Prof. Henry Smith, one of its most distinguished ornaments, and who had been a Member almost from its commencement in 1865. The loss to mathematics in this country was almost irreparable, and it would be hard to find anywhere a fitting successor to him as an exponent of the higher geometry. It had been said that there were not half a dozen mathematicians in Europe who could breathe on the mathematical heights to which he was accustomed; it was further true that few were so fitted as he for introducing others to those heights. His charm of manner and power of fixing the attention of his hearers were wondrous, and were as strikingly exhibited at the December meeting of the Society (the last meeting at which he was present) as on any previous occasion. What Clifford once said when reading a paper by Hesse might be said with equal truth of Henry Smith's papers: "This is reading poetry." [Perhaps this Society will miss him more than any other; he was always willing, if possible, to respond to the Secretary's request for a paper, and he was a true imitator of the Jewish king, for he never gave us of that which cost him nothing. "Everything that he did was as perfect as he could make it." In a letter now before us the writer says truly: "Of all who 'knew' him, none knew or saw *him himself* as we did at the Mathematical Society." "Very pleasant" was he to us, and his death has left a void in our ranks which time will hardly fill.] —Mr. J. W. L. Glaisher made a communication on the calculation of the hyperbolic logarithm of π.—Mr. Tucker read (in its entirety) a paper by Prof. Cayley entitled "On Monge's 'Mémoire sur la Théorie des Déblais et de Remblais.'"—Mr. J. Hammond made a few critical remarks on a recent paper by Prof. Sylvester in the *American Journal of Mathematics*.

Zoological Society, March 6.—Osbert Salvin, F.R.S., vice-president, in the chair.—The Secretary exhibited, on behalf of the Rev. F. O. Morris, the drawing of a bird shot in Hampshire in November, 1882, which it was suggested represented a Tinamou of some species that had escaped from captivity.—Mr. J. E. Ady exhibited some microscopical preparations of bone, in one case showing the growth of blood-vessels into cartilage previous to ossification, and in another case presenting a hard section in which the lacunæ and canaliculi were extremely well shown.—Dr. Hans Gadow read a paper on the laryngeal muscles of birds, and pointed out first that the muscles of the syrinx are developed from the sterno-hyoid muscles; and, secondly, that the cutaneous muscles are derived from superficial layers of the common muscular stratum. Thirdly, the author considered the connection between muscle and nerve-supply, illustrating his remarks by diagrams.—A communication was read from the Rev. H. S. Gorham, F.Z.S., containing the descriptions of some new species of Coleoptera belonging to the family Erotylidæ. Twenty-nine new species of this family were described, of which ten were from the Philippine Islands, three were from the Andaman Islands, two from Assam, two from the Malay district, six from Africa, and six from Peru. The species treated of belonged chiefly to the subfamilies *Encaustini* and *Dacnini*, the author reserving the remaining subfamilies for a future communication.—Dr. Gwyn Jeffreys read the sixth part of his communications on the Mollusca procured during the *Lightning* and *Porcupine* Expeditions. This included an account of the specimens of the groups of *Scissurella*, *Trochus*, *Turbo*, and part of *Littorina*, referable altogether to seventy species. Four genera and twenty species were for the first time described as new.—A communication was read from Mr. H. O. Forbes, F.Z.S., describing a species of scarlet *Myzomela* obtained in the Island of Boeroe, one of the Ceram group.—Mr. G. A. Boulenger read a paper on the Geckos of New Caledonia. The object of the author in preparing this paper was that it might serve as a guide to the identification of the Geckotidæ of New Caledonia, and at the same time bring the synonymy into order. To this end the author had compared the typical specimens in the Museums of Brest, Lisbon, Paris, and Brussels with those in the British Museum, and had given short descriptions of every species taken from typical or well-authenticated specimens. The number of species of Geckotidæ actually known from New Caledonia was fourteen: of these two were recorded for the first time, one being new to science.

Geological Society, February 21.—J. W. Hulke, F.R.S., president, in the chair.—Rev. John Birks, Capt. James Scott Black, John Bradford, Thomas Alexis Dash, Henry Lewis, and Thomas Morris were elected Fellows of the Society.—The following communications were read:—On the relation of the so-called "Northampton Sand" of North Oxfordshire to the Clypeus-Grit, by Edwin A. Walford, F.G.S.—Results of observations in 1882 on the positions of boulders relatively to the underlying and surrounding ground in North Wales and North-West Yorkshire; with remarks on the evidence they furnish of the recency of the close of the Glacial period, by D. Mackintosh, F.G.S. The author entered into a consideration of the time which has elapsed since the close of the Glacial period, and stated the main results of his observations as follows:—1. That the average *vertical extent* of the denudation of limestone rocks around boulders has not been more than six inches. 2. That the average *rate* of the denudation has not been less than one inch in a thousand years. 3. That a period of not more than six thousand years has elapsed since the boulders were left in their present positions, by land ice, floating-ice, or both.—Notes on the Corals and Bryozoans of the Wenlock Shales (Mr. Maw's washings), by G. R. Vine. Communicated by Prof. P. Martin Duncan, F.R.S.

Entomological Society, March 7.—Mr. J. W. Dunning, M.A., F.L.S., president, in the chair.—Three new members were elected.—Exhibitions: A specimen of *Polistes hebræus*, Fabr., an East Indian wasp, captured alive in one of the London docks, by Mr. R. McLachlan; Two British Ichneumons, and an orthopterous insect (*Copiophora cornuta*, De Geer) from Central America, by Mr. T. R. Billups; A preparation showing the structure of the thorax in a large beetle (*Chalcolepidius porcatus*, Linn.), by Dr. D. Sharp.—Paper read: "Further additions to Mr. Marshall's Catalogue of British *Ichneumonidæ*," by Mr. J. B. Bridgman.

Physical Society, March 10.—Prof. G. C. Foster in the chair.—New Member, Major W. S. Boileau.—Mr. Shellford Bidwell read a paper on a new method of measuring resistances with constant currents. It consists in placing a resistance-box in the arm of the bridge which afterwards has to contain the unknown resistance. A balance is effected by unplugging resistance in this box. The unknown resistance is then inserted in the same arm, and the balance restored by plugging resistance out of the box. The amount plugged out equals the unknown resistance.—Prof. F. Guthrie made a communication on liquid slabs. Films of liquid, spread out like a flattened drop on a solid surface, are found by the author to have a thickness which is a physical constant for the same liquid, provided the area is very great in proportion to the thickness. Sodium amalgam inserted in a mercury slab causes it to spread out further. Prof. Guthrie also finds that an electric current increases the diffusion of sodium amalgam through mercury in the direction of the current.—Mr. Baily suggested that, as the diffusion produces a current, an opposing current might be found to stop the diffusion. Mr. Stanley said the largest water-drop he had measured was one-fifth of an inch in diameter.

EDINBURGH

Mathematical Society, March 12.—Mr. J. S. Mackay, M.A., F.R.S.E., president, in the chair.—Prof. Chrystal, in his address on "Present Fields of Mathematical Research," remarked at the outset that the times seemed peculiarly suitable for the foundation of such a society in Scotland where, as in England and America, the tide of mathematical research had certainly begun to flow. The direct effect of the work of the society would be to keep alive the interest of its members in mathematics, and especially, by division of labour, to benefit the teacher whose daily tasks leave him somewhat unfitted to undertake in moments of leisure the reading necessary to keep him abreast of the time. Further, such benefits would surely extend their influence to the improvement of secondary education in Scotland. The lines along which members might advantageously work were then indicated in a suggestive sketch of the history in modern times of geometry and algebra. Descartes' system of analytical geometry was the first great step, though for long it remained simply a series of solutions of special problems. The discovery and development of the calculus no doubt kept analytical geometry for a time in the background; but there is every reason to believe that great progress in developing geometrical methods was effected by Pascal, Desargues, Newton, and Maclaurin. With them originated the idea of projection, which was systematised into a powerful geometrical method by Monge and his disciples, Poncelet, Chasles, Brianchon, and others. Monge also, however, established the analytical side of geometry, as well as the synthetic, upon an independent basis; his work has been ably supplemented by Dupin and others, and more especially by Plücker. The treatment by the latter geometer of the singularities of higher plane curves, his introduction of the abridged notation, and his invention of the system of line geometry, have been developed each into an extensive branch of mathematics. At the same time algebra has been differentiating itself into well-marked parts. The theories of forms, of equations, of substitutions, and of determinants have been greatly developed by Abel, Jacobi, Galois, Cayley, Sylvester, Jordan, Clifford, and others. The address concluded with a reference to the theory of transcendents and the closely-related properties of the complex variable. A hearty vote of thanks was accorded to Prof. Chrystal on the motion of Prof. Blyth of Glasgow, seconded by Mr. Muir of Glasgow.

CONTENTS

PAGE
Pathological Anatomy 477
Ensilage. By Prof John Wrightson 479
Our Book Shelf:—
  Adamson's "Another Book of Scraps, principally relating to Natural History" 480
Letters to the Editor:—
  Incubation of the Ostrich.—George J. Romanes, F.R.S. . . . 480
  Difficult Cases of Mimicry.—Alfred R. Wallace; R. Meldola 481
  On the Value of the "Neoarctic" as one of the Primary Zoological Regions.—Alfred R. Wallace 482
  A Remarkable Phenomenon—Natural Snowballs.—Samuel Hart 483
  The Late Transit of Venus.—Samuel Hart 483
  Rankine's "Rules and Tables."—W. J. Millar 483
  Meteors.—Thomas Masheder; Henry Cecil 483
The British Circumpolar Expedition. By Capt. Dawson, R.A. . 484
On the Nature of Inhibition, and the Action of Drugs upon it, IV. By Dr. T. Lauder Brunton, F.R.S. 485
Ben Nevis Observatory. By Clement Lindley Wragge (*With Illustrations*) 487
Hydrogen Whistles. By Francis Galton, F.R.S. 491
Preliminary Note on the Bacillus of Tubercle (Koch) By J. W. Clark 492
The Shapes of Leaves, III. By Grant Allen (*With Illustrations*) 493
Notes 495
Our Astronomical Column:—
  The Observatory at Melbourne 497
  The Supposed Variable μ Doradûs—a Spurious Star 498
  The Comet of 1812 498
Insects Visiting Flowers 498
University and Educational Intelligence 498
Societies and Academies 499

THURSDAY, MARCH 29, 1883

THE AMERICAN ASSOCIATION

Proceedings of the American Association for the Advancement of Science. Thirteenth Meeting, held at Cincinnati, Ohio, August, 1881. (Salem: Published by the Permanent Secretary, 1882.)

IN the same year that the British Association for the Advancement of Science was celebrating its jubilee, this, its eldest daughter, had reached the mature age of thirty years. The volume which embodies the results of the Cincinnati meeting is considerably smaller than the corresponding one published on this side of the Atlantic. The latter, containing the reports and proceedings of the York meeting, is a bulky and closely-printed volume of 824 pages, besides 82 pages of introductory matter and a list of members. The corresponding American volume only contains 416 pages, with about the same amount of introductory matter, in which the list of members is included. The ratio of the one to the other is even less than the above number indicates, for the type used in the American is on the whole larger than in the English volume, the smallness of which, in the Transactions, can only be justified by the necessity for restricting the bulk of the volume.

The American Association appears to be constituted very nearly on the same plan as the British, but there are some minor points of difference. The American consists of Members, Fellows, Patrons, and Honorary Fellows: of which the former two appear to correspond roughly with the Members and the General Committee of the British Association. A donation of one thousand dollars constitutes a Patron—only two persons, however, one of each sex, appear to have availed themselves of that avenue to distinction. The sections, or sub-sections, into which the Association divides itself for purposes of business at the time of meeting, are nine in number, as were those, including departments, at the York meeting of the British Association. But the distribution of the subjects differs. The American Association has a section for Physics separate from Mathematics and Astronomy, rendering permanent the fission which only occasionally takes place with us. Geology and Geography are placed in one section, which certainly would be found impracticable in Britain, as both these departments are in general well supplied with papers. Our Section D (Biology), with its three departments, Zoology and Botany, Anatomy and Physiology, and Anthropology, is divided in America into Biology, Histology and Microscopy, and Anthropology. The remaining sections correspond exactly. The two most noteworthy differences in the American volume are the general absence of the Presidential addresses, so marked and often so valuable a feature of the British, and the small number of Special Committees (and consequently of their Reports). Only in the sub-sections of Entomology and of Anthropology is there any record of addresses by the chairmen, and there does not appear to have been any general address by the President of the whole Association. The number of Special Committees also is smaller than we should have expected. Of these we find but eight, excluding those connected with executive business. They are "On Weights, Measures, and Coinage," "For Obtaining a New Survey of Niagara Falls," "On the Best Method of Science Teaching in the Public Schools," "On Standard Time," "On Stellar Magnitudes," "On State Geological Surveys," and two others, the purpose of which seems a little singular to English readers, one being "On the Registration of Deaths, Births, and Marriages" (the order of sequence seems a little curious), and a "Committee to cooperate with the American Philologica Association in relation to the proper restriction of the degree of Ph.D.," a subject which, we imagine, even if the necessity existed here, the British Association would be a little shy of touching.

It is of course difficult to express an opinion on the requirements of an American institution, but we cannot help thinking that as some of the most useful work of the British Association has been and is being done through its Committees, and that their reports (including those of individuals) are the most valuable part of its volume, the Transatlantic society would do well to develop this feature in its constitution. At the York meeting in 1881 thirty reports were read and forty-eight committees appointed.

There were a considerable number of mathematical and physical papers read before the American Association, but of the majority only the titles are printed. Of the few reported at any length, one, we should have thought, would have been more appropriate placed in the section of Mechanics, as the mathematical reasoning is of the simplest kind. The conclusion, however, is interesting, for it shows, as the result of a number of experiments, that "timber may be injured by a prolonged stress, far within that which leaves the material uninjured when the test is made in the usual way, and occupies a few minutes only." Bars of timber (most of the experiments were made with yellow pine) yielded and broke, generally suddenly, under loads below their average breaking weight under ordinary test. When the load was about three-fifths of the average breaking weight, it was sometimes a full year before they gave way. This suggests pleasant reflections for occupiers of newly-built "jerry" houses in London!

Among the physiological papers there is one on a subject which must, we think, be novel. It is entitled "A Study of Blood during a Long Fast" (by Lester Curtis, of Chicago). In May, 1881, a Mr. John Griscom, of Chicago, commenced a fast of forty-five days. The author was invited by the "managers" to make any investigations that he pleased, and after satisfying himself that the fast was to be conducted honestly, he chose the blood as a subject of study. The first examination made, at the commencement of the fast, shortly after the patient had eaten his last meal, showed the red corpuscles abundant, bright coloured, pure in appearance, regular and smooth in outline. Four days afterwards two kinds were noticed, one pale, almost colourless, large, with a "sticky" aspect, the other deeper in colour than the ordinary corpuscles, smaller and covered with nodules. By the fifth day the colourless corpuscles had disappeared, but they returned in a few days, and continued in greater or less amount to the end. The darker corpuscles assumed various shapes, and many very small ones appeared, apparently by subdivision of the larger. Their aspect was most abnormal on the thirty-ninth day of the fast, when Mr.

Griscom was extremely exhausted; but on the fortieth, after he had been refreshed by a rather long excursion on the lake, the corpuscles returned to a normal condition, except as regards size. This improvement was not lost during the remainder of the fast, though the abnormal appearance to some extent returned.

In the joint section of Geography and Geology are some interesting papers—one, the substance of an evening lecture, describes the Grand Cañon of the Colorado River, and shows that the denudation, of which it is a consequence, commenced in Middle Eocene, and has been continued to the present time, the greater part however having been accomplished by the end of the Miocene. During the whole period there has been a vertical uplift of from 16,000 to 19,000 feet, and a removal of a total thickness of rock equal to about 10,000 feet.

Another interesting paper connected with physiography is by Mr. J. W. Spencer, "Notes on the Origin of the Great Lakes of North America," together with one by Mr. W. Claypole, on "Evidence from the Drift of Ohio, Indiana, and Illinois, in support of the Preglacial Origin of the Basins of Lakes Erie and Ontario." The authors discuss the physiography and geology of the districts in which these lakes are situated, and show the most probable theory of their origin to be that they are fluviatile valleys of preglacial age, which during glacial times were obstructed by the accumulation of drift. This, aided by submergence owing to change of level, has produced the lakes in their present form. These papers are well worth the study of some English geologists, to whom no work seems too small or too great for a glacier, and whose faith at one time seemed quite equal to gulping down Lake Superior itself, sooner than falter in supporting a fascinating theory.

We would venture in conclusion to suggest to the American Association one improvement in detail: this is to imitate the British, and give their volume a cloth binding instead of sending it forth merely stitched in a paper cover, so loosely as to tumble to pieces after a few days' use.

T. G. BONNEY

PRINGSHEIM'S BOTANICAL YEAR-BOOKS

Jahrbucher fur wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Vol. XII. Part 4, and Vol. XIII. Part 3. (Leipzig: W. Engelmann, 1881 and 1882.)

THE two parts now before us include six papers dealing with anatomical and physiological subjects, illustrated by 13 plates, some of them of great beauty. In the concluding part of vol. xii. there are papers by Westermaier, Ambronn, and Zimmermann; while in the third part of vol. xiii. the editor, Dr. Pringsheim, contributes a long controversial paper, and there are two papers—a long one by Godlewski, and a short one by Tschirch.

Westermaier's paper is on the "Intensity of Growth of the Apical Cell and of the Youngest Segments." From an examination of figures of Dictyota, Hypoglossum, Metzgeria, Salvinia, Equisetum, and Selaginella, as given by Naegeli, Goebel, Pringsheim, Cramer, Rees, and Pfeffer, Westermaier concludes that the maximum of the increase in volume in the apical region occurs in general either in the apical cell itself or in the youngest segments, and that taking the region which includes the apical cell itself and the four youngest segments, in none of the plants examined was the increase in volume of the apical cell found to be the minimum for the region. The results are represented graphically and afford very instructive curves.

A paper on the "Development and Mechanical Properties of Collenchyma, a Contribution to the Knowledge of the Mechanical System of Tissues," is contributed by Dr. Ambronn, and is illustrated by six plates of microscopical sections. The Collenchyma with the prosenchymatous fibres of the wood and bast form the mechanical system of Schwendener and Haberland. When the mechanical elements form separate plates, or bundles, or individual isolated cells, the cells are known as *Stereides*, and the whole tissue as *Stereome*. When on the other hand the mechanical cells are united with others which are non-mechanical, as in wood and bast bundles, then Schwendener has distinguished them as *Mestome*. The investigation of the structure of a number of plants shows that the Collenchyma may be arranged in bundles or in the form of a ring, and that in both the arrangement of Collenchyma and Mestome may follow a uniform plan, or the arrangement of the Collenchyma may be quite independent of the Mestome. Ambronn confirms the statement of Haberland that Collenchyma does not originate from any special morphological series of cells, but has the most variable origin: and further confirms the statement of Schwendener that the grouping and arranging of the cells depends entirely upon mechanical and not upon morphological causes. In *Fœniculum vulgare* the bast and Collenchyma of the external bundles are connected together and lie in the same radii, while in *Clematis vitalba* the bast and Collenchyma lie in the same radii but are not connected. In *Philodendron eximium* the Collenchyma forms a ring and is connected with the separate peripheral fibro-vascular bundles, both developing from a zone of secondary meristem. In *Peperomia latifolia* a ring of Collenchyma is formed, but it is independent of the bast. These plants afford examples of the four great types of structure. The Collenchyma cells are always prosenchymatous, often two millimetres in length, or even longer, and they frequently contain secondary partitions, being chambered by numerous fine transverse walls. They always contain fluid very rarely with any chlorophyll. The walls when viewed in a longitudinal section present elongated slit-like pores. Other collenchymatous cells are more parenchymatous in character, and originate by secondary collenchymatous thickening of parenchymatous cells. The wall always colours blue with Schultz's iodochloride of zinc, but is not coloured by the combined action of phoroglucin and hydrochloric acid, Wiesner's exceedingly delicate test for lignin, which is coloured a fine and intense rose-red by the reagents. Collenchyma swells up but little in water, contrary to the usual opinion, and only contracts about ½ per cent. when deprived of water. Collenchyma may originate from Cambium, from Meristem, or from Parenchyma, but the origin is found to be unimportant. The strength of collenchymatous cells is very little inferior to that of bast fibres, which have been shown by Schwendener to equal that of wrought iron wire.

The last paper is by Albrecht Zimmermann, "On the

Mechanism of the Scattering of Seeds and Fruits, with Special Reference to Torsion." The paper deals with the torsion in the awn of grasses, such as *Avena sterilis* and *Stipa pennata*, torsion of the legume of Orobus and Caragana, the curving and torsion of the awn or beak of the fruits of Geraniaceæ, and the scattering of the seeds of Oxalis, and is illustrated by three plates. The author points out the relation of the different phenomena observed to the mechanical cells in the part, as demonstrated by a microscopical examination of the different structures. Sometimes the cells of the part contract, at other times they swell up, and one or other or a combination of both these causes, gives rise to the effects noticed in the different plants under examination. Thus swelling of the cell-walls causes the remarkable ejection of the seeds of Oxalis. Unequal contraction of the mechanical cells causes the movements in the beaks of Geraniaceæ, and combined contraction and swelling in different layers of cells may be observed in the awns of Stipe and Avena.

In the second part under consideration, namely, vol. xiii. part 3, there is a paper by E. Godlewski, with the title "Contributions to the Knowledge of Vegetable Respiration." The details of a large number of experiments are given which were made, with an ingeniously contrived and simple apparatus, upon the respiration of germinating seeds with both fatty and starchy endosperm, and a smaller series of experiments made on the respiration of the flower buds of *Papaver somniferum* and on the ripening fruits with oily seeds of the same poppy and the castor oil plant. Some of the more important results as set forth by Godlewski himself may here be alluded to. During the early stage of germination in which the seeds swell up by imbibing water, the volume of CO_2 given off equals or is only a little less than the volume of oxygen taken up, both in fatty and in starchy seeds. When the swelling takes place under water or when air is excluded, *intramolecular* respiration takes place. When air is admitted the intramolecular respiration does not immediately cease, but is gradually replaced by normal respiration. As the rootlets of the seedlings are developed the volume of carbonic acid gas evolved gradually diminishes in proportion to the quantity of oxygen taken up, so that at the period of most active respiration only from 55–65 volumes of CO_2 are given off for every 100 volumes of O taken up. The formation of transitory starch during the germination of fatty seeds probably depends upon the action of atmospheric oxygen in each molecule of fat, converting it into CO_2, water, a certain quantity of an undetermined substance, and three molecules of starch. In the later stages of germination of fatty seeds the transitory starch is used as well as the fat, so that the difference between the volume of CO_2 given off and O taken up became gradually smaller, until at last the volumes are equal.

In the germination of starchy seeds the volume of CO_2 given off in all stages nearly equals that of O taken up, in peas sometimes a little more or a little less, but in wheat maintaining a seemingly constant relation of 1 to 1·05, the CO_2 being a little in excess of the O taken up.

In the buds of *Papaver somniferum* the CO_2 given off practically equals the O taken up (100·9 CO_2 for every 100 vols. O). In ripening fruits with oily seeds more CO_2 is given off than O absorbed; in *Papaver somniferum* 150 CO_2 for every 100 vols. of oxygen.

When oxygen is supplied under diminished pressure, respiration is variously influenced in different parts of the plant, but respiration is more affected in fatty than in starchy seeds. When the pressure of the oxygen is very slight *normal* respiration is reduced to a minimum, and *intramolecular* respiration commences. Intramolecular respiration is, under normal conditions, not a primary phenomenon as Pfeffer and Wortmann assert. *Normal* respiration consists in the immediate action of atmospheric oxygen upon the molecules of living protoplasm. Intramolecular respiration only begins when the normal respiration is rendered difficult by the want of atmospheric oxygen. Under ordinary conditions intramolecular respiration only begins when processes of reduction are going on in the plant as when fat is formed from carbohydrates.

The concluding paper in this part is "Contributions to the Anatomy and Mechanism of the Rolling up of the Leaves of certain Grasses," by Dr. A. Tschirch, with three plates. In this paper the author fully describes the mechanism by which such grasses as *Macrochloa tenacissima* (Esparto), *Lygeum spartum*, *Aristida pungens*, and others, which he groups as Steppe grasses, roll up their leaves in dry weather to protect the upper surface which bears the stomata, and prevent too great evaporation.

The first paper in the part and the longest is by the editor, Dr. N. Pringsheim, himself, "On the Function of Chlorophyll and the Action of Light in the Plant." This paper is a controversial one, issued in the form of an open letter to the Philosophical Faculty of the University of Wurzburg. The first part of the paper includes a "personal defence," in which the statements contained in a paper by Dr. A. Hansen, with the title "History of Assimilation and the Function of Chlorophyll," published separately as a "Habilitationsschrift," and also reprinted in Sachs' "Arbeiten," vol. ii. p. 557, are minutely criticised. The second part of the paper is an historical discussion of the theory of assimilation, of the function of chlorophyll, and of the action of light on the plant. In this part Pringsheim does not seem to bring forward any new experiments, but gives a careful *résumé* of the whole subject under three heads. These are (1) Problem of the primary action of light on the cell; (2) the function of the colouring matter of chlorophyll in the exchange of gases in the plant; and (3) the function of the chlorophyll bodies and the primary product of assimilation of carbon. Into the merits of the controversy we cannot enter.

W. R. McNab

OUR BOOK SHELF

Mexico To-day. By Thomas Unett Brocklehurst. (London: Murray, 1883.)

During a recent tour round the world Mr. Brocklehurst turned from the beaten track in the United States southwards to Mexico, where he spent seven profitable months in the capital and neighbourhood in the year 1881. Since the suspension of our diplomatic relations with that country in 1860, great difficulties have been felt in procuring accurate information regarding its internal relations. All the more welcome will be this pleasantly written volume, which gives a far brighter picture of the Republic and its prospects than its most sanguine sympathisers may have anticipated. Since the expulsion of the French in 1867, profound peace has prevailed both at home and abroad, interrupted only by a few feeble and aimless pronunciamientos in the years 1868 and 1869; signs of moral and material progress are everywhere perceptible; security for life and property is being extended from the capitals to the remotest districts of the several states; the whole country is already covered with a network of railways connected in the north with the United States system, and affording several alternative routes between the Atlantic and Pacific Oceans; lastly, the Liberal party, which has guided the destinies of the Republic for over twenty years, has succeeded in establishing free institutions on a firm basis. "I have every confidence," writes our author, "that the favourable terms in which I have spoken of the country will not hereafter be found to be exaggerations; that my ideas as to the future prosperity of Mexico being early realised are true, and that such ideas are held by most of its leading men." And he adds that the time has come for England to bring about "a reconciliation with a country, in whose aid her influence and power could be so beneficially exerted" (p. 259).

The contents of this work, which is sumptuously illustrated by no fewer than fifty-six coloured and other plates from sketches by the author, are extremely varied, special chapters being devoted to the present state of the capital and surrounding districts, to the public institutions, the Roman Church, Protestant missions, trade, manufactures, farm life, the Pachuca silver mines, antiquities, the ruins of Teotihuacan, the remarkable limestone caves of Cacahuampila, Popocatapetl, and many other topics of general interest. During the ascent of Popocatapetl, the traveller ascertained that, according to the latest survey, the edge of the crater was 19,000 feet above sea-level, the usual estimates being 17,850 to 17,880, and that the peak still rose 1000 feet higher. Should these calculations be confirmed, Popocatapetl will again take its place as the culminating point of North America, a position from which it had recently been deposed by Mount St. Elias on the Alaska coast.[1] On the same occasion another curious discovery was made. General Uchoa, present owner of the crater and its rich sulphur deposits, told our author that the eruption of 1521, as described by Diego Ordaz, one of Cortes's captains, must have been due to some misapprehension. All geologists who have lately visited the crater, or who have examined specimens of its minerals, are now convinced that no eruption can have taken place for the last 10,000 years. This is a great confirmation of the opinion now generally entertained that the underground energies diminish steadily in vigour as we proceed from the Southern Cordilleras, through Central America, northwards to the Anahuac tableland. The financial condition of Mexico is described, contrary to the current impressions, as far from hopeless.

Of the numerous illustrations a large number are occupied with curious little clay heads, obsidian knives, stone pestles, arrowheads, and other objects found amongst the debris of the Teotihuacan ruins and elsewhere. There are also excellent reproductions of the famous Aztec Calendar and sacrificial stones, of a beautiful vase from Teotihuacan, of Teoyamiqui, the goddess of death, and of an exquisite vase of Centeotl, or the Mexican Ceres, a perfect gem of Aztec art. Many of the objects brought home by the traveller have been placed in the hands of Mr. Franks of the British Museum, and are no doubt ultimately destined to enrich the Christy collection.

A. H. K.

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[1] On the British Admiralty charts this mountain is marked 14,800 feet, but by the late United States Survey it was raised to 19,500 feet.

LETTERS TO THE EDITOR

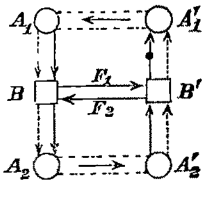
[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Matter of Space[1]

II.

In the aggregations of points which form ponderable bodies, other means exist of suppressing the effects of the points' attractions for each other than the simple counteracting forces of the above figure. Clausius's equation of stationary motion in fact informs us that this will take place when there is no exertion of tractive moment, or no total instantaneous sum of motor-couple actions in the system. This simply appears to imply that the pair of orbs A_1 A_2 and the pair A_1' A_2' are in that case no longer independent of each other in their transference and counter transference of motor energy, but that the twofold action of such energy is then a self-neutralising one; or in other words that the energy given off at A_2 passes on to A_2'; and that discharged at A_1' is taken up by A_1; so that in the case where B B′ are in stationary motion, or combine to form a "sphere" of two gravitating points, or again where many such points collected together form a permanent ponderable body, orb-couples intervene between the otherwise free extremities, $A_1'A_1$ and A_2A_2' of the two ether systems (in the directions shown by arrows in the figure), and bind them together conservatively by an endless circuit of motor energy through the ether-orbs, while a similar endless flow of ordinary momentum through the ponderable channels of the system in the meantime constitute also the usually recognised internal, gometrical, or "lost" forces of such a permanent aggregation, "sphere" or "body" of ponderable matter.



This subjection of two or more baric points B B′ &c. to the condition of stationary motion, as the bond of neutrality of "lost" geometrical or "internal" forces between them differs therefore from the case before supposed of absolute suppression of all interforce between them in this, that when (in the latter case) the motions of the points B B′ &c. are absolutely free and entirely exempted from disturbing force action, the motor-vigours of the couplets $A_1 A_2$, $A_1' A_3'$ &c. of the ether-orbs in permanently bound binary attendance upon the baric points B B′, &c., respectively, will then also be equally exempt and free from disturbing actions of any other orb-couples upon them, than those only by which they are dually and counter-equally bound to each other through the channels of their respective baric centres B B′, &c.

Throughout the whole of a baric point B or B_1's state of undisturbed rest or motion, the ether-couplet attached to it is constantly transmitting from one of its ether-orbs to the other a ceaseless flow of undirected energy, or it is etherially exercising a ceaseless undirected horse-power, whose supply of energy is drawn from and is returned again without mal-destination to the universal ether's general stock of energy, if the meaning of the principle of conservation of energy in this case may be said to be that, for the entire sum of all its parts, the universal ether's whole stock of energy never undergoes any alteration. We may next consider also the case where the interforces between baric points are not entirely absent, but may present us with a resultant algebraic sum of any number of interforces, instead of with a neutral sum only of two equal and opposite ones. Although in that case there is no counter-equality between the motor-couples which act on the ether cortége A_1 A_2 &c. of each baric point B or B′ &c. yet if the motions of these latter points are subjected to no condition of stationariness under the influence of the forces acting on them we may yet recognise the universal ether's whole stock of energy as being the source and destination of all the flows of undirected energy exerted by the collective cortége's couples, if we assume for the whole of the ether together the same obedience to the law of conservation of energy as before, because, for each one of the interforces between B and some other baric point B′, and therefore in the sum of all such mutual forces and points paired with B to produce them, the orb-couples at A_1 A_1' which yield the force F_1 are exactly counter-equivalent to each other, and so will abstract from such a general stock of energy just as much at one of their points of action A_1 or A_1' as they restore to the same general stock of their energy requirements at the other one.

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[1] Continued from p. 460

The presumption here used that the undirected energy funded and effunded by the motor-couples acting on the ether-retinues of the reacting baric points, belongs to an invariable stock of that description of energy residing in the universal ether as a whole, and that it is not extracted from and rendered up to any other imaginable sources, or in other words the theory of the conservation of motor energy in the universal ether by all the motor orb-couples together which are in action in it, acquires an important meaning, when we recur again to the nature, as above explained, of the condition to which these orb-couples are subjected when they act upon the ether-retinue of a collection of baric points which compose a "sphere" or body, or which are together in stationary motion. The description already given of this case informs us that when the state subsists, the simple sum of all the motor actions or tractive couples on the body's retinue of aërilians[1] is either nil at all times instantaneously, or, when it is a periodic sum, its average value for a time-period or recurring time-cycle of its changes is so. No instantaneous resultant can be formed at all, if the sum's value is perceptibly periodic, and it is not in our power to say whether ether orbs of aërilian points originally differentiated from weighty material points (or whether those points themselves) yield sums which have periodic or instantaneous resultants; it belongs to a strict examination of the subject to pronounce and illustrate the rules by which stationary or periodic resultant sums can, in combinations of orbs or aërilian parts, afford by proper means either periodic or stationary resultant actions on a collective aërilian assemblage. The mode of combination of such actions on subordinate parts into a resultant action of one or both kinds on a united group which they compose is certainly not a hopeless problem, when its character is once regarded as the essential problem of etherial mechanics. What free or unbound ether may exist besides the ether enrolled in the retinues of ponderable matter, and what actions these free and enrolled portions of the ether may have upon each other, and separately or together upon the originally sundered multitudes of matter composing the ponderable parts of gravitating bodies, so as (with time as another element of the reactions) to explain the gradual process of condensation which appears to be a perfectly regulated progressive principle of material economy, are all questions which, by a closer discussion of the surmises here explained and indicated, may without doubt be certainly expected to follow from their careful consideration, in due course. But as the phenomenon of stationary motion is shown by Clausius's equation (which states its condition) to be at least a rigidly true absence of average total tractive moment in a system which presents it, when the average is a time-average taken over a sufficiently long fixed or over a proper repeatedly recurring term or movable interval of time, and since, to senses incapable of discriminating exceedingly minute quantities, this time-average becomes an instantaneous quantity when the time-term for which it is reckoned diminishes without limit, a conclusion may be readily drawn from this which will fairly justify us in accepting the presumption used above, that the instantaneous effects of individual motor-couple actions are conserved in the universal ether as a whole. For we are unable to discriminate *what* periodic variations the sum of these effects may or *may not have* in their total value for the universal ether; and we have therefore exactly the same grounds for regarding the instantaneous effects of all motor-couples as being instantaneously conserved by the occult fluctuating terms or periods of the universal ether, as we have for viewing them as instantaneously conserved (so as to give a sensibly stationary zero resultant sum) throughout the parts of a ponderable body's mass in which we cannot detect any periodic motion, or any perceptible vibration.

A bent bow, when its string is released, a soap-bubble or an air-gun's charge, when they give way and burst, or a bubble of hydrogen and chlorine gas mixture when an active light-ray strikes it, ignition of a train of gunpowder by hot iron, or of fire-damp in a safety lamp, of gases and gold leaf by the electric spark, are instances, if we could penetrate the process, of suddenly infringing by a forced vibration the gradually attained subsidence of all perceptible periodicity in a system's inner motions, with instant disintegration for its consequence of the stationariness of the motor-actions of its parts. A little universe in effigy has collapsed, leaving to the universal ether the task of saving and storing up, by means of individual free motor-couples, the vibrations let loose, and of so modelling into something else the scattered fragments.

But on the other hand the resisted jet, as well as the shutting of a water-pipe or steam-boiler valve, the swing of a hammer as well as its stroke on a rock or bell, directed radiations of all kinds as well as their radiometer-like interceptions, the steadily resisted flow as well as the breaking or making of an electric current, conduction between bodies of a steady flow of heat, sound, and all perceptible horse-power exercises of motor-vigour's effective, or unreversible operations, can only be conducted (as the ether does conduct such effective works there conservatively) by the individual periodic actions of unbalanced motor-couples acting on some free-coursing ether-orbs or orb-clouds forming equally free-coursing heavy bodies' retinues. These all rely directly on the universal ether's store of motor energy to maintain in their isolated severance (and in that of the free-coursing bodies also) from other works' and matters' motions a constant conservation of their unreversible motor-activities' effects.

This view of the ether's function as a whole to conserve the individual effects, both of primordial and of resultant motor-couples on ether aërilians and orbs and clouds, whether those couples' intensities are stationary, or fluctuate and vary in any periodical or unperiodical manner, is the second maxim above noted, to be kept in view along with that of description of couples' intensities as a time-rate of a certain kind of energy, in discussing the properties, or the etherial mechanics, of motor-couples' balanced and unbalanced actions. The maxim, as thus laid down, also cautions us against confusing the kind of *instantaneous* energy effects of motor-couples, which the ether conserves as a whole, with any periodically *term-averaged* semi-mean square of a collection of particles' rhythmically fluctuating velocities, or with any temporary or enduring "peræval sum," as it may be called, of the collection's total undirected energy, since the instantaneous undirected sum thus obtained, is not really instantaneous as long as the length of the term or period over which the average is taken is a perceptible and measurable one. The resultant quantity whose effects the ether conserves at every instant, on any individual aërilian assemblage, by a total sum of counter-equivalent quantities acting on other aërilian assemblages or aërilians is the sum (treating every aërilian as of the same inertia $m = 1$)

$$\Sigma \frac{d}{dt}\left(r \frac{dr}{dt} \right) = \Sigma \left(Rr \right) + \Sigma \left(v^2 \right);$$

where, for a single aërilian, r is a vanishingly small distance between at least two parts of which it must consist, and for groups of aërilians the sum also includes, under the general symbol r, the distance between centres of every pair of aërilians possessing, relatively to the assemblage's centre of inertia (just as the aërilian parts do relatively to their aërilian centres), counter equal accelerations, $R, -R$. We are not at liberty in applying the equation to include in its sum any other distances and accelerations, nor any other velocities v, of the aërilians' parts and centres than these barocentric ones, relative to centres of inertia included in the given system, because as an equation of couples having no truth or meaning, except in virtue of its composition of pairs of quantities (so furnished by pairs of inert points contained in the system as to be independent in its sum of the origin of reference used in its formation), all distances, velocities, and accelerations of the given system's centre of inertia cannot form part of the equation conformably to its physical use and applications, but must form part of physical actions in some other system, of which the given assemblage and its centre of inertia forms one individual member.

It is this necessary view of the above equation of stationary motion drawn from such views as those here offered of its physical interpretation, which obliges us to regard the simplest aërilian point of the ether as consisting of at least two parts; and this assumption agrees with the dual view of the ether's nature taken by Professor O. Lodge in his address on the "Functions of the Ether" (NATURE, vol. xxvii. p. 328), while this system also explains the kind of conservation which has been noticed independently by Dr. G. Lippmann and Professor S. P. Thompson[1] as characterising the phenomena of electricity, and the close resemblance which not only exists between the processes of conduction of heat and of electricity, but also, as noticed by the former writer, between the laws of electrical potential and the thermal principle of Carnot's law.

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[1] Those entering into the baric body's actual composition may be left out of the enumeration, since this body's baric motions being themselves (all taken together) stationary, they satisfy the equation of condition identically; and in general instantaneously, unless a common periodic motion is given to the baric points.

It seems scarcely probable that so many converging views can be all fallacious, and ingenuity may without doubt be spent with profit and advantage in further attempts to adapt and reconcile some comprehensive theory of the ether's properties, of a mechanical description, to embrace in a common review all the multiplicity of remarkably analogous laws, which physics in its various branches at present offers to our contemplations.

If the description which precedes of a connected outline of such a system of synoptic views has extended to a much greater length than I was originally prepared to offer as comments on Mr. C. Morris' communication, it is because the logical developments which I have repeatedly found them to admit of induced me to try to establish them on a satisfactory foundation. In many trials, moreover, of their applications, I have met with such frequent proofs of the validity of some such general principles as those here indicated, that the results to which they have easily conducted me in numerous directions, are in general so accordant with those which Mr. Morris has skilfully reviewed, that, save in the small divergence between us, upon which I have dilated, in the main principles adopted for explaining them, Mr. Charles Morris's and my views of the properties and laws of motion, of the distribution of the "Matter of Space," and of the mechanical behaviours of "motor-vigour," are for the most part only *variæ lectiones* of each other. A. S. HERSCHEL

Newcastle-on-Tyne, February 10

P.S.—It will perhaps serve to correct some misconceptions which, although they were not intended to be produced, may yet not impossibly have arisen from the form of defective reasoning, which at some few points of this letter's descriptions it has been unavoidably necessary to introduce, to notice in recapitulation that it formed at the outset no part of its main object to define and represent exactly the extremely complicated part which (at least in combinations of its periods) it is evident that time discharges in determining the operative efficacies and strengths of motor-couples, in exerting "vigour" of undirected motion. With a well-grounded geometrical substructure, there need be no occasion to entertain a doubt that the first principles at least of time's use in definitions of the actions and effects of wrests or motor couples will be easily identified and laid down with all the precision and accuracy needed for purposes of their mathematical applications.

The principal object, however, here aimed at and sought to be attained has not been to offer such an exact description of time's relations of form and economy to the different states of action and repose of undirected motion (which do not actually admit of successful discussion without much more abstract elementary conceptions than those ordinarily recognised of geometrical principles), but simply as a preliminary step towards this question's future surer treatment, to point out clearly and plainly the distinctive and peculiar character of undirected motion's space-relations.

This kind of motion, it has been endeavoured to explain, consists in change of magnitude of a certain ratio-index, ϕ, of tractional configuration between collocated points. In the capacity of a ratio-index ϕ very closely simulates, in its algebraical and geometrical properties, all the analogous properties of angles. But it differs from them in this important particular, that it possesses no directional qualification. For a crank-arm's description of angle at once assigns the plane of the crank's revolution, and this plane has direction; but extension of a connecting-rod is a ratio which affects the rod's length equally in all positions, without giving rise in so doing to any new direction.

On the other hand, neither motion in angle nor in traction-index can by their unaided to-and-fro presence in a crank or connecting-rod give backward and forward motion to a piston or piston-rod, but only by virtue of certain constraints involving the properties in one case of trigonometrical, and in the other case of hyperbolic, sines and cosines. It is probably because changes of angle are, like the changes of the angle's sine which result from them, directed quantities, that the relations of angle-variations to changes of coordinate lines by means of trigonometrical ratios is a familiarly applied and well-established theory. But no such theory having the same scope and extent of application connecting changes of coordinate lines with variations of the ratio-index ϕ, by means of hyperbolic sines and cosines, and showing what necessary conditions directed geometrical quantities (including angle) *must satisfy* to make a fixed law of hyperbolic connection with the undirected quantity ϕ have any possible existence, has yet been brought into general notice and acceptance.

But that the directed and undirected geometrical quantities do satisfy and fulfil such a condition, and that the fixed law of connection between them does actually exist, there is sufficient evidence to assure us in the consistency with which such reasonings as those which Mr. Morris has produced, and which I have tried to base on that geometrical assumption, represent correctly a very large array of physical phenomena. Forming therefore, as the undirected quantity ϕ does, a position-scale in space, of whose possession of certain distinctive and special geometrical and physical properties no theoretical employment has yet been made, and no sufficient proof of satisfactory evidence has, in fact, heretofore been produced, no excuse, it is opined, for hyperbole or figurative use of speech need be presented, for describing the new quantity, as it was termed in a former part of this letter, as a new position-scale of interspheral, ethereal, or aërilian motions foreign to and independent of our ordinary graphic field of space.

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[1] NATURE, vol. xxiv. p. 140; and pp. 78, 164.

Mr. Stevenson's Observations on the Increase of the Velocity of the Wind with the Altitude

I AM sorry if I took Mr. Stevenson too literally when I understood his remark, "great heights above sea-level," to mean *absolutely* great heights; but I certainly think the phrase is extremely liable to such interpretation, and as no superior limit was assigned, I naturally inferred that the author deemed the formula $\frac{v}{V} = \frac{h}{H}$ applicable to such heights as those considered in my paper.

On his own showing, however, this formula succeeds no better than mine on Arthur's Seat; while mine possesses the unquestionable advantage of approximating to the truth throughout the higher levels, where all Mr. Stevenson's formulæ fail according to Vettin's data. If the data furnished from Arthur's Seat correctly represent the conditions in a free atmosphere up to the same level, which I very much doubt; we must infer that the velocities increase in a more rapid ratio with the heights than that given by the formula $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{2}}$, which is preferred by Mr. Stevenson, but in not so rapid a one as that given by $\frac{v}{V} = \frac{h}{H}$; and in fact, if we make the index $\frac{2}{3}$ instead of $\frac{1}{2}$, we get a formula which gives far better results, as far as the Arthur's Seat observations are concerned, than that preferred by Mr. Stevenson. The agreement is so close for nearly all the velocities, that I give below a comparison of the results afforded by both formulæ :—

Velocity recorded at high elevation, 775 feet above sea-level.	Velocities computed for lower station		Velocity recorded at lower station, 550 feet above sea-level.
	By Mr Stevenson's formula $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{2}}$	By the formula $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{2}{3}}$	
885	703[1]	704	720
1,698	1,430	1,351	1,364
2,620	2,206	2,084	2,133
3,416	2,876	2,718	2,718
4,328	3,646	3,443	3,465
5,575	4,697	4,435	4,592
6,763	5,698	5,381	5,640
8,035	6,765	6,393	6,782
9,368	7,893	7,453	7,862
10,820	9,115	8,609	8,765
12,410	10,455	9,874	9 789
13,700	11,542	10,900	10,639
15,058	12,687	11,980	11,680
Sums	79,713	76,325	76,149
Means	6,132	5,871	5,857

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[1] This value is wrong as given by Mr. Stevenson It should be 745

From the above table it is seen that in ten cases out of thirteen, the formula I have proposed gives results closer than Mr. Stevenson's, while the means differ by a quite insignificant amount.

If then, as seems probable on all grounds, the higher we ascend, the slower the increase of the velocities with the heights, Mr. Stevenson's formula, $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{2}}$, should hold for a level somewhat higher than 775 feet, and not below it. Above that, again, a formula, $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{3}}$ should apply; and finally, the formula, $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{4}}$, recommended in my paper.

I cannot believe, however, that the formula $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{2}}$ holds up to such a comparatively large height as this inference would postulate, since it gives such an excessive value at 1600 feet with Vettin's data (more than twice that observed), and I can only conclude, therefore, until experiments in a free atmosphere corroborate Mr. Stevenson's data from Arthur's Seat, that these latter do not correctly represent the actual rate of increase in the velocity between such levels in the atmosphere, away from the disturbing influences of mountains and valleys.

In any case, however, I must enter a distinct protest against having my name prefixed to the pressure formula $\frac{f}{F}=\sqrt[4]{\frac{h}{H}}$. If Mr. Stevenson carefully examines my paper, he will nowhere find the remotest allusion to such a formula. The formula for the velocity which I there recommended for the higher levels, was in fact shown to be directly deducible from Mr. Stevenson's first formula for the pressure, viz. $\frac{f}{F}=\sqrt{\frac{h}{H}}$, to which it is exactly equivalent on the ordinary assumption that $\frac{f}{F}=\frac{v^2}{V^2}$.

Moreover, the paradoxical result which Mr. Stevenson arrived at in violation of this assumption, viz. that the same formula was practically applicable both to force and velocity, is controverted by the conclusion entertained in his letter, that the formula $\frac{v}{V}=\sqrt{\frac{h}{H}}$ agrees best with the recorded results of velocity, and the formula $\frac{f}{F}=\frac{h}{H}$ with those of pressure.

While these two formulæ can hardly be called the *same*, it is somewhat striking to find that on the assumption force varies as (velocity)^2, which is supposed to be annulled by the diminished density as we ascend, they are *identical*.

Finally, Mr. Stevenson has evidently quite misunderstood my allusion to sea-level. When I spoke of sea-level, I simply meant the approximate equivalent to the level of the sea *on land*, as at Berlin, for example, where Vettin's observations were made. When Mr. Stevenson therefore maintains that the velocity of the wind at 100 feet above sea-level over land, is probably not so great as that near the surface over the sea, he entirely misses the point of the argument, which lies in the *relatively* excessive velocity of the wind at 100 feet above, to that near the surface, *over land which lies approximately at sea-level*.

The very fact mentioned by Mr. Stevenson regarding the greater friction encountered by air in passing over land than over water, as well as the results of his experiments, point to a considerable increase in the velocity from the surface to an elevation of 100 feet above it. For the very same reason, I should expect to find a more moderate increase up to the same height over water.

E. DOUGLAS ARCHIBALD

On the Formation of Mudballs

THE letter from Mr. Hart in NATURE, vol. xxvii. p. 483, on the natural formation of snowballs, has recalled to my memory the similar formation of balls of mud.

About eight miles south of Bromley in Kent the soil is clayey, and after rain the country lanes are apt to be very muddy. Some five or six years ago there was a very violent storm of rain, whether or not accompanied with melting of snow I cannot now remember. The steep lanes were in many places regularly scoured with water, and it looked afterwards as though the whole surface had in places been a sheet of water, for the soil was quite washed off and the flints were left bare. After this storm my brother and I noticed in the lanes a considerable number of mudballs, usually almost perfectly spherical, but in some cases with a tendency to a cylindrical shape. They varied in size from small pellets up to four or five inches in diameter. On seeing the first one or two, they looked to us like the handiwork of some boy with an enthusiasm for mud pies, but the number of them, and the fact that they were always found on the slopes of hills proved them to be a natural formation. They were formed throughout of soft clayey mud, and I do not remember finding any nucleus in the middle when we cut them open. We concluded that they were formed by accretion to pellets of mud washed down the hillside and rolling as they went. I have only once since seen a similar ball, and that was in a furrow in a ploughed field in the same country; it is possible that this ball may have been made inside an agricultural roller, although there were no marks on the field of recent rolling and there had been heavy rain. The comparative rarity of the appearance of these balls seems to show that they can only be formed with some precise degree of stickiness of the mud. Closely similar are the marvellously spherical balls of matted vegetable fibre to be found on the seabeach in some places. Sir Anthony Musgrave informed me that on the beach in Australia, I think near Adelaide, he had seen tens of thousands of such balls, all perfectly spherical. It seems rather obscure why the fibres should begin to mat together in such a form as to be rolled by the surf, but the perfection in shape is clearly due to incessant rolling. It is probable that, with a flat bath and some cocoanut fibre or oakum, the process of formation might be watched, but I have never tried the experiment. It is very common to see after rain matted lines of such objects as pine-leaves or the flowers of lime-trees, but I have never seen any apparent tendency to rolling, and such lines are left lying flat after the water has drained off.

G. H. DARWIN

Cambridge, March 23

Snow Rollers

THE phenomenon described in NATURE, vol. xxvii. p. 483, under the title of "Natural Snowballs," is known to British meteorologists under that of snow rollers, and as the latter agrees more closely with the phenomenon, I venture to plead for its adoption.

I believe that the first person who carefully examined their formation was that excellent and venerable observer, the Rev. Dr. Clouston of Sandwick Manse, Orkney, and I am under the impression that he published a description of their formation in an early number of the *Philosophical Magazine*. He has observed them on the lawn at Sandwick more than once, and has always noticed the hollowness at the ends; in fact, he described them to me as resembling ladies' white muffs.

I remember only one instance of their being reported in England, namely in the following letter from the late Admiral Sir F. W. Grey, which appeared in the *Meteorological Magazine* for May, 1876.

G. J. SYMONS

62, Camden Square, N.W.

SIR,—The snowstorm of Thursday night (April 13, 1876) was marked by one circumstance which I have never witnessed before, though it may not be uncommon. It was this:—

On Friday morning I observed that for a considerable distance, and following a regular line, the lawn, to leeward of the house, was strewed with masses of snow like boulders, varying from the size of a snowball to a cubic foot at least, and as the snow melted, a track either straight or curved led up to the large ones, following, apparently, the direction of the wind. I had observed before dusk that the eddies of the wind and the swirls of the snow were very marked, and I have since heard from a friend who observed the same thing, that he saw the snow rolled along by the wind, and forming masses such as I have described.

As I have said, I know not whether this has been observed in other cases, and perhaps it may interest you to have this account of it.—Yours faithfully,

F. WM. GREY

Lynwood, Sunningdale, Staines, April 16

Incubation of the Ostrich

IT seems strange that there should have existed an uncertainty in the mind of an ornithologist as to the mode of incubation of the ostrich in confinement at the Cape of Good Hope. The habits of the birds are of course as familiar to the ostrich-farmers as those of barndoor fowls to ourselves. I have stayed at a farm at Cape Point, where a pair of the birds were nesting within fifty yards of the house, in a small paddock, and have seen the hen on the nest.

An interesting subject of inquiry, however, seems to me to be still open in the matter. It is, How far do the habits of nidification of the ostrich vary in the different climates through which it ranges? The nest of the ostrich is commonly described as a heap of sand, and so no doubt it is in warm desert regions; but the nest which I saw at the Cape was carefully built of grass and other warm materials, so as to aid in retaining heat. The birds kept the nest almost constantly covered between them.

In warmer regions, however, the hen appears often to leave the nest in the daytime, and it is just possible that where the temperature is very high the hen may not incubate at all, and the cock alone may do so at night. I merely wish to point out that it should not be assumed that the habits of the ostrich as to incubation are necessarily the same in the various climates of Africa with those to be observed in the Cape region.

I have noticed that at the Zoological Gardens the ostriches at the breeding season are supplied every year with a cartload of silver sand as the traditional nest. It would not be amiss to try them with some more substantial materials as an experiment, and prove whether in our climate they would not build a warm nest as at the Cape.

That birds' eggs can be hatched like those of turtles in mere sand is undoubtedly a fact. The Megapodius inhabiting Cape York, Australia, makes, as is well known, a huge mound of vegetable matter, which by decomposition supplies the necessary warmth to hatch the eggs; but at the Philippine Islands another Megapodius buries its eggs in the perfectly clean calcareous sand near the seashore.

The habits of the emu in nesting have been carefully watched at Blenheim. The head keeper told me not long ago that the cock alone incubates. The hens lay their eggs anywhere about in the grass, the cock builds a nest, and rolls the eggs to it, the hen sometimes endeavouring to prevent him and to break them. I believe an account of observations on the habits of the emu at Blenheim were published by Mr. Frank Buckland.

H. N. MOSELEY

Bonchurch Hotel, Isle of Wight, March 26

Holothurians

MY experience of about three months in Bermuda and Jamaica fully bears out Mr. Guppy and Mr. Kent's view that the Holothurians do not feed on living coral. They were moderately common in both localities close to the shore, where corals are comparatively scarce, and are mainly of the massive kinds, such as the Astræas, against which the tentacles of a Holothurian would be useless. There were a few branching Oculinas here and there, but not enough to support the Holothurians. But further, some of the latter bury their bodies in the mud or sand, leaving only the tentacles exposed; and I have watched these thrusting their tentacles into their stomachs right up to the base in the comical way described by Mr. Kent. It is quite clear that these were not feeding on living coral. I did not, however, see them actually taking up sand and shell and thrusting it down, as Mr. Kent did; in fact I was puzzled as to what they were feeding on. From the way the tentacles were set, standing nearly erect, I fancied they were catching swimming creatures, as other tentacled animals do. This idea is supported, though not proved, by a fine specimen from the Zoological Station at Naples, which has a half-swallowed fish protruding from its mouth. The specimen is in the Bristol Museum. It proves at all events that they do not reject this kind of food. Possibly in default of it they may fall back upon sand and shell, and the minute organisms contained in these. Some of my experiences with these creatures were interesting. At Bermuda two large kinds used to lie quite exposed in shallow water. I might have guessed from this that they would probably be protected in some way. I used to wade along shore carrying a fishing-basket and a landing-net, and one day, as my basket was full, I put a couple into the landing-net to carry home. As their skins were quite hard, I thought they would travel well so. After handling them, I found my hands smarted a little, and the irritation lasted till bedtime. I found that little bits of their skin had got under mine, and this caused the irritation. As I was going home, I found my Holothurians were literally melting away; long streamers of a colourless gelatinous substance were hanging down between the meshes. Of course I threw the nasty things away, and had a dreadful job to get the net clean. I attributed my misfortune to the sun, so another day I packed a couple up comfortably at the bottom of my basket, which is very closely made. After an hour or two I was horrified to find long streamers hanging down from the basket of the same horrible substance. They had literally gone to pieces again, and spoilt everything in the basket. Shortly after, I left for Jamaica, and there I took out a wide-mouthed bottle and brought one home in triumph. Being engaged that evening, I left the Holothurian in the bottle all night. Next morning the creature was all there, but he had cleared out the whole of his inside; his intestinal canal and the beautiful tree-like organ were perfect. The latter was still alive and was waving about in the water in the prettiest way, and looking remarkably like branchiæ. Some accessory organs along the intestinal canal were exhibiting rhythmical pulsations. Altogether it was a most interesting sight. But my poor Holothurian was only a tube. I did not know at the time that he could grow a complete new inside.

Clifton College J. G. GRENFELL

The British Circumpolar Expedition

SUPPLEMENTARY to the very interesting notice in NATURE (p. 484) of the above expedition, permit me to give a brief extract from a letter recently received from Capt. Dawson, as follows:—"I have heard of a large cavern about a day from this (Fort Rae), which I shall try and explore. There are some eyeless fish that live there, that I hope may turn out to be a new species." I do trust Capt. Dawson may be able to carry out his intention, but he must be heavily weighted with work, in which he appears to take a deep interest. I had long ago been told of this cave and its fish, but had no time to visit it, never having been within one or two hundred miles of the place.

March 24 J. RAE

Meteor

MR MASHEDER'S account in your last number of NATURE (p. 483) of the meteor seen by him at Ashby-de-la-Zouch on March 17, corresponds in some particulars with the inclosed note of one seen by myself on the same evening at Malvern. I am therefore inclined to send it you.

The discrepancies are in the time, which Mr. Masheder states to have been 7.5, while here the meteor passed at 6 56 p.m.; also in his description of "pieces dropping," I noticed no such appearance, but simply the not unusual one of rapidly recurring scintillations in the train.

Great Malvern, March 17, 6.56 p.m.

This evening a bright flame-coloured meteor with a short scintillating train, nucleus the brightness of Jupiter, passed rapidly across the sky. When first seen it was beneath the moon, then shining brightly, and was apparently about the altitude of Betelgeux, at that time nearly 10° past the meridian. It disappeared behind the hills almost due west, but so quickly that it was difficult to determine its course with any exactitude.

Lambert House, Great Malvern, March 25 E. BROWN

Mimicry

SUCH remarkable instances of mimicry as that described by the Duke of Argyll in NATURE, vol. xxvii. p. 125, as occurring in a moth, make heavy demands upon the faith of the non-scientific reasoner, since it seems to him impossible that organic Nature in her "blind groping in the dark" could, under any imaginable combination of circumstances, have succeeded in taking the successive steps requisite to bring her to such a state of perfect adaptation to condition. But the proverbially keen sight of birds, *as at present organised*, is apt to lead to erroneous inferences with regard to the evolution of protective mimicry in their insect prey, seeing that the high development of this faculty now attained by them renders nugatory any disguise that is not almost perfect. The theory of natural selection, however, requires the gradual perfecting of this, no less than of other structural and physiological acquirements; and there is no reason for supposing that the Ornithoscelidan ancestors of the feathered tribes possessed exceptional visual powers, but rather that the reverse was the case; so that it may be concluded that improvement in vision and in rapidity of flight proceeded *pari passu*. This being granted, the initiatory steps of mimicry in the *Lepidoptera* may have been tentative, and well within the competence of ordinary variability.

The above sufficiently trite train of thought has been suggested to me by the consideration of analogous facts known to every angler. Many fishes greedily snap at anything that glistens or is highly coloured, especially if it be rapidly drawn through the water, and the slight additional disguise imparted to artificial bait of this description by a spinning motion renders it very attractive. The highly specialised salmon is easily deceived, and the most killing artificial flies for this fish make no pretence to resemble anything in nature, and are attractive in proportion to their gaudiness. The same is true of his congener the trout, although this fish appears to be somewhat more æsthetic in his tastes; and the most useful artificial flies employed to entice him are mere generalised imitations of his natural food. Indeed, on these grounds no less than on those of anatomy, it cannot be doubted that the *Teleostei*—albeit highly specialised of their kind—have failed to develop that acuteness of vision which their rapid movements would seem to render desirable, and are yet in the stage in which a very imperfect mimicry misleads them; and it is not an unreasonable presumption that birds were once in a very similar condition, from which they have emerged in consequence of the necessity for frequent and abundant supplies of food entailed upon them by their active mode of life. Under these circumstances it must have gone hard with the helpless caterpillar, so toothsome and nutritious, seeing that he could not, like the mature *Phryganidæ* and *Ephemeridæ*, keep out of harm's way by shunning the element inhabited by his natural foe; and hence arose the necessity for his protective modification. How urgent was the need for this is amply shown by the fact that several distinct modes of protection have been enlisted in his defence, viz. cuticular hypertrophy resulting in hairiness, mimicry of the vegetation on which he feeds and lives, and unpalatable flavour; to which has been superadded mimicry of the unpalatable forms by those of good flavour. But even with all this adventitious aid the struggle would probably have proved exterminating to him by reason of the voracity of birds, had not the teeming imago participated in the protective modifications, and thereby been enabled to maintain the balance of supply and demand necessary for the survival of the order.

Wycombe Court, Bucks PAUL HENRY STOKOE

Threatened Extinction of the Elephant

THE threatened extinction of any existing species of plant or animal cannot fail to be matter of real concern to all students of science, who ought to neglect no feasible means for preventing so deplorable an occurrence.

Of the few gigantic mammals still living on the surface of our planet, none possesses more interest and none are more worthy preservation than the elephant. Yet it is an accepted conclusion that the elephant is doomed to extinction, and that within a measurable period of time this majestic quadruped will have suffered the fate of the Dodo. Cannot such a calamity be prevented? Surely the destruction of elephants might be legally controlled (in India, at any rate), and their capture (for domestication) might be limited, as it is well known they never breed in confinement. The continuous rise in the market-price of ivory, and its recent unprecedented scarcity as an article of commerce, are ominous signs, and renders it incumbent on the votaries of science to consider what may be done in the matter. It is no question of mere sentiment—it is of vital importance; and if "ancient monuments, ruins, &c.," are worth protecting, it cannot be denied that so remarkable and interesting a creature as our colossal Pachyderm merits some effort in his behalf.

EDWARD E. PRINCE

United College, University of St. Andrews, March 15

A Curious Case of Ignition

ONE fine morning recently, as two ladies were standing together in the drawing-room of a house in this neighbourhood, smoke was observed to rise from the dress of one of them. This was found to be due to ignition by the solar rays focused on her dress by the lens of a graphoscope which stood on the table. Similar cases of accidental concentration of the sun's rays have, I am aware, been recorded. It would be interesting to know whether any serious fires have thus originated. One can easily imagine circumstances which would favour such results from a simple cause.

M.

Finchley, March 26

SINGING, SPEAKING, AND STAMMERING[1]

I.—SINGING

THE voice, essentially a musical instrument, has only of late been scientifically considered. Even now singing is too much dealt with as an art, and its acquirement as an accomplishment. The professional mystery with which it is surrounded serves no good purpose, and favours empiricism. At ladies' schools the old fiction of what are quaintly termed "finishing lessons" still survives; they often succeed in finishing any prospects the pupil may have had of becoming a singer. Most of the current primers and tutors are ludicrously vague and feeble, many methods are absolutely injurious to the voice; for the improvement of which one ingenious inventor has suggested the use of a false palate, and another the fitting of singers' mouths with a sort of bell-shaped snout or proboscis to act as a resonator. A chorus of such proboscidians on the Handel orchestra would be an appalling sight. The real foundation of our knowledge rests on the researches of Helmholtz on the physical, and of Garcia on the physiological, side. The classical discoveries of the former as to the production of vowel-sounds by the superaddition of a varying harmonic in the mouth-cavity, and of the latter by the examination of the larynx in action by means of a mirror, brought before the Royal Society in May, 1855, have formed the substratum of much which has now become the common property of scientific men. Dr. Bristowe, in his Lumleian lectures of 1879, has added some pathological data of considerable value, and Dr. Walshe, in his "Dramatic Singing, Physiologically Estimated," has touched on points connected with the sympathetic and emotional power which this most perfect of instruments can be made to exercise. It owes this in a great measure to the fact that it can combine musical sounds with significant words, and thus interest at once the ear and the intelligence. After a demonstration of the action of the larynx and fauces in phonation, illustrated by some excellent photographs taken from his own larynx by Mr. Emil Behnke, and thrown on the screen, vowel sounds were shown to be thirteen in number in the English language, with six more in French and German, fifteen of these being oral in origin, and four, all French sounds, nasal. Consonants were about sixteen in number, and had been called "noises" by Max Müller, owing to their comparatively unmusical character. They are chiefly caused by some check or obstruction to the laryngeal note. A diagram of Madame Seiler's was, however, shown which indicates that there is an oral resonance-note even for consonants, though it is much more obscure and uncertain than that of the vowels. Melville Bell's division of vocal sounds into vowels, consonants, and glides or semivowels was adverted to, and his ingenious device of visible speech briefly explained, but left for fuller consideration in the second lecture. The contrast was then pointed out between singing, in which the musical notes predominate and are separate or discrete; intoning, which is speech intentionally rendered monotonous for better transmission in large spaces like cathedrals; recitative, which is the converse of the former, being singing partially loosened from the trammels of time, rhythm, and melody, so as to approximate to speaking; speech itself, which uses continuous inflection; declaiming, which is speech with the addition of a histrionic and emotional element; reading, which is a faint and as it were distant reproduction of speaking in a lower key, quieter and less marked in accent than in speaking *vivâ voce*; and whispering, which is purely oral, without a laryngeal ground note, and which may be termed voiceless speech.

The different qualities, compass, and register of voices were then described. The larynx of the child, like its head, is large relatively to the rest of the body. At the age of fourteen or fifteen, rather earlier in girls than in boys, the vocal apparatus enlarges and strengthens. In boys the vocal chords about double in length; in girls they increase from five to seven. In the latter case the pitch of the voice is not materially altered; in the male it usually descends an octave or more.

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[1] Abstract by the Author of three Lectures at the Royal Institution, by W. H. Stone, M.D.F.R.C.P.

Garcia adopted the division of Registers into three, namely, the chest, falsetto, and head voice, due originally to Muller. This remains the most practical mode of classification, though the word falsetto is misleading, being liable to confusion with the artificial male voice bearing the same name, and may well be replaced by the phrase Medium. The term register has been enveloped in much professional mystery, and has been far too much refined upon. There has also been a confusion of octaves, from which even Madame Seiler is not free, due mainly to the modern and objectionable method of scoring music for the tenor voice in the soprano clef, and an octave too high. Register evidently marks an alteration of mechanism in the voice-reed and resonator to enable it to obtain the very remarkable compass, amounting to nearly five octaves, of which the human voice is possessed. Single voices run to three octaves or more. Catalani had 3½; Bastardella, heard by Mozart in 1770, had the same. Madame Carlotta Patti can reach G♯ in alt. Bennati, a tenor, had three full octaves, and Tamberlik reached the C♯ of 544 double vibrations.

The words Head and Chest obviously only represent subjective sensations which accompany the shifted mechanism. In many parts of the voice similar notes can be reached in two registers, but with different force and quality, on either side of the break

In using chest-voice the vibration can be seen to involve the whole vocal chord and the arytœnoid cartilages. At about A in the male and C in the female the chords act alone, though the first mechanism can by an effort be continued. The second form of vibration takes the voice up to F, the usual limit of bass voices and of the chest register. Above the F the chords are stated to lengthen, giving by a second elongation the second series of the chest register, which forms the bulk of the tenor compass, the remainder being formed by a variable number of falsetto notes. These seem to be produced by a thinning of the edges of the chords. Czermak lighted the larynx of thin persons strongly from the outside, and found that sufficient light was transmitted to show a decided increase of transparency in the chords at this point. All observers agree in placing this change, both in males and females, between F and F♯. In this region, common to both males and females, an amusing experiment can be made by causing a tenor male and a contralto female singer to execute the same passage behind a screen, or in an adjoining room. It is difficult, and at times impossible, to discover the sex of the singer from the quality of the tone. There still remains among male voices the curious and only partially explained counter-tenor. Sometimes by arrest of development or by accident the boy's compass is retained in after-life. This accident may be quite independent of masculinity, as those who have heard lusty, rubicund Yorkshiremen, with their wives and children round them, trolling out a sweet treble in glees on the terraces of the Crystal Palace after the Handel Festival, can testify. But besides this rare accident, most basses and baritones can cultivate an artificial and peculiar voice which most properly bears the name falsetto. Its production appears to be in great measure a matter of education. It was seemingly commoner in the madrigalian epoch and in the time of Queen Elizabeth than it is now. Dr. Bristowe says truly that the mechanism of its production is still doubtful, though many attempts have been made to determine it. Such voices are not only artificial, but complex and uneven, being a compound of high chest notes and others of special quality. There is a serious break between the two both in production and in quality, which practised singers disguise by running the one into the other at different places, according as the passage to be sung ascends or descends.

It will have been seen that female voices overlap the compass of the male voice by an octave or more. Many contraltos take the E on the bass stave, which is well in the middle of the bass voice, and a low note for a tenor singer. Hence we sometimes hear of female tenors, though the effect is usually more peculiar than pleasing. Our great English contralto, Madame Patey, however, drops to this note with fine effect in Handel's oratorio of Solomon, which was written for the exceptional and now fortunately obsolete voice of Farinelli.

In females the break is somewhat higher than in males, but the transition to the falsetto takes place at the same note G. The contralto does not use the head-register.

This, otherwise called the Small, begins as just stated. Its upper limit varies, the extremes having been already given. Mozart wrote the fine air *Gli Angui d'Inferno* in the "Magic Flute" for such an exceptional voice reaching to F in alt. A commoner and perhaps pleasanter limit is the C below this.

All authorities agree in describing a curious appearance of the glottis in singing these notes. This is a folding together of its posterior half with vigorous vibration of its anterior part. Such an appearance can only be produced either by some stopping of the chords at the middle by contact with structures lower down, or by overlapping from vigorous approximation of the arytœnoid cartilages. The former supposition lacks anatomical confirmation, and the latter, which is anatomically possible, has the implied, though not the expressed sanction, of Helmholtz. The drawing of this appearance is given by Madame Seiler, who alone of laryngoscopists, possesses a register peculiar to the female.

Dr. Stone was materially assisted in his first lecture not only by Mr. Behnke, but also by his colleague Dr. Felix Semon, who gave admirable demonstrations of the healthy larynx, as seen in Mr. Williams, and some other pupils of St. Thomas's Hospital.

ACCLIMATISATION OF EDIBLE MOLLUSKS

A RECENT and interesting notice by Mr. F. P. Marrat of Liverpool, who is an excellent conchologist, mentions the introduction into the Cheshire coast of what he calls the "wampum clam," or *Venus mercenaria* of Linné; and he concludes that there is "a fair prospect of the naturalisation, on the extensive shallow shores of Lancashire and Cheshire, of an extremely nutritious and highly esteemed food-product, new to Great Britain." The late Prof. Gould says that this mollusk is known in Massachusetts under the name of "Quahog," given to it by the Indians. According to him and other American writers on the subject, the true "clam" *par excellence* is *Mya arenaria* of Linné. I was present as a guest at one of the fashionable "clam-feasts"; but the muddy flavour derived from the habitat of that mollusk does not agreeably commend itself to my palatable recollection. However, *chacun à son goût! Mya arenaria* inhabits the western coasts of the North Pacific as well as both sides of the North Atlantic.

The American oyster (*Ostrea virginica* of Gmelin = *O. borealis* and *O. canadensis* of Lamarck) is peculiar to North America, and has now found its way into the London market. It differs from the common European oyster (*O. edulis*, L.), and is equally variable as regards size. *O. virginica* has been within the last few years introduced into the mouth of the Tagus, and is called the Portuguese oyster. Our own or "native" oyster was highly esteemed by the Romans, as we know from Juvenal; but there are no grounds for imagining that it was in those times imported into Rome from Britain. The facility of transport was not then so great as it is at present; and a gamy flavour was probably not so much relished by the Romans as it is said to have been by our King George the First, who preferred oysters a week old at Hanover to those which he afterwards got in England.

Within the last few years the "periwinkle" (*Littorina litorea*, L.), which is a favourite delicacy of our poorer classes, has spread with unusual rapidity along the eastern shores of the North American continent. Mr. Arthur F. Gray, in *Science News* for April, 1879, attributed its origin to Europe. It certainly does not seem to have been observed in America by Gould or any other conchologist before 1870.

Preeminent among land shells, as a dainty article of food in France, is *Helix pomatia*, L. We are more fastidious or more conservative in our gastronomic notions. It is a mistake to suppose that the Romans, when they possessed and inhabited Great Britain, brought this snail with them to indulge their luxurious tastes. In all probability it was not even known to them, because another species (*H. lucorum*, Muller) takes its place in Central Italy. *H. pomatia* has not been found at Wroxeter or York, or in any other part of England or Wales where the Romans built cities or had important military stations. Among the debris of an extensive Roman villa discovered in Northamptonshire, in which the shells of cockles, oysters, mussels, and whelks abounded, not one of *H. pomatia* occurred, although at Woodford, a few miles distant, that species is plentiful in a living state.

J. GWYN JEFFREYS

THE ALFIANELLO METEORITE

SIGNOR DENZA, Director-General of the Italian Meteorological Association, sends us an account of the remarkable aerolite which fell in the province of Brescia on February 16, and to which we referred last week. On that date, at 2.43 p.m. local time, a strong detonation was heard in many places of the province of Brescia and even in the neighbouring provinces of Cremona, Verona, Mantua, Placenza, and Parma. The detonation was quite awful in the commune of Alfianello, in the district of Verolanuova, Brescia. This was found to be caused by a meteorite which exploded a few hundred yards above Alfianello. A peasant saw it fall in the direction of N.E. to S.W.; or, more exactly, N.N.E. to S.S.W., at a distance of about 150 yards. When the meteoric mass fell to the earth, it produced on the ground, in consequence of the transmission of the shock, a movement similar to that of an earthquake, which was felt in the surrounding districts; the telegraph wires oscillated and window frames shook. Before the meteorite fell a confused commotion was seen in the sky, and immediately after a prolonged noise was heard similar to that of a tram moving rapidly along the rails. No light was seen; but the meteor must have been accompanied, as usual, by a light vapour, produced by the volatilisation of the substance melted at the surface; for some of those who saw it fall compared it to a chimney falling from the sky, and surmounted by a wreath of smoke. The meteorite fell in a field about 300 yards south-west of Alfianello. It penetrated the ground obliquely, nearly in the same direction as it was seen moving in the air, from east to west, sinking to the depth of about a yard, deducting the height of the meteoric mass. The peasants who saw it fall and who were the first to touch it, found it somewhat hot. The meteorite fell entire, but unfortunately was soon broken to pieces and carried away by the crowd who gathered to see the strange sight. The form was ovoid, but a little flattened at the centre; the under part was broad and convex, presenting the form of a cauldron; the upper part was truncated. The surface was covered with the usual blackish crust, and studded with small concavities, partly separate, partly grouped together.

As to the dimensions and weight of the aerolite, the estimates differ. According to the evidence of some, its height was about 75 centimetres, greatest breadth 60 centimetres, and its volume about 25 cubic decimetres. Its weight has been variously estimated at 50, 100, 200, and 250 kilograms. Its real weight was probably not much under 200 kilograms. It is certain that Prof. Bombicci carried more than 25 kilograms to Bologna, to add to the rich collection of meteorites in the Mineralogical Museum of the University; that a specimen weighing 13½ kilograms was taken possession of by MM. Ferrari, the owners of the field in which the meteorite fell; that about 40 kilograms remained in possession of other persons; that the municipality of Alfianello sent a specimen of 5 kilograms to the Athenæum of Brescia; and that two pieces weighing 12 kilograms each were thrown into a stream and lost; without speaking of a considerable quantity of small fragments, distributed here and there, of which Signor Denza possesses four, of a total weight of 39 grammes.

By its structure the Alfianello meteorite belongs, according to Prof. Bombicci, to the sporasiderite-oligosiderite group, being almost identical with the New Concord (Ohio) meteorite. The substance is finely granulated, of an ashy grey; the bright glossy surface has elements showing varied gradations of colour. Metallic particles abound; they are found scattered like small nuclei, in which are iron and perhaps one of its alloys, in brilliant crystalline aggregations, of a yellowish or bronze white. Circles of rust of a yellowish brown rapidly form around the particles of iron. In the places where there are no metalliferous nuclei, the grains of iron are attached to the stony portion in the proportion of 68 per 1000 of weight. The blackish crust is rough, and to some extent rugged in some parts of the surface, and rather smooth and uniform in others; in general it is somewhat lustrous. The total specific weight of the stone is from 3·47 to 3·50. The chemical analysis of the meteorite is being made in two different laboratories at Bologna. Signor Denza's information has been obtained from Prof. Bombicci of Bologna University, and from Professors Briosi, Ragazzoni, and Casali of Brescia.

THE SHAPES OF LEAVES[1]

IV.—*Special Types in Special Environments*

FROM the previous papers it will be clear that degree of subordination to the stem accounts in large measure for the extent to which leaves vary from the primitive ovate-lanceolate type. Where they are still so most subordinated, there will be a strong tendency towards the long pointed ribbon-like form, and also a marked inclination towards decurrence. This combination of peculiarities is well seen in several thistles, and in comfrey, as also to a less extent in many epilobes and stellarias. Compare *Verbascum thapsus*, and other mulleins. From these extreme cases, in which leaf and stem are not fully differentiated from one another, one can trace several gradations, through square stems with sessile leaves (as in certain St. John's worts) up to merely sessile stem-leaves, or leaves that clasp the stem with pointed or rounded auricles. Wherever lines exist along the stem, they may be observed in pairs up to the point where a leaf is given off, and they are undoubtedly surviving marks of the primitive unity of stem and leaf. The same may be said of rows of hairs, like those of *Stellaria media* and of *Veronica chamædrys*. There can be little doubt that special selective causes (protection against creeping insects, &c.) have often come into play in preserving or modifying such decurrent wings, stem-lines, auricles, clasping stipules, and rows of hairs; but as a whole they nevertheless point back distinctly to the origin of dicotyledonous stems from superposition of leaves and midribs upon one another. They are rudimentary forms of stem-lamina.

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[1] Concluded from p. 495.

Sessile leaves are particularly apt to be lanceolate. They approach nearest among dicotyledons to the monocotyledonous type. The botanist will readily fill in examples for himself.

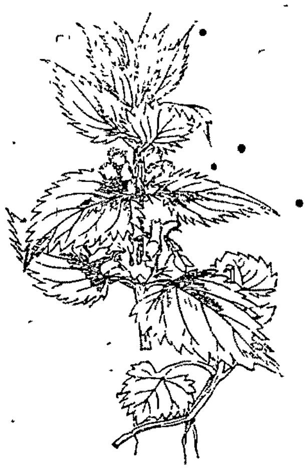


FIG. 34.—White Deadnettle (*Lamium album*)

On the other hand, it is clear that the conditions under which leaves assume the orbicular and peltate types can only occur where there is least subordination to a central stem. And these conditions must have occurred for immense numbers of generations in order to overcome the ancestral tendency towards the lanceolate or ovate form. For a leaf must first pass through a cordate or reniform stage, like that of the coltsfoots, before it can reach an orbicular shape, like that of our common waterlily; and even when it becomes completely circular, like the *Victoria regia*, it may still retain a mark of junction where the overlapping edges have met without becoming connate. In the case of *Victoria regia* the transformation has been traced during germination. The first leaves produced by the young plant are linear and submerged; the next are sagittate and hastate; the later ones become rounded, cordate, and orbicular; and even when they assume the peltate form, the line still marks the point of union. This sufficiently accounts for the rarity of perfectly peltate leaves, such as those of *Tropæolum*, *Hydrocotyle*, and *Podophyllum*. Radical leaves growing on long footstalks will be oftenest orbicular cordate; stem-leaves on the same plant may pass from ovate-cordate to ovate, lanceolate, and linear. Large cordate radical leaves will be most frequently produced from perennials with richly-stored rootstocks. The sagittate and pointed leaves of *Arum* and *Sagittaria* show the furthest step attained in the same direction by monocotyledonous foliage, starting from the liliaceous form.

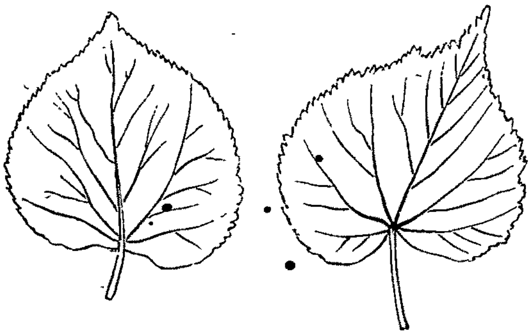
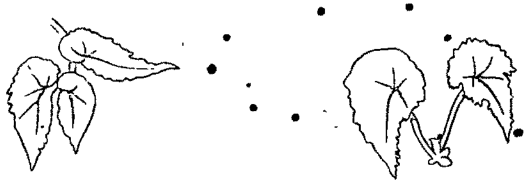


FIG. 35.—Lime.

Where the stem, or, what comes practically to the same thing, solitary ascending branches, rise high into the air, especially with opposite leaves, we get a common type which may be well represented by the white deadnettle (Fig. 34). Hedgerow plants with perennial stocks frequently assume this type. It reappears almost identically, under the very same conditions, in so distant a group as the true nettles; and though it is possible that the causes which produce mimicry in the animal world may here have come somewhat into play, so as to modify sundry *Lamiums* into the similitude of the protected *Urtica*, yet the analogy of other Labiates shows that the circumstances alone have much to do with producing the resemblance. For a great many tall-stemmed hedgerow Labiates closely approximate to the same type: for example, *Lamium galeobdolon*, *Ballota nigra*, *Galeopsis tetrahit*, *Stachys silvatica*, and *S. palustris*. Compare, *mutatis mutandis* for ancestral peculiarities, the other hedgerow plants, *Scrophularia nodosa* and *Alliaria officinalis*. On the other hand, notice the orbicular long-stalked lower leaves of the latter (especially when biennial) side by side with the lower leaves of some Labiates, such as *Nepeta glechoma*. Indeed, the Labiates as a whole present an excellent study of local modification in an ancestral type, according to habit and habitat. Take as other groups of this family the following: first, *Mentha* and *Lycopus*; then, *Salvia pratensis*, *Prunella*, *Marrubium*, radical leaves of *Ajuga reptans*, and lower leaves of *Nepeta glechoma*; finally, the typical form dwarfed in little prostrate retrograde types, such as *Thymus serpyllum* and *Mentha pulegium*. Compare these last with other prostrate or dwarfed types elsewhere, like *Veronica serpyllifolia*, *Peplis portula*, *Hypericum humifusum*, *Montia fontana*, and *Arenaria serpyllifolia*.



FIGS. 36 and 37.—Begonias

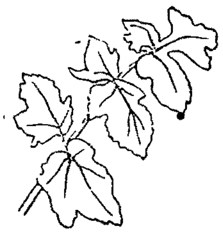


FIG 38.—Cow-parsnip.

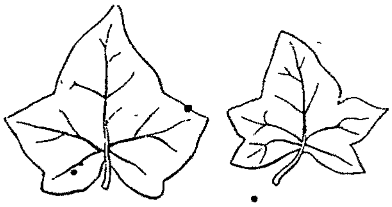


FIG. 39 —Creeping leaves of ivy.

As grassy types, the best familiar examples are those of the flaxes, *Stellaria graminea*, Toadflax, Bastard Toadflax, &c.; all of which have been largely influenced by monocotyledonous competition. Even a pea, *Lathyrus nissolia*, has got rid under such circumstances of its leaflets, and has flattened its petiole into a grass-like blade. Intermediate forms occur in Southern Europe. The peas, indeed, are papilionaceous plants which have largely cast off their ancestral leaf-type, in order to avail themselves of new conditions. *L. aphaca* has lost its leaflets, and flattened and enlarged its stipules so as to resemble simple opposite leaves; and *L. hirsutus* and *pratensis* have reduced the leaflets to one long almost linear pair. Marshy plants have also often been forced into adopting grass-like forms. The great spearwort is a swampy buttercup, whose ancestral leaf has been lengthened out into a long ribbon, with almost parallel ribs; the lesser spearwort shows the same tendency to a less degree, still retaining ovate lower leaves, with lanceolate upper ones; and *Veronica scutellata* is a similar marshy case among the Scrophularineæ.

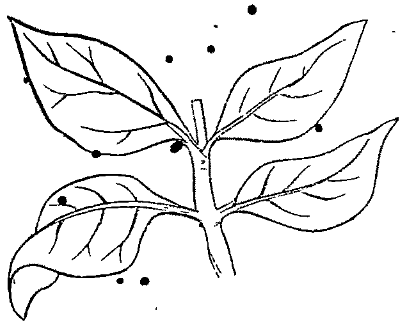


FIG 40 —Ascending leaves of ivy

When the tree-like form is attained, or free access to air is otherwise gained (as by climbers), the supply of carbon, being practically unlimited, becomes relatively little important, and the supply of sunlight assumes the first place in the economy of the plant. Under such conditions, the great object must be to prevent the leaves from overshadowing one another. Now this result may be obtained in a great number of ways, and we must not expect that every tree or shrub will solve the problem for itself in exactly the same fashion. It is enough that the shape into which the ancestral form is finally modified should sufficiently answer the purpose in view. As a matter of fact, the suitability of the actual forms and arrangements of tree-leaves to the functions they have to perform can be readily tested by observing any tree in bright sunshine. On the one hand, almost every leaf is in full illumination, no leaf unnecessarily shading its neighbour; and on the other hand, there is hardly any interspace between the leaves, as may be seen by the fact that the shadow thrown by the tree as a whole is almost perfectly continuous. In short, there is no waste of chlorophyll, and there is no waste of sunshine.

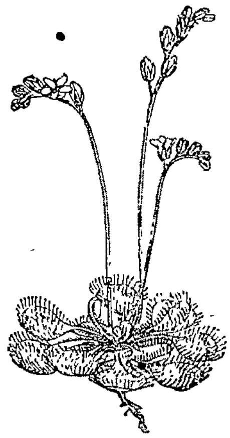


FIG. 41.—Sundew.

Mr. Herbert Spencer has called attention to the results of varying exposure to light in the various parts of the same leaves, which often causes them to become unequally developed. In the lime (Fig. 35) such obliquity is normal. In the various *Begonias* (Figs. 36 and 37) the resulting asymmetry is very noticeable. In the cow-parsnip (Fig. 38) it is the leaflets of the same leaf which are asymmetrically developed, so as not to overshadow one another. In more symmetrical leaves, there is an equal provision for preventing overshadowing, only here it takes the form of indentation of the edge, as in the oak, or of subdivision into leaflets, as in the horse chestnut. In the latter case, indeed, the two outermost leaflets are habitually asymmetrical. On the whole, however, the mass of forest trees in temperate climates have almost entire leaves; and full exposure to sunlight is secured rather by their special specific arrangement at the end of the minor branches. Most often they are more or less ovate, as in the elm, beech, alder, birch, and poplar. Where the leaves are divided, the separate leaflets assume the appearance of almost entire leaves: compare the leaflet of the horse chestnut with the leaf of the true chestnut; the leaflet of the ash with the leaf of the hornbeam; the leaflet of the walnut with the leaf of the beech; and the leaflet of the mountain ash with the leaf of the blackthorn. In all these cases, almost identical results are practically produced in the end by similar circumstances acting upon wholly unlike original types.

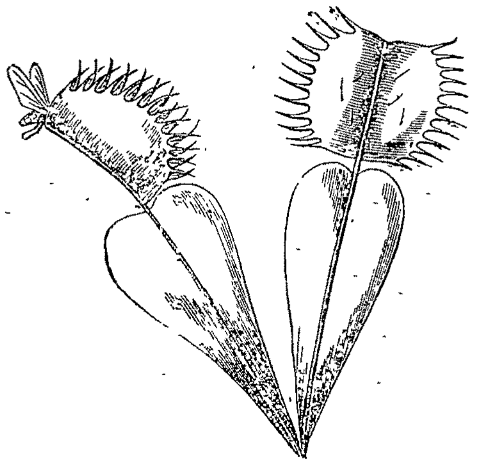


FIG. 42 —Dionæa.

Some minor typical forms exist in certain groups of climbers, which are worth a moment's notice. Take as an example the creeping leaves of ivy. As long as this plant grows close to a wall or the trunk of a tree it assumes the well-known shape shown in Fig. 39. But as soon as it branches out its flowering sprays into the open, acquiring a tree-like habit, which it often does on the top of a wall, it takes a simpler and totally different form of leaf, as shown in Fig. 40, growing on the same plant. This last type is quite comparable to that of the pomegranate. That both types admirably suit their particular situation can easily be seen by noting how well they fit in with one another without overshadowing. It would be difficult to point out the geometrical grounds for this relation, but the relation itself becomes obvious on watching an ivy-plant in broad sunshine. Moreover, the first or truly ivy-like form of leaf tends to recur among many plants which similarly press close to a flat surface. In *Veronica hederæfolia* we get it in a weed that climbs over banks of earth; in *Linaria cymbalaria* we get it in a trailer hanging upon stone walls; in *Campanula hederacea* and *Ranunculus hederaceus* we get it in a creeper along the edge of rills or over soft mud. Compare in each case other forms of the typical generic leaf, as seen in germander speedwell, toadflax, harebell, and meadow buttercup.

Another special climbing type, proper to more open habits of twining round alien stems, is that of the common bindweed. This, the ordinary convolvulus form, reappears exactly in so distant a plant as *Polygonum convolvulus*, whose habits are exactly similar. Even among monocotyledons we get it closely simulated by *Smilax*, with precisely like conditions, and somewhat less closely by *Tamus*. Indeed, this form of leaf may be said to be almost universal among lithe twining creepers.

The hop type belongs rather to mantling than to mere twining climbers. It reappears under identical conditions in the vine, and less closely in true bryony. More subdivided into leaflets, it produces the Virginia creeper, and many forms of clematis.

Among ground plants, it is only possible very briefly to refer to the succulent types which abound in dry situations. A regular gradation may here be traced from rich forms with rather thin, flat, ovate leaves, growing in favourable situations, like *Sedum telephium*, through dwarfish forms, with oblong leaves, like *Sedum album*, to forms with knobby, globular leaves, growing in very dry spots, like *Sedum anglicum*. Where the stem becomes very succulent, the leaves may be dwarfed out of existence altogether, or reduced to prickles, as in those dry desert plants, the cactuses. Compare some tropical Euphorbias. Miscellaneous examples of these dry types are also found among Mesembryanthemums and other Ficoideæ, natives of hot, sandy plains in South Africa. The succulence here acts as a reservoir for water. Special precautions are taken against evaporation. We see the first symptoms of such a habit in some English dry-soil saxifrages.

Proximity to the sea, whether the plant grows in sand or mud, also tends to produce succulence. This effect is seen casually in many seaside weeds, and habitually in such cases as samphire, *Inula crithmoides*, *Spergularia rubra*, *Cakile maritima*, and common scurvy-grass. *Suæda maritima* is in this group the exact analogue of *Sedum anglicum*, while *Salicornia* is similarly the analogue of the leafless cactuses. Compare also *Salsola kali*. There is a somewhat similar tendency to fleshiness in certain freshwater weeds of moist spots, such as *Chrysosplenium*, and many saxifrages.

In such a brief sketch as the present it is impossible to do more than allude in passing to sundry more special developments of leaves, for protective or other purposes. One development of this character is seen in the growth of prickly tips (*Agave*, *Aloe*, *Salsola*, *Juncus acutus*, *Bromelia pinguin*), or of prickly edges (thistles, *Carlina*, holly, *Stratiotes*, *Dipsacus*, *Rubia peregrina*). Such prickles may be purely defensive, or they may assist the plant in clambering (*Stellatæ*, *Smilax*, hop). Again, the leaf as a whole may be reduced to a prickle, as in gorse, where the very young seedling has trefoil leaves like its allies: but these give way gradually to entire lanceolate blades, and finally to mere thornlike spines. Another very different development is that of the insect-eating plants, which grow in very boggy spots, and so require animal matter not yielded them by the roots. Our English sundew (Fig. 41) is an example of the first step in such a process; essentially its leaves belong to the obovate tufted or rosetted type represented by the daisy, only a little exaggerated; but they have been specialised for the insect-eating function by the evolution of the little glandular hairs. Even simpler is the type of the butterwort, which belongs to the same foliar class as the London Pride, *Draba aizoides*, *Samolus Valerandi*, *Sempervivum tectorum*, &c., but with the edges folded over so as to inclose its insect prey. From these simple forms we progress at last to highly specialised types like *Dionæa* (Fig. 42), *Sarracenia*, *Darlingtonia*, *Nepenthes*, and *Cephalotus*. Once more, the connate form in opposite leaves (*Dipsacus*, *Chlora*) or the perfoliate in alternate ones (*Bupleurum*) may be due, as has been suggested, to the facilities these arrangements afford for storing a little reservoir of water, which acts as a moat to protect the flowers from climbing ants. But such minor selective actions are too numerous and too diversified to be noticed in full here; it must suffice to point out the general principles upon which the forms of leaves usually depend, leaving the reader to fill in the details in every case from his own special observations.

GRANT ALLEN

FOSSIL ALGÆ[1]

THE publication of Saporta and Marion's "Evolution of the Cryptogams" (see NATURE, vol. xxiv. p. 75, 558) has been followed by a work in which Dr. Nathorst has endeavoured to prove that nearly the whole of the supposed fossil marine Algæ, especially from the older rocks, are either tracks of Invertebrata or were produced by mechanical agency. "Florideæ, Laminarieæ, Chondriteæ, Alectorurideæ, Arthrophyceæ, Bilobites, and other algæ; comprising among them forms curious and remarkable by the regularity of their branching thallus, their phyllome with raised periphery and striated surface; all had disappeared as if by enchantment, and in their place there remained but tracks of Invertebrata, moving upon the ooze, swimming or creeping, and impressing the extremities of their tentaculary palpæ around them, or of larvæ gliding through the slimy mud." When these are insufficient, the movement of water acting on inert bodies, or waving tufts of sea-weed, are appealed to, for no fossil imprint either sunk or in relief, unless preserving carbonaceous matter, is admitted in Dr. Nathorst's hypothesis to have ever been a plant. This view is energetically combated by Saporta in the present work. The issue however does not very materially affect either the general theory of plant-evolution, as traced by Saporta and Marion, since this relies but little upon the evidence of doubtful fossil algæ, or the succession of marine algæ in time, which seems to have been probably Laminarieæ, Fucaceæ, and Florideæ. The main point in dispute is whether the supposed primordial algæ, Eophyton and Bilobites, are of vegetable or of other origin. There are numerous *a priori* reasons for supposing plant life to have existed in palæozoic seas, and the complexity of life seen in even the older rocks renders their presence almost a necessity. The question is whether certain impressions which are abundant in Silurian rocks reproduce some of these forms, or whether we are still without indications of the primæval algæ.

Dr. Nathorst appears to rely very greatly upon the fact that many of these supposed sea-weeds are marked in relief upon the under-sides of slabs, proving, as he supposes, that they are the filling-in of furrows, and also upon the very general disappearance of all trace of carbon. In denying the plant-origin of certain impressions lately described as algæ by Prof. Walter Keeping in the *Geological Magazine*, he lays particular stress on the former hypothesis. Saporta however devotes two or three pages to clearing up this, as he believes, misconception. The fact that very unmistakable impressions of even terrestrial plants do occur in this condition, is known to most collectors of them, and is explained by the author as follows:—A plant-stem of sufficient substance to resist pressure, but destined in the long run to decompose, would, if resting on the sea-bottom, become covered with sand or silt, if such deposit were taking place. (*a.*) As the weight increased above, its under-surface would become pressed into the bed upon which it chanced to be resting. (*b.*) As it decomposed, infiltrated sediment would replace the organic matter (*c.*), until finally the decomposition being complete, the sediment from above entirely fills in the space, leaving on the under-surface a reproduction in semi-relief of the decayed organism, while the upper part is merged in the sand.

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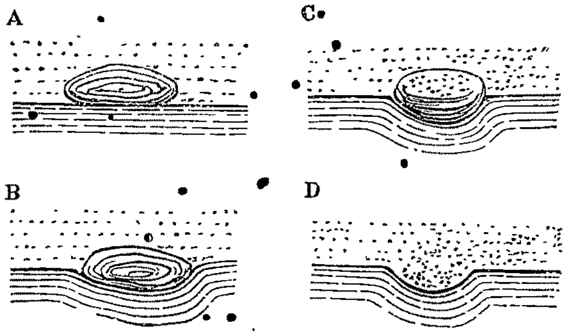
[1] "À propos des Algues Fossiles." Le Marquis de Saporta. (Paris. G. Masson, 1882.)



(*d.*) Instances of this form of fossilisation are by no means rare, but cases in which all carbonaceous matter has disappeared from vegetable impressions are still more common, especially with sea-weeds, which, as M. Grand 'Eury has remarked, decompose into a semi-fluid gelatinous matter when imbedded in mud. Nor does the destruction of carbon cease when the mass they are buried in becomes consolidated, for percolating water brings oxygen to them, which slowly destroys every remaining vestige of organic matter.

The author is careful in the present work only to select specimens for illustration about which little or no reasonable doubt can exist. Commencing with impressions from the Tertiaries of almost existing species of sea-weed, he compares these with the more doubtful secondary Chondrites. The Chondrites of the Flysch, strongly impregnated as they are with carbonaceous matter, are admitted on all hands to be Algæ, and the author asks how the same origin can be denied to casts of specifically identical Chondrites of the Cretaceous, and so on to the Liassic forms. The algous nature of most of those selected for illustration is indeed so obvious that no shadow of doubt respecting them can exist. The gigantic Liassic Laminarias with reticulated structure are more problematic, but it seems at least highly improbable that any movements of invertebrata could have produced such markings. The Alectoruridæ, an extinct group of algæ which existed from the Silurian into the Tertiaries, and their equally extinct ally Glossophycus, whose vegetable nature is even more apparent, may challenge reasonable criticism on account of their divergence from recent algæ. While the algous nature of these, and many other types, is maintained, the author does not hesitate to acknowledge that many forms which it was previously considered might be algæ, are probably tracks of invertebrates. He simply holds that Dr. Nathorst's generalisations are far too sweeping, and in many cases utterly against the evidence. The true nature of Bilobites, however, is still open to some question. They are always preserved in semi-relief, a process explained above, but the arguments, while abundantly proving that they cannot be due to tracks of invertebrates, fall short of absolute proof that they must be Algæ, and can be nothing else. In like manner the Eophyton of the Lower Cambrian, alleged by Nathorst to be furrows made by moving sea-weed on a muddy bottom, is almost proved by its occasionally cylindrical form and interlacing fragments, and wholly confined as it is to this most ancient formation, to be something more than mere scratches upon ooze, however produced, yet the evidence does not prove conclusively that it is a plant. The discussion has at least produced two most valuable works, the one serving to show how even the most accomplished palæophytologists may be deceived in dealing with so perilous a subject as fossil algæ, and the other proving that in spite of numerous errors, there is a considerable basis of truth in even the most speculative branch of their science. J. S. G.

NOTES

WITH reference to the scheme of the Grocers' Company for the encouragement of sanitary research, it is stated that so far as the administration of the scheme will involve scientific considerations, the Court proposes to act with the advice of a committee of eminent scientific men, and the following gentlemen have consented to form the first committee:—Messrs. John Simon, C.B., F.R.S., John Tyndall, F.R.S., John Burdon Sanderson, M.D., F.R.S., and George Buchanan, M.D., F.R.S.

A PRIVATE test took place on Monday of a telephone between New York and Chicago, a distance of 1000 miles, and the result was a complete success. Previously the longest distance over which a telephonic message had been sent was 700 miles, between New York and Cleveland. The present result is not due solely to the telephone, although that possesses some novelty, but is mainly due to a novelty in the conductor. This consisted, it is stated, of a steel wire core, copper plated, the electrical resistance of which to Chicago was only 1522 ohms. This new achievement is regarded as marking a new era in the development of telephonic communication.

AFTER assuming threatening proportions, the eruption of Mount Etna has almost subsided. Eleven new fissures had opened on the side of the mountain, giving out smoke, scoriæ, and showers of small stones, accompanied by a rumbling sound, and a trembling of the earth. Strong shocks of earthquake were felt at various parts of the surrounding country, and crevices were formed in the earth. A telegram from Prof. Silvestri, dated the 25th, states that the eruption is without importance and seems ceasing. Later news on Monday night states that there is still cause for some uneasiness in respect to Etna. The lava has not flowed, but has formed a new cone. On Monday strong shocks of earthquake were felt at Pedara, and slight ones at Catania. The site of the present eruption is further down the mountain than any previous eruption in modern times, and it is the first eruption which has occurred on the southern side of the mountain for more than a century.

WE regret to record the loss to science of a gifted and energetic young worker through a gun accident. A telegram from Hong Kong informs us that Mr. Frank Hatton, mineralogist and scientific explorer for the British North Borneo Company was killed by the accidental discharge of his gun while hunting in the jungle. The deceased gentleman was the only son of Mr. Joseph Hatton, and gave promise of a brilliant and useful scientific career. He was a student of the Royal School of Mines, South Kensington, where he distinguished himself by the extraordinary rapidity and accuracy with which he worked through the course of studies in that institution. He was especially distinguished in the Chemical Section, in which he made and published some valuable researches on Bacteria, &c., for which he obtained the Frankland prize of the Institute of Chemistry, entitling him to the degree of Associate. Mr. Hatton had great linguistic aptitude, and this, with a considerable amount of natural tact, contributed much to his success in dealing with the natives of Borneo during his exploring expeditions for the Company. During the last eighteen months he has explored the greater part of the Company's dominion, an area about as large as France, without losing a man, and in regions in which in many cases he was the only individual able to speak the Malay and Dusun dialects. A large number of scientific observations and notes on climate, geology, &c., of Borneo made during these expeditions will probably be published. Mr. Hatton, who had scarcely attained his twenty-second year at the time of his death, was a Fellow of the Chemical Societies of London and Berlin and of the Asiatic Society.

THE Gothenburg Museum will be represented at the coming Fisheries Exhibition by a magnificent selection of exhibits from its Zoological Section, the expenses of which will be borne by Dr. Oscar Dickson. This collection will be selected, arranged, and taken care of to London by Dr. A. H. Malm of that Museum. The collection will consist of the choicest gems of the Museum, among which are five rare species of whales, and the ichthyological fauna of the province of Bohus, as well as the well-known collection of herrings of various kinds and from different countries belonging to the Museum. There will also be sent a collection of skeletons of the fishes and birds comprising the fauna of Southern Sweden. The entire selection made by Dr. Malm is remarkable for its scientific accuracy, as well as finish. He will also show privately a splendid collection of Mollusca from the Cattegat.

THE Commission, consisting of Baron Nordenskjold, Consul Elfwing, and Prof. Gyldén, which the Royal Swedish Geographical Society had appointed to report on the question of an international meridian and a common time, has come to the conclusion that it would undoubtedly be a matter of great difficulty to decide as to the former on account of national jealousies, but it has offered a solution of the latter question which is worthy of notice. If the Greenwich meridian is fixed on as the common one, it would strike a point 180° from Greenwich, east of New Zealand, and if another circle is drawn 90° from Greenwich, its western half would nearly touch New Orleans, and its eastern a point a few minutes east of Calcutta. This system would furnish four cardinal times, viz. one European, one American, one Asiatic, and one Oceanic. As it would however be necessary to find several mean times for Europe, Prof. Gyldén proposes that twelve meridians be drawn from Greenwich, which he numbers at intervals of $2\frac{1}{2}$°, which will make the time of the places falling under each differ from those under the nearest meridian by 10 minutes of actual time. These meridians as numbered would either touch the places mentioned below, or fall so near them that the actual difference would be of no consequence. The difference of time from 10m. is however shown in the parentheses: No. 1. Paris (40s.); No. 2, Utrecht and Marseilles (1m. 29s.); No. 3, Bern (16s.) and Turin (42s.); No. 4, Hamburg (6s.), Altona (14s.), Gottingen (14s.), and Christiania (2m.); No. 5, Rome (50s.), Leipzig (26s.), and Copenhagen (20s.); No. 6, Sweden (15s. from common mean time); No. 7, Brieg (Prussia); No. 8, Konigsberg (2m.); No. 9, Abo (1m.) and Mistra (Greece) (5s.); No. 11, no place of importance; No. 12, St. Petersburg (1m. 14s.), and Kiev. Further east it is not suggested to carry the system. Should the various European countries decide on adopting the mean time of the nearest meridian they might be arranged as follows:—No. 1 for France; No. 2 for Holland and Belgium; No. 3 for Switzerland; No. 4 for Norway and Western Germany; No. 5 for Denmark, Central Germany, and Italy; No. 6 for Sweden and Austria; No. 7 for Eastern Germany; No. 8 for Hungary; No. 9 for Poland and Greece; No. 10 for Finland, Roumania, and Bulgaria; No. 11 for Turkey; No. 12 for Eastern Russia. West of Greenwich No. 1 would serve for Spain, and No. 3 for Portugal. By this system Prof. Gyldén thinks it would be a simple matter for every one to remember that the difference between two meridians, as, for instance, between London and Paris, was exactly 10 minutes. Prof. Gyldén also suggests that, for the convenience of travellers and others, all public clocks should be provided with coloured rings showing the differences of time between the various meridians.

THIS winter, at a large number of private and official *soirées* in Paris, the electric light has been used from storage batteries in a very simple manner. The accumulators are carried in a vehicle which is stationed in front of the house, and electric wires are conducted into the building through the windows. Incandescent lamps are placed in the ordinary candelabras, and the fitting of the most complex lighting is an affair of a very few hours.

THE new Elphinstone-Vincent dynamo machine was shown the other evening to a large party of visitors at Messrs. Unwin's printing offices; 411 Swan incandescent lights of twenty candle-power being well sustained by an engine of not at all large dimensions. The exhibition seems to show that the Elphinstone-Vincent machine in its present form of maturity is one of considerable merit. Its most notable feature is that the armature works between both external and internal magnets, that the saddles of wire of which the armature is formed constitute a very simple construction, that there is close proximity in the working parts to the magnets, and that, all the parts of the machine being duplicated, taking to pieces and repairing can be most readily effected.

THE Commissioners on Technical Education—Mr. Woodall, M.P., Mr. Samuelson, M.P., Mr. Wyer Smith, and Mr. Magnus, with Mr. Redgrave, secretary—paid a flying visit to Edinburgh last week. They visited, we understand, the Watt Institute—where they were received by Prof. Fleeming Jenkin and Lord Shand, to whom they expressed themselves highly satisfied with the tuitional and other arrangements of the Institute—the Museum of Science and Art, and Heriot's Hospital. One half of them afterwards inspected the Merchant Company's Schools, and the other half several of the Board Schools.

THE Surveying Expedition, under the direction of M. de Lesseps, has left Hamma, Tunis, and visited the mouth of the Oued Melah, which is to form the outlet of the projected Inland Sea Canal. It is declared that the result of the investigations shows that the cutting of the earth may be accomplished without difficulty.

M. COCHERY, the Minister of Postal Telegraphy, presided over the first monthly dinner of French Electricians, which is to take place on the 21st of every month, at the Café Durand. English electricians wishing to join should communicate with the director of *L'Électricité*, 16, Rue du Croissant, Paris. The president of the meeting for April 21 will be M. Berger, ex-director of the Electrical Exhibition of 1881.

A TELEGRAM from Copenhagen states that "volcanic ashes" have fallen in the neighbourhood of Trondhjem, Norway, and that a serious eruption of Mount Hecla is therefore supposed to have taken place. If these "ashes" are the dust referred to in our note last week (p. 496), then they are not of a volcanic character, according to the examination of Dr. H. Reusch of Christiania University. On this subject a Glasgow correspondent writes:—"My son, who is a passenger by the P. and O. steamer *Deccan* to the East, writes on February 27, when the steamer was in the Red Sea: 'Nothing of note occurred till evening, when G. and myself determined to sleep on deck, on account of the heat. We accordingly did so, and retired to our bunks about 4 a.m. . . . During our sleep on deck we were much annoyed by a quantity of small particles of dust which covered our faces, pillows, &c., and indeed was spread all around. . . . I am convinced it must have been a shower of lava dust, which, it is well known, is often carried hundreds of miles from the crater where it has origin. The dust was of hard particles. I am convinced that lava dust it was, but can get no one to coincide with my opinion.' Can this be a relation to the Norway dust? I see your Norway note says the wind blew strongly from north-north-west, which would bear towards the Red Sea."

M. Napoli, electrician to the French Great Eastern Railway, has published in the *Aeronaut* an article showing that electricity supplies a less ponderous motive power than steam for propelling balloons. He supposes that 3230 grammes of material is enough to generate, by means of Bunsen elements, an electric current able to give with a Gramme machine of a convenient construction one horse-power working during an hour.

On the evening of February 28, at 8·40 p.m., two travellers sledging over the Lesjö, a remote lake in Varmland, in Sweden, saw a meteor of remarkable size and lustre fall about a mile off. Their backs were turned at the time of its appearance, but its luminosity was so strong that the whole country round was illuminated, and when they turned its brilliancy blinded them for a few seconds. Its track was marked by a vivid band, to the eye one foot broad and three yards long, of a yellowish colour. The meteor, after about five seconds, burst with a shower of sparks of the same colour before striking the earth. The night was perfectly clear.

The Swedish Chamber of Agriculture has granted a Mr. A. Carlsson 50*l.* for the practical study of English agriculture during the coming season.

It is undoubted that Gramme was the first to construct a dynamo-electric machine with continuous induction, using (independently of Pacinotti) a ring-armature similar to Pacinotti's ring. But regarding the question, who it was that first produced continuous dynamo electric currents, and so was the first to combine experimentally the principles of Siemens and Pacinotti, Prof. von Waltenhofen offers proof (*Wied. Ann.* No. 2) that this priority belongs to Prof. Pfaundler of Innsbruck. In 1867 Herr Kravogl of Innsbruck showed his electromagnetic motor at the Paris Exhibition; this consists of coils forming a hollow ring which rotates round a horizontal axis, while it incloses a bent cylindrical rod tending by weight to take the lowest position, but kept suspended in a certain raised position by currents in the coils, whereby also the ring is rotated. In a letter on this machine in 1867 Prof. Pfaundler proposed to apply Siemens's principle to it, and get electric currents from mechanical work of rotation (the battery being included at first with a shunt, then quite excluded). This he tried and effected about three years later, as a letter dated February 11, 1870, records. Thus Pfaundler seems to have produced continuous dynamo-electric currents before Gramme, and to have indicated the possibility of getting such currents from the Kravogl ring machine in the same year (1867) as Siemens's invention of dynamo-electric machines acquired publicity.

The Committee of the Annonay Montgolfier celebration have already collected 60,000 francs, and subscriptions are pouring in. They have decided upon the publication of a special organ, of which the first number will be issued in a few days. The celebration will consist in the erection of a statue to the two brothers, several ascents, the sending up of a Montgolfier similar to the original one, and a cavalcade representing the provincial officials, who witnessed the proceedings on June 5, 1783.

It seems to result from recent researches by A. W. Pehl, brought before the Russian Chemical Society, that the poisonous action of the ergot, the bad effects of which are so often witnessed in Russia, is due to putrefaction poisons called ptomaines, which appear during the decomposition of the albuminoids in flour. The ergot, that is the sclerotium of the small mushroom, *Claviceps purpurea*, has energetic peptic qualities and thus would directly contribute to the formation of ptomaines in the flour.

We have received the last number of the Caucasian *Izvestia*, which appeared at Tiflis on February 24. It contains several interesting papers; M. Stebnitzky contributes a paper on the measurements by Parrot, in 1829, of the seconds pendulum on the Great Ararat, and, introducing all necessary corrections for rendering them comparable with recent measurements, he arrives at the result that the length of the pendulum at the monastery of St. Jacob on the Ararat is 440·1613 Paris lines. The anomaly would be thus equal to 7·7 swings per day, and corresponds to an elevation of geoide on the normal spheroid of 855 metres. Compared with Tiflis (1343 metres), this diminution of gravity would point out the existence of great cavities in the Ararat. We notice also a paper on the changes of height of the level of the Caspian Sea, by M. Filipoff; measurements of heights in the villayet of Trapezunt; complementary notes to the formerly-published anthropological measurements, by M. Erxert; and a summary of the first part of M. V. Miller's researches on the Osetian language. In the bibliographical part we find an interesting sketch of the climate of the Caucasus, on the ground of the meteorological observations published by Dr. H. Wild in his work, "Die Temperatur-Verhaltnisse des Russichen Reichs," and a report, by M. Zagursky, on Baron Uslar's posthumous work on the Tabasatan language; it is a serious work, containing a very elaborate grammar of the language, a list of words, and a chestomathy. The same fascicule contains the necrologies of Dr. Land and Count Sollogoub, and a variety of notes. In the appendix we find a translation of Mr. Palgrave's reports on Anatolia and Lazistan, which are considered as the more reliable with regard to population.

A series of shocks, lasting several seconds, believed at present to be due to earthquake, were felt at Amsterdam at 5 a.m. on March 17. The movement was in a vertical direction, and caused mirrors and other pendent articles of furniture to oscillate.

The additions to the Zoological Society's Gardens during the past week include a Common Wigeon (*Mareca penelope* ♂), British, presented by Lieut.-Col. C. Birch Reynordson; three Sirens (*Siren lacertina*) from South Carolina, presented by Mr. G. E. Manigault; six Common Squirrels (*Sciurus vulgaris*), British, a Lemur (*Lemur* ——) from Madagascar, two Robben Island Snakes (*Coronella phocarum*) from Robben Island, South Africa, purchased; a Gayal (*Bibos frontalis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Comet 1883 *a*.—From elements calculated by Dr. Hepperger of Vienna upon observations extending from Feb. 24 to March 4, the following ephemeris for midnight at Berlin results:—

	R.A. h. m. s.	Decl. ° ′	Distance from Earth.	Distance from Sun
March 30	3 29 24	+21 49·2	1·497	1·073
31	3 33 59	21 19·5		
April 1	3 38 25	20 50·3	1·536	1·098
2	3 42 44	20 21·4		
3	3 46 55	19 52·9	1·576	1·124
4	3 50 58	19 24·8		
5	3 54 55	18 57·0	1·616	1·150
6	3 58 46	18 29·6		
7	4 2 32	18 2·7	1·657	1·177
8	4 6 13	17 36·2		
9	4 9 48	+17 10·2	1·698	1·204

The ascending node of this comet falls at a radius-vector of about 2·36 in the region of the minor planets, the descending node at a radius-vector of 1·12, or 0·14 outside the earth's path; but, for the comet to pass at its least distance from our globe, the perihelion passage must occur about November 16.

THE MINOR PLANET NO. 228.—The nearest approach to the earth's orbit made by any one of the 232 small planets so far known appears to occur in the case of No. 228, discovered by Herr Palisa at Vienna on August 19, 1882. At the perihelion point this planet may be distant from us only 0·662 of our mean distance from the sun, and on this account would prove a favourable object for a determination of solar parallax. But unfortunately the brightness of the planet at discovery was only 12·5m., though the mean anomaly was then 1½°, or the perihelion passage took place five days subsequently. Hence it is very questionable if such an object could be utilised for the purpose. No. 132, *Æthra*, has the smallest perihelion distance (1·6038), but in consequence of the large angle between the lines of nodes and apsides, and an inclination of nearly 25°, this planet is much further from the earth's track at perihelion than No. 228. *Andromache*, No. 175, recedes furthest from the sun, the distance at aphelion being 4·7234, or within 0·48 of the mean distance of Jupiter.

BINARY STARS.—According to Dr. Doberck's orbit of γ Coronæ Borealis, this very difficult object should now be measurable with our larger instruments. For 1883·5 the calculated position is 123°, and the distance 0″·34. This object was single, with the great refractor at Washington, from 1875 to 1879. In June, 1881, it was pronounced round, or doubtfully elongated, by Mr. Burnham, who remarks, "It has been apparently single with all apertures since about 1871." Doberck's period of revolution is 95½ years: periastron passage, 1843·7.

The following calculated angles and distances of several other binaries may serve for comparison with observations :—

Epoch.	Star.	Position.	Distance.	Authority for orbit.
		°	″	
1882·5	η Cassiopeiæ	163·3	5·52	Doberck.
	,,	161·8	5·38	Duner.
1882·5	ξ Bootis	268·9	3·56	Doberck.
1883·5	,,	267·6	3·20	,,
1882·5	ω Leonis	86·5	0·60	,,
1883·5	,,	88 2	0 61	,,
1882·5	η Coronæ Bor.	140·9	0 51	,,
1882·5	ζ Herculis	105·9	1·43	,,
1882·5	μ^2 Herculis	297·3	0·88	,,
1882 5	70 Ophiuchi	63·5	2·98	Tisserand.

ELECTRICAL TRANSMISSION OF FORCE AND STORAGE OF POWER[1]

DR. SIEMENS, in opening the discourse, reverted to the object the Council had in view in organising these occasional lectures, which were not to be lectures upon general topics, but the outcome of such special study and practical experience as Members of the Institution had exceptional opportunities of acquiring in the course of their professional occupation. The subject to be dealt with during the present session was that of electricity. Already telegraphy had been brought forward by Mr. W. H. Preece, and telephonic communication by Sir Frederick Bramwell.

Thus far electricity had been introduced as the swift and subtle agency by which signals were produced either by mechanical means or by the human voice, and flashed almost instantaneously to distances which were limited, with regard to the former, by restrictions imposed by the globe. To Dr. Siemens had been assigned the task of introducing to their notice electric energy in a different aspect. Although still giving evidence of swiftness and precision, the effects he should dwell upon were no longer such as could be perceived only through the most delicate instruments human ingenuity could contrive, but were capable of rivalling the steam engine, compressed air, and the hydraulic accumulator, in the accomplishment of actual work.

In the early attempts at magneto-electric machines, it was shown that, so long as their effect depended upon the oxidation of zinc in a battery, no commercially useful results could have been anticipated. The thermo-battery, the discovery of Seebeck in 1822, was alluded to as a means of converting heat into electric energy in the most direct manner; but this conversion could not be an entire one, because the second law of thermodynamics, which prevented the realisation as mechanical force of more than one-seventh part of the heat energy produced in combustion under the boiler, applied equally to the thermo-electric battery, in which the heat, conducted from the hot points of juncture to the cold, constituted a formidable loss. The electromotive force of each thermo-electric element did not exceed 0·036 of a volt, and 1800 elements were therefore necessary to work an incandescence-lamp.

A most useful application of the thermoelectric battery for measuring radiant heat, the thermopile, was exhibited. By means of an ingenious modification of the electrical pyrometer, named the Bolometer, valuable researches in measuring solar radiations had been made by Prof. Langley.

Faraday's great discovery of magneto-induction was next noticed, and the original instrument by which he had elicited the first electric spark before the members of the Royal Institution in 1831, was shown in operation. It was proved that although the individual current produced by magneto-induction was exceedingly small and momentary in action, it was capable of unlimited multiplication by mechanical arrangements of a simple kind, and that by such multiplication, the powerful effects of the dynamo-machine of the present day were built up. One of the means for accomplishing such multiplication was the Siemens armature of 1856. Another step of importance was that involved in the Pacinnoti ring, known in its practical application as the machine of Gramme. A third step, that of the self-exciting principle, was first communicated by Dr. Werner Siemens to the Berlin Academy, on January 17, 1867, and by the lecturer to the Royal Society on the 4th of the following month. This was read on February 14, when the late Sir Charles Wheatstone also brought forward a paper embodying the same principle. The lecturer's machine which was then exhibited, and which might be looked upon as the first of its kind, was shown in operation; it had done useful work for many years as a means of exciting steel magnets. A suggestion, contained in Sir Charles Wheatstone's paper, that "a very remarkable increase of all the effects, accompanied by a diminution in the resistance of the machine, is observed when a cross wire is placed so as to divert a great portion of the current from the electro-magnet," had led the lecturer to an investigation read before the Royal Society on March 4, 1880, in which it was shown that by augmenting the resistance upon the electro-magnets a hundredfold, valuable effects could be realised, as illustrated graphically by means of a diagram. The most important of these results consisted in this, that the electromotive force produced in a "shunt-wound machine," as it was called, increased with the external resistance, whereby the great fluctuations formerly inseparable from electric-arc lighting could be obviated, and that, by the double means of exciting the electro-magnets, still greater uniformity of current was attainable.

The conditions upon which the working of a well-conceived dynamo-machine must depend were next alluded to, and it was demonstrated that when losses by unnecessary wire-resistance, by Foucault-currents, and by induced currents in the rotating armature were avoided, as much as 90 per cent., or even more, of the power communicated to the machine were realised in the form of electric energy, and that *vice versâ* the reconversion of electric into mechanical energy could be accomplished with similarly small loss. Thus, by means of two machines at a moderate distance apart, nearly 80 per cent. of the power imparted to the one machine could be again yielded in the mechanical form by the second, leaving out of consideration frictional losses, which latter need not be great, considering that a dynamo-machine had only one moving part well balanced, and was acted upon along its entire circumference by propelling force. Jacobi had proved many years ago that the maximum efficiency of a magneto-electric engine was obtained when

$$\frac{e}{E} = \frac{w}{W} = \frac{1}{2}$$

which law had been frequently construed by Verdet ("Théorie Mécanique de la Chaleur") and others to mean that one-half was the maximum theoretical efficiency obtainable in electric transmission of power, and that one-half of the current must be necessarily wasted or turned into heat. The lecturer could never be reconciled to a law necessitating such a waste of energy, and had maintained, without disputing the accuracy of Jacobi's law, that it had reference really to the condition of maximum work accomplished with a given machine, whereas its efficiency must be governed by the equation

$$\frac{e}{E} = \frac{w}{W} = \text{nearly } 1.$$

From this it followed that the maximum yield was obtained when two dynamo-machines (of similar construction) rotated nearly at the same speed, but that under these conditions the amount of force transmitted was a minimum. Practically the best condition of working consisted in giving to the primary machine such proportions as to produce a current of the same magnitude, but of 50 per cent. greater electromotive force than the secondary; by adopting such an arrangement, as much as 50 per cent. of the power imparted to the primary could be practically received from the secondary machine at a distance of several miles. Prof. Silvanus Thompson, in his recent Cantor Lectures, had shown an ingenious graphical method of proving these important fundamental laws.

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[1] Abstract of lecture given at the Institution of Civil Engineers on March 15 by Dr. C. William Siemens, F.R.S., M.Inst C.E. Revised by the author.

The possibility of transmitting power electrically was so obvious that suggestions to that effect had been frequently made since the days of Volta, by Ritchie, Jacobi, Henry, Page, Hjorth, and others; but it was only in recent years that such transmission had been rendered practically feasible.

Just six years ago, when delivering his presidential address to the Iron and Steel Institute, the lecturer had ventured to suggest that "time will probably reveal to us effectual means of carrying power to great distances, but I cannot refrain from alluding to one which is, in my opinion, worthy of consideration, namely, the electrical conductor. Suppose water-power to be employed to give motion to a dynamo-electrical machine, a very powerful electrical current will be the result, which may be carried to a great distance, through a large metallic conductor, and then be made to impart motion to electro-magnetic engines, to ignite the carbon points of electric lamps, or to effect the separation of metals from their combinations. A copper rod 3 inches in diameter would be capable of transmitting 1000 h.p. a distance of say 30 miles, an amount sufficient to supply one quarter of a million candle-power, which would suffice to illuminate a moderately-sized town." This suggestion had been much criticised at the time, when it was still thought that electricity was incapable of being massed so as to deal with many horse power of effect, and the size of conductor he had proposed was also considered wholly inadequate. It would be interesting to test this early calculation by recent experience. Mr. Marcel Deprez had, it was well known, lately succeeded in transmitting as much as 3 h.p. to a distance of 40 kilometres (25 miles) through a pair of ordinary telegraph wires of 4 mm. diameter. The results so obtained had been carefully noted by Mr. Tresca, and had been communicated a fortnight ago to the French Academy of Sciences. Taking the relative conductivity of iron wire employed by Deprez, and the 3-inch rod proposed by the lecturer, the amount of power that could be transmitted through the latter would be about 4000 h.p. But Deprez had employed a motor-dynamo of 2000 volts, and was contented with a yield of 32 per cent. only of the power imparted to the primary machine, whereas he had calculated at the time upon an electromotive force of 200 volts, and upon a return of at least 40 per cent. of the energy imparted. In March, 1878, when delivering one of the Science Lectures at Glasgow, he said that a 2-inch rod could be made to accomplish the object proposed, because he had by that time conceived the possibility of employing a current of at least 500 volts. Sir William Thomson had at once accepted these views, and with the conceptive ingenuity peculiar to himself, had gone far beyond him, in showing before the Parliamentary Electric Light Committee of 1879, that through a copper wire of only ½-inch diameter, 21,000 h.p. might be conveyed to a distance of 300 miles with a current of an intensity of 80,000 volts. The time might come when such a current could be dealt with, having a striking distance of about 1·2 feet in air, but then, probably, a very practical law enunciated by Sir William Thomson would be infringed. This was to the effect that electricity was conveyed at the cheapest rate through a conductor, the cost of which was such that the annual interest upon the money expended equalled the annual expenditure for lost effect in the conductor in producing the power to be conveyed. It appeared that Mr. Deprez had not followed this law in making his recent installations.

Sir William Armstrong was probably first to take practical advantage of these suggestions in lighting his house at Cragside during night-time, and working his lathe and saw-bench during the day, by power transmitted through a wire from a waterfall nearly a mile distant from his mansion. The lecturer had also accomplished the several objects of pumping water, cutting wood, hay, and swedes, of lighting his house, and of carrying on experiments in electro-horticulture from a common centre of steam-power. The results had been most satisfactory; the whole of the management had been in the hands of a gardener and of labourers, who were without previous knowledge of electricity, and the only repairs that had been found necessary were one renewal of the commutators and an occasional change of metallic contact brushes.

An interesting application of electric transmission to cranes, by Dr. Hopkinson, was shown in operation.

Amongst the numerous other applications of the electrical transmission of power, that to electrical railways, first exhibited by Dr. Werner Siemens, at the Berlin Exhibition of 1879, had created more than ordinary public attention. In it the current produced by a dynamo-machine, fixed at a convenient station and driven by a steam-engine or other motor, was conveyed to a dynamo placed upon the moving car, through a central rail supported upon insulating-blocks of wood, the two working-rails serving to convey the return current. The line was 900 yards long, of 2-feet gauge, and the moving car served its purpose of carrying twenty visitors through the Exhibition each trip. The success of this experiment soon led to the laying of the Lichterfelde line, in which both rails were placed upon insulating sleepers, so that the one served for the conveyance of the current from the power station to the moving car, and the other for completing the return circuit. This line had a gauge of 3 feet 3 inches, was 2500 yards in length, and was worked by two dynamo-machines, developing an aggregate current of 9000 Watts, equal to 12 h.p. It had now been in constant operation since May 16, 1881, and had never failed in accomplishing its daily traffic. A line half a kilometer in length, but of 4 feet 8½ inch gauge, was established by the lecturer at Paris in connection with the Electric Exhibition of 1881. In this case two suspended conductors in the form of hollow tubes with a longitudinal slit were adopted, the contact being made by metallic bolts drawn through these slit tubes, and connected with the dynamo-machine on the moving car by copper ropes passing through the roof. On this line 95,000 passengers were conveyed within the short period of seven weeks.

An electric tramway 6 miles in length had just been completed, connecting Portrush with Bush Mills in the north of Ireland, in the installation of which the lecturer was aided by Mr. Traill, as engineer of the Company, by Mr. Alexander Siemens, and by Dr. E. Hopkinson, representing his firm. In this instance the two rails, 3 feet apart, were not insulated from the ground, but were joined electrically by means of copper staples and formed the return circuit, the current being conveyed to the car through a T iron placed upon short standards, and insulated by means of insulite caps. For the present the power was produced by a steam-engine at Portrush, giving motion to a shunt-wound dynamo of 15,000 Watts = 20 h.p., but arrangements were in progress to utilise a waterfall of ample power near Bush Mills, by means of three turbines of 40 h.p. each, now in course of erection. The working-speed of this line was restricted by the Board of Trade to 10 miles an hour, which was readily obtained, although the gradients of the line were decidedly unfavourable, including an incline of 2 miles in length at a gradient of 1 in 38. It was intended to extend the line 6 miles beyond Bush Mills, in order to join it at Dervock station with the north of Ireland narrow-gauge railway system.

The electric system of propulsion was, in the lecturer's opinion, sufficiently advanced to assure practical success under suitable circumstances—such as for suburban tramways, elevated lines, and above all lines through tunnels, such as the Metropolitan and District Railways. The advantages were that the weight of the engine, so destructive of power and of the plant itself in starting and stopping, would be saved, and that perfect immunity from products of combustion would be insured. The limited experience at Lichterfelde, at Paris, and with another electric line of 765 yards in length, and 2 feet 2 inches gauge, worked in connection with the Zaukerode Colliery since October, 1882, were extremely favourable to this mode of propulsion. The lecturer however did not advocate its prospective application in competition with the locomotive engine for main lines of railway. For tramways within populous districts the insulated conductor involved a serious difficulty. It would be more advantageous under these circumstances to resort to secondary batteries, forming a store of electrical energy carried under the seats of the car itself, and working a dynamo-machine connected with the moving wheels by means of belts and chains.

The secondary battery was the only available means of propelling vessels by electrical power, and considering that these batteries might be made to serve the purpose of keel ballast, their weight, which was still considerable, would not be objectionable. The secondary battery was not an entirely new conception. The hydrogen gas battery suggested by Sir Wm. Grove in 1841, and which was shown in operation, realised in the most perfect manner the conception of storage, only that the power obtained from it was exceedingly slight. The lecturer, in working upon Sir William Grove's conception, had twenty-five years ago constructed a battery of considerable power in substituting porous carbon for platinum, impregnating the same with a precipitate of lead peroxidised by a charging current. At that time little practical importance attached, however, to the subject, and even when Planté, in 1860, produced his secondary battery, composed of lead plates peroxidised by a charging current, little more than scientific curiosity was excited. It was only since the dynamo-machine had become an accomplished fact that the importance of this mode of storing energy had become of practical importance, and great credit was due to Faure, to Sellon, and to Volckmar, for putting this valuable addition to practical science into available forms. A question of great interest in connection with the secondary battery had reference to its permanence. A fear had been expressed by many that local action would soon destroy the fabric of which it was composed, and that the active surfaces would become coated with sulphate of lead preventing further action. It had, however, lately been proved in a paper read by Dr. Frankland before the Royal Society, corroborated by simultaneous investigations by Dr. Gladstone and Mr Tribe, that the action of the secondary battery depended essentially upon the alternative composition and decomposition of sulphate of lead, which was therefore not an enemy, but the best friend to its continued action.

In conclusion, the lecturer referred to electric nomenclature, and to the means for measuring and recording the passage of electric energy. When he addressed the British Association at Southampton, he had ventured to suggest two electrical units additional to those established at the Electrical Congress, in 1881, viz., the Watt and the Joule, in order to complete the chain of units connecting electrical with mechanical energy and with the unit-quantity of heat. He was glad to find that this suggestion had met with favourable reception, especially that of the Watt, which was convenient for expressing in an intelligible manner the effective power of a dynamo-machine, and for giving a precise idea of the number of lights or effective power to be realised by its current, as well as of the engine power necessary to drive it: 746 Watts represented 1 h.p.

Finally the Watt-meter, an instrument recently developed by his firm, was shown in operation. This consisted simply of a coil of thick conductor suspended by a torsion wire, and opposed laterally to a fixed coil of wire of high resistance. The current to be measured flowed through both coils in parallel circuit, the one representing its quantity expressible in Ampères, and the other its potential expressible in Volts. Their joint attractive action expressed therefore Volt-Ampères or Watts, which were read off upon a scale of equal divisions.

The lecture was illustrated by experiments, and by numerous diagrams and tables of results. Measuring instruments by Professors Ayrton and Perry, by Mr. Edison and by Mr. Boys were also exhibited.

FAUNA AND FLORA OF THE ALEUTIAN ISLANDS

THE last number of *Naturen* contains an interesting report by Dr. Leonhard Stejneger of the result of his six months' observations of the fauna and flora of the Kamschatkan coast and of the so-called Kommandorski Islands, which form the western group of the Aleutian archipelago between Behring's Sea and the Pacific, in 50°–55° N. lat. The Kommandorski group consists of two islands, one of which is known as Mednoj Ostrov, Copper Island, from the large amount of the pure metal found there; while the other, which was the scene of Behring's shipwreck and death, bears his name. Both islands are geologically allied to Kamschatka, and excepting at the north of Behring's Island, where the gradual subsidence of the sea has left raised beaches, terraces, and tabulated rock-formations, the islands consist generally of deep narrow valleys separated by rocky barriers, which rise precipitously to a height of from 1000 to 2000 feet above the level of the sea. The islands, which were uninhabited before their annexation by Russia, are now occupied by about 700 persons, in the employment of a Russo-American fur company, which has been attracted to the spot by the enormous numbers of sea-bears (*Callorhinus ursinus*) and sea-otters (*Enhydra lutris*) which frequent the coasts. The climate is foggy, and the vegetation stunted and sparse, while in the neighbouring Kamschatkan territory the blue of the summer sky, the stillness of the sea, and the softness of the air, are almost Italian in character. The flora, moreover, is so exuberant that numerous plants, which in Norway never exceed two or three feet, here attain the height of a tall man. Next to the birch (*Betula ermanni*), alders, willows, and roans (*Sorbus Kamschaticus*), are the most frequent trees, the berries of the last-named, and those of *Lonicera cœrulea*, possessing a sweetness which brings them into great request among strangers as well as natives. Some flowers also, as the wild, indigenous, dark red rose, several rhododendrons, and native lilies, are equally remarkable for exceptional fragrance. Among wild flowers, some of the geraniums, potentillas, taraxacums, &c., are almost identical with those found in Norway. Besides a large whale, and a specimen of the walrus (*Rosmarus obesus*), which had been killed near Avatscha Bay, Dr. Stejneger could find no trace of any mammal but a small specimen of *Arvicola œconomus*. Of birds there is, however, an enormous variety, some of which, as *Calliope Kamschatica*, *Carpodacus Erythrinus*, and a kind of sedge-warbler, provisionally named by the author "*Acrocephalus dybowskii*," combine an almost tropical brilliancy of colouring with a sweetness of song equal to that of our own nightingale or thrush. Besides these melodious warblers, Kamschatka harbours large numbers of *Locustella lanceolata*, whose grasshopper-like cry is heard when all else is still. *Cuculus canorinus* represents our common cuckoo. Pipits, chats, and wagtails abound; *Larus capistratus* is commoner than any other gull, and the osprey is not unfrequent. Mosquito-like gnats of vindictive nature swarm in such numbers as to make the pursuits of the field naturalist almost impracticable. The fauna, generally, is palæarctic in character, with a scarcity of American forms which is very remarkable when we consider the vicinity of the western continent.

PHYSICAL HISTORY OF THE DEAD SEA, THE JORDAN VALLEY, AND PALESTINE

PROF. E. HULL, LL.D., F.R.S., delivered an interesting lecture on the above subject on March 2, in the Theatre of the Royal Dublin Society's premises, Kildare Street. Prof. Hull said:—"There is no country which possesses for us an interest equal to that which I have to treat of this evening. Its religious and historical associations stand alone amongst those of all nations, and will ever maintain in the history of the world an undying import. But while this is true as regards the religious and social aspects of Palestine, I hope to show that in its physical aspect it possesses points of interest which render it unique amongst all countries, and which have attracted to it the attention of naturalists during a lengthened period down to the present day. Probably no country has been so often described. Its physical features have attracted the attention of observers of natural phenomena from Strabo downwards to the recent admirable work of M. Lartet and the Duc de Luynes, to which I am largely indebted. In more recent times we have the observations of Humboldt, of the late Dr. Hitchcock, of Lieut. Lynch of the United States Navy, who carried out a systematic series of soundings over the bed of the Dead Sea, and more recently of the Rev. Dr. Tristram, of Prof. Roth, Burkhardt, and others, including the Survey made by the officers of the Royal Engineers. It is curious however that the remarkable physical phenomenon which renders the Holy Land unique amongst all countries (regarded in its physical aspect) was not discovered till the year 1836-37, when Heinrich Von Schubert and Prof. Roth determined by barometric observations that the surface of the Dead Sea lies no less than 1300 feet below the level of the Mediterranean, a fact not suspected by previous observers. It is the deep depression of the Jordan Valley, deeper by far than any river valley elsewhere, which is the key to the physical history of the whole country; and in endeavouring to trace out its origin I shall reproduce in as general a manner as I can the successive phases through which the region bordering the Mediterranean, and extending eastwards towards the Euphrates and southwards to the Dead Sea, has passed. The fundamental basis of the geological formation of Palestine is the gneissic granite, of Archæan age and metamorphic origin, which rises into the mountains of Idumea, and is the rock from which the huge monoliths of Egypt have been hewn, such as Cleopatra's Needle' the obelisk of Luxor, and the columns which adorn the Piazza of Venice. This foundation rock formed part of a continental area down to the Carboniferous period, when it was submerged, and a great sandstone formation was spread over it known as "the Nubian sandstone." After another interval of time the sandstone itself was overspread by limestone deposits of Cretaceous and Tertiary age, deposited over the floor of the ancient sea, and down to the close of the Eocene period the waters of the sea overspread the greater portions of Asia Minor, Palestine, and the adjoining districts of the Asiatic and African continents. The first appearance of Palestine and the adjoining districts as a land surface dates from the succeeding Miocene period, when the bed of the sea was upraised into dry land, and at the same period a great fissure corresponding with the line of the Jordan valley was produced. Along this fissure, which has been traced from the Lebanon southwards towards the Gulf of Akaba—the strata on the eastern (or Moabite) side have been relatively elevated: those on the western relatively depressed;—so that the strata on the opposite sides of the Jordan valley and the Dead Sea do not correspond with each other. This great fissure is the key to the physical formation of the whole region, because it gave origin to a river which once flowed down from the mountains of Lebanon—southwards through the Gorge of Arabah (discovered by Burkhardt)—into the Red Sea in a remarkably straight line running north and south for a distance of over 250 miles. This is now the Jordan. The depression of the valley continuing through the succeeding Pliocene epoch, the district of the Ghor and the Jordan valley was conveyed into a lake, which Prof. Hull considered ultimately extended from the southern end of the Dead Sea, northwards nearly to the Lake Merom, and included the Sea of Galilee. This lake would then have had a length of 160 miles and an average breadth of ten miles. During "the Pluvial period," which succeeded "the Glacial," the waters probably reached their maximum elevation, and continued to flow southwards through the Gorge of Arabah and the Gulf of Akaba into the Red Sea; but from the increasing dryness of the climate they gradually decreased, and the surface of the lake became contracted, and ultimately reduced to its existing limits. During this lowering of the surface, the remarkable terraces noticed by most travellers were formed. Dr. Tristram has taken the barometric level of several of these above the Dead Sea. They range up to 750 feet, and even higher. They appear to be undoubtedly old lake margins, and indicate the successive levels at which the lake stood. The 750-foot terrace very closely corresponds to the summit-level of the Gorge of Arabah. When the waters were reduced so low as not to pass through the Gorge of Arabah, they became brackish, and ultimately salt—the salinity increasing as the area became diminished. All lakes not having an outlet become saline; and the contrast of the waters of the Sea of Galilee and those of the Dead Sea form a striking illustration of the law just stated. The saline ingredients in the surface waters of the Dead Sea are 24·57 lbs. in 100 lbs. of the water, while that of the Atlantic only contains 6 lbs. in the same quantity. The Dead Sea water is therefore over four times as strongly impregnated with salts as that of the ocean, and in the deeper waters the salinity amounts to saturation, as saline deposits are forming over the floor of the Dead Sea. This remarkable inland sea had assumed somewhat of its present contracted dimensions, and was known as "the Salt Sea" as far back as the time of the Patriarch Abraham. Near its borders stood the doomed cities of Sodom and Gomorrah—not beneath its waters, as was often supposed—but near its upper margin. With the call of Abraham the political and religious history of Palestine begins, and the narrative of the physical historian ends

SCIENTIFIC SERIALS

American Journal of Science, March.—The selective absorption of solar energy, by S. P. Langley.—New locality of the green turquois known as chalcuite, and on the identity of turquois with the callais or callaina of Pliny, by W. P. Blake.—On portions of the skeleton of a whale from gravel on the line of the Canada Pacific Railway near Smith Falls, Ontario, by J. W. Dawson.—The cobwebs of Uloborus, by J. H. Emerton.—Glacial drift in the Upper Missouri River region, by C. A. White.—Late observations concerning the molluscan fauna and the geographical extent of the Laramie group, by the same.—The Sphingidæ of North America, by A. R. Grote.—"Rotational coefficients" of various metals, by E. H. Hall.—Recent exploration of the volcanic phenomena of the Hawaiian islands, by C. E. Dutton.

Journal of the Russian Chemical and Physical Society, vol. xiv. fasc. 9.—On several ethylenic hydrocarbons, and on their action on water, by M. A. Eltekoff. Of the compounds of the series $C_nH_{2n}O$, the oxides are the least known, and it still remains in doubt as to those described by MM. Bauer, Würtz, Jekyll, and Clermont being true oxides and not ketones; M. Eltekoff studied, therefore, the action on water of seven compounds of this series. He arrives at the conclusion that the characteristic features of oxides do not disappear, as seemed formerly to be the case, in more compound oxides containing even as much as six equivalents of carbon. Their capacity of entering in direct compounds with water diminishes, however, in proportion as the molecule becomes more complicated.—On the oxidation of sulphur used for covering the vineyards, by M. A. Bazaroff—On the evaporation of liquids, by M. Sreznewsky. Evaporation of benzol, ether, ethyl-alcohol, chloroform and sulphur of carbon at different temperatures. The paper will be continued.—On the critical temperature and pressure of water, by M. O. Strauss. The average of a series of observations gives for the critical temperature of water 370°, with a probable error of 5°. The critical pressure would be 195·5 atmospheres.—Historical sketch of the work accomplished by the Physical Society during its ten years' existence, by M. N. Hesehus.—On the temperature of the absolute vaporisation of liquids, by M Nadejdin.—On the spheroidal state of liquids, by M. D. Diakonoff.—Minutes of proceedings.

Rivista Scientifico-Industriale e Giornale del Naturalista, January 15.—The glossograph of S. Gentilli.—Influence of ozone in agriculture, by S. Zinno.—The radiometer and school experiments, by C. Rovelli—Fossil elephants in the district of Parma.—Simple holohedral forms of the rhombohedral system, by M. de Lupo.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. xvi. fasc. i.—Meteorological *résumé* of the year 1882, calculated on observations made at the Royal Observatory of Brera, by E. Pini.—The frost of 1882 considered in its agrarian and meteoric aspect, by E. Ferrario.—Results of observations on the amplitude of diurnal oscillation of the declination-needle made during 1882 at Brera Observatory, by G. Schiaparelli.—On the action of metallic iodide on leucine and other like substances, by G. Korner and E. Menozzi.

Fasc. ii.—Property of a class of functions with more variables than are presented in dynamics in the case of permanent motion, by C. Formenti.—On some plane involutions, by E. Bertini.—Generalisation of a theorem on the analytical representation of substitutions, by A. Grandi.

Schriften der Physicalisch-Ökonomischen Gesellschaft zu Konigsberg. 1880, first part; 1881, first and second parts.—Geological investigation of the North German level country, especially East and West Prussia, in the years 1878–80, by A. Jentzsch.—Contributions to a knowledge of the Silurian Cephalopoda found in the East and West Prussian diluvial formations, by H. Schröder.—Rugous corals in the same formation, by G. Meyer.—The scales of our fishes, by B. Benecke.—On some diluvial and alluvial diatom-layers of North Germany, by P. T. Cleve and A. Jentzsch.—The underground portion of the North German level country; by A. Jentzsch.

Verhandlungen der Naturhistorischen Vereines der Preussischen Rheinlande und Westfalens, 1882 (first half).—Further observations on fertilisation of flowers by insects, by H. Muller.—On the various systems of measurement of electric and magnetic quantities, by R. Clausius.—The lower Devonian strata of Olkenbach, by O. Follmann.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 8.—"Notes on the Absorption of Ultra-Violet Rays by various Substances," by Professors Liveing and Dewar.

These notes contain some records of ultra-violet absorptions in addition to those which have been examined by Soret, Hartley, M. de Chardonnet, and other investigators. For these observations the spark of an induction coil, with Leyden jar, between iron electrodes, was generally used as the source of light. The lines of iron are so multitudinous, and so closely set in a large part of the ultra-violet region of the spectrum, that they form almost a continuous spectrum, at the same time there are amongst them a sufficient number of breaks and conspicuous lines to serve as points of reference. The optical train used was wholly of quartz, and the spectra were all photographed.

Chlorine in small quantity shows a single absorption band extending from about N (3580) to T (3020). As the quantity of chlorine is increased this band widens, expanding on both sides, but rather more rapidly on the less refrangible side. Different quantities of chlorine produced absorption from about H (3968) to wave-length 2755, from wave-length 4415 to 2665, and from wave-length 4650 to 2630. With the greatest quantity of chlorine tried, the absorption did not extend above wave-length 2550.

Bromine vapour in small quantity absorbs light up to about L (3820), and is quite transparent above that. With larger quantity the absorption increases, gradually extending with increase of bromine vapour from L to P (3360); and at the same time there is a gradually increasing general absorption at the most refrangible end of the spectrum beginning at about wave-length 2500; so that the denser bromine vapour is transparent for a band between wave-length 2500 and 3350.

Liquid bromine in very thin film between two quartz plates is transparent for a band between wave-length about 3650 and 3400, shading away on both sides, so that below M on one side and above P on the other the absorption seems complete. The transparency of the liquid film ends on the more refrangible side just where that of the vapour begins.

Iodine vapour tolerably dense cuts off all within the range of our photographs below wave-length 4300, and its absorption gradually diminishes from that point up to about wave-length 4080; from that point it is transparent. Denser vapour produces complete absorption up to 4080 and partial absorption above that point.

Comparing the absorptions of the three haloid elements, the principal band shifts towards the less refrangible side with increasing atomic weight, as Lecoq de Boisbaudran has noticed in the case of lines corresponding to one another in the spectra of groups of similar metals.

Iodine dissolved in carbon disulphide is transparent for a band between G and H; cutting off all above and below. It is not possible to tell how much of the light above M (3727) is absorbed by iodine in such a solution, inasmuch as carbon disulphide is opaque for rays more refrangible than M.

Iodine dissolved in carbon tetrachloride when the solution is weak shows only the absorption due to the solvent, described below. More iodine increases the absorption until it is complete above P (3360), with shading edge as far down as about wave-length 3400.

Sulphurous acid gas produces an absorption band which is very marked between R (3179) and wave-length 2630, and a fainter absorption extending on the less refrangible side to O (3440), and on the other side to the end of the range photographed, wave-length 2300.

Sulphuretted hydrogen produces complete absorption above wave-length 2580. Below that a partial general absorption.

Vapour of carbon disulphide in very small quantity produces an absorption band extending from P to T, shading away at each end; no absorption in the higher region. With more vapour the absorption band widens, extending from about wave-length 3400 to 3000, and a second absorption occurs beginning at about wave-length 2580, and extending to the end of the range photographed.

Carbon tetrachloride liquid produces an absorption band with a maximum about R, extending, but with decreasing intensity, up to Q (3285) on one side, and to *s* (3045) on the other. In the higher region there is a second absorption sensible about wave-length 2600, and increasing in intensity up to about wave-length 2580, beyond which point it is complete.

Chlorine peroxide gives a succession of nine shaded bands, at nearly equal intervals, between M and S, with faint indications of others beyond. In the highest region this gas seems quite transparent.

A slice of chrome-alum a quarter of an inch thick is transparent between wave-lengths 3270 and 2830; its absorption gradually increases on both sides of those limits, but rather more rapidly on the more refrangible side than on the other, and becomes complete below about wave-length 3360 and above wave-length 2730.

A very thin plate of mica shows absorption beginning about S (3100), rapidly increasing above U (2947), and complete above wave-length 2840.

A thin film of silver precipitated chemically on a plate of quartz transmits well a band of light between wave-length about 3350 and 3070, but is quite opaque beyond those limits on both sides.[1]

A thin film of gold similarly precipitated merely produces a slight general absorption all along the spectrum.

The difference between the limits of transparency of Iceland spar for the ordinary and extraordinary rays, inferred from theory, was found to be very small, and hardly to be detected without using a considerable thickness, three inches or more, of the spar.

The authors had expected to be able to apply the well-known photometric method by means of polarised light to the comparison of intensities of ultra violet rays. Ordinary Nicol's prisms are not applicable to ultra-violet rays on account of the opacity of the Canada balsam, with which they are cemented, so Foucault's prisms were used. Upon taking photographs of the spectrum of the iron spark through this pair of prisms at various inclinations between the planes of polarisation of the two prisms, it was found that for the whole range between the position of parallelism and the inclination of 80° there was no sensible difference of effect upon the photographic plate, though the length of exposure was in all cases the same. For inclinations between 80° and 90° there was a sensible and increasing diminution in the photographic effect as the planes of polarisation of the polariser and analyser were more nearly at right angles to one another. It seems to follow from this that the full photographic effect on the dry gelatine plates used ensues when the intensity of the light reaches a certain limit, but that for intensities of light beyond that limit there is no sensible increase in the effect until the stage of solarisation is reached.

Chemical Society, March 15.—Dr. Gilbert, president, in the chair.—Dr. Gilbert will resign the presidential chair at the end of the session.—The Council have proposed Dr. W. H. Perkin to fill the vacancy, and Mr. J. Millar Thomson to be Secretary.—The following papers were read:—On some condensation-products of aldehydes with aceto-acetic ether and with substituted aceto-acetic ethers, by F. E. Matthews. The author has studied the following reactions: condensations of aceto-acetic ether with isobutylic aldehyde, valeric aldehyde, chloral furfurol, acrolein; of benzoic aldehyde with aceto-diethylacetic ether, aceto-dichloracetic ether, and aceto-benzilidene-acetic ether, and of benzoic aldehyde with aceto-mono-ethylacetic ether.—Contribution to the chemistry of "Fairy Rings," by Sir J. B. Lawes, Dr. Gilbert, and Mr. Warington. The authors have analysed samples of the soil inside the ring, on the ring, and outside the ring. The soil inside is much poorer in organic carbon and nitrogen than the soil outside the ring; the soil at the ring itself is intermediate in character as to carbon and nitrogen, but contains a larger quantity of nitrates. The fairy ring fungi seem to derive and assimilate nitrogen from the soil; this nitrogen is eventually deposited as manure at the ring, and becomes available to the associated herbage, which thereby acquires the characteristic dark-green colour.—On lines of no chemical change, by Dr. Mills and Mr. D. Mackey. The authors have investigated the strength at which sulphuric acid ceases to attack zinc at certain temperatures.—On homologous spectra, by W. N. Hartley. The author has photographed and mapped the spectra of various elements belonging to the same homologous series, *e.g.* magnesium, zinc and cadmium, calcium, strontium and barium, &c., especially with a view to finding out whether the striking similarity in such spectra was due to harmonic vibrations of a common fundamental vibration. The author concludes that the data contained in the paper support the view that elements whose atomic weights differ by a constant quantity, and whose chemical character is similar, are truly homologous, or in other words, are the same kind of matter in different states of condensation.

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[1] Cornu ("Spectre Normal du Soleil," p. 23, *note*) mentions that such films of silver are transparent for rays about $\lambda = 270$, which is a good deal too high. Chardonnet (*Comptes Rendus*, February, 1883) states that the band extends from O to S. W. A. Miller (*Phil Trans.* 1863) noticed that a silver reflector failed to reflect a band in the ultra-violet.

Linnean Society, March 1.—Sir John Lubbock, Bart., president, in the chair.—The following gentlemen were elected Fellows of the Society:—W. B. Barrett, L. J. K. Brace, J. B. Bridgman, W. O. Chambers, W. E. Clarke, W. Godden, F. H. H. Guillemard, J. C. Havers, T. M. Hocken, C. H. Middleton Wake, James Stirling, and Rev. P. W. Wyatt.—Two pieces of North American yellow pine were exhibited for Mr. R. M. Middleton, which displayed on their surface a great number of depressions like fine shot holes. These were doubtfully supposed to be produced by insect depredations.—Mr. W. T. Thiselton Dyer called attention to and made remarks on the dried leaves and rind of the fruit of oranges from the Bahamas, partially destroyed by the *Mytilaspis citricola*, Packard.—Mr. R. F. Towndrow showed examples of a new variety of *Rosa stytosa*, obtained at Madresfield, near Malvern, by Mr. A. D. Melin. This variety is evergreen, and its fruits ripen in the second year.—Mr. Alfred W. Bennett read a paper on the constancy of insects in their visits to flowers.—Then followed a communication on the methodic habits of insects when visiting flowers, by Mr. R. M. Christy, see notice (p. 498).—The Secretary, Mr. G J. Romanes, read some observations on living Echinodermata. He stated that star-fish possess a sense of smell which is not localised in any particular organs, such as the ocelli, but is distributed over the whole of the ventral surface. The function of the Pedicellariæ was shown by some further experiments, corroborative of those already published by him in the *Philosophical Transactions*, to be that of seizing upon and arresting the movements of fronds of seaweed in order to give the pedicels time to establish their adhesions. It was also shown that the righting movements of echinus, when inverted on its aboral pole (which are performed by means of the pedicels) are due to central coordination proceeding in part from the pentagonal nerve-ring surrounding the mouth, and in part from central nerve-matter distributed along the course of the radial nerve-trunks. One of the experiments whereby the fact of such central coordination (depending on a sense of gravity) was proved consisted in rotating an inverted echinus upon a wheel moving in a vertical plane. It was found that whatever phase in the righting manœuvre the echinus might have attained at the moment when the rotation commenced was maintained so long as the rotation continued, but the manœuvre was resumed so soon as the rotation was allowed to cease. The paper concluded with an account of the effects of the various nerve poisons on the Echinodermata.—There followed in abstract the 17th part of the Rev. R. Boog Watson's memoir on the mollusca of the *Challenger* expedition; therein he deals with the family Pyramidellidæ, describing twenty-three new species of the genus *Eulima*, and one of the genus *Stylifer*.

Geologists' Association, March 2.—Mr. W. F. Stanley read a paper upon the possible causes of the elevation and subsidence of the earth's surface. In this he offered an hypothesis that both the rising and sinking of land was entirely due directly or indirectly to the action of our great common motor, the sun. But most particularly for the greatest effects to the elevation of aqueous vapour, and to its after deposition as snow about the poles of the earth. The deposition of snow was assumed at the present time to reach a considerable altitude at the south pole, and in this position by its gravity to react as a pressure upon the interior mass of the earth, which was assumed to be in a highly heated viscous or semi-liquid state, and to be surrounded by a somewhat rigid crust of 200 miles or so in thickness. The crust was assumed to offer a certain amount of resistance to internal and external pressures, beyond which it was deflectable upon or from the viscous interior. The pressures from continued accumulation of snow at the poles acting as an hydraulic pressure upon the interior mass were assumed to be distributed in such a manner as was evident by elevation of land in volcanic and plutonic action, so that the earth could remain approximately under the conditions present, a symmetrical spheroid whose outward figure would constantly represent a natural resultant of the action of gravitation upon all its parts, and of the tangential force of such parts in revolution. It was argued that the stability of the land-surface was entirely due to *permanent elevation* by volcanic and plutonic action, and that if this did not exist the effects of atmospheric denudation would reduce the land surface within moderate geological time to a nearly level swampy plane. It was further discussed that if the interior of the earth is metallic, which has been reasonably inferred from its high specific gravity (about 5·6), then it would consist of a heat-conducting material, so that, beyond the non-conducting coating, which we term the crust, a certain degree of heat would be reached which might henceforth remain uniform throughout the interior mass. The crust would therefore be that portion of the exterior which was oxidised into a non-conducting coating in which the interior heated mass would conserve its heat with little loss. It was further argued that if the interior were a *viscous* mass the reaction of hydraulic pressure upon it, as from great accumulation of ice at either pole, would be made most evident about the most deflectable parts of the crust, so that the central mass might remain static, and if this was assumed by the presence of enormous pressure to form a practically incompressible semi-liquid, it would in this state possess enormous rigidity. Mr. Stanley further discussed the conditions of continuity of volcanic action throughout all time that the earth has existed as a cooling globe with a solid crust accumulating ice at either of its poles, and that the periods of greatest glaciation at either pole would be the periods of greatest volcanic eruption and elevation. Dr. Croll's theory of displacement of the earth's centre by polar glaciation was shown not entirely to coincide with observation, in that the coast of Greenland was sinking, and the coast of Norway, in the same latitude, was rising whereas by this theory of displacement of the earth's centre, the present accumulation of ice at the south pole should cause both of these parts to be rising equally. Mr. Stanley held that the cause of the coast of Greenland sinking was the weight of the present accumulation of ice upon that continent, which represented on a small scale a polar pressure system such as he had discussed.

Royal Horticultural Society, March 13.—Sir J. D. Hooker, K.C.S.I., in the chair.—*Potato-disease*: Dr. Masters read a portion of a paper on this subject forwarded to him by Mr. A. Stephen Wilson, and having especial reference to the "sclerotia" which Mr. Wilson has discovered in nearly all the organs of the adult plant, as well as in the seedlings and tubers. The sclerotia are supposed to germinate and lie in a state of incubation in the haulm. Ultimately they give rise to the conidial threads. The conidia form, according to circumstances, either (1) zoospores, (2) plasm granules, or (3) secondary conidia. These are succeeded by oospores and a non-parasitic mycelium, from which latter, as it creeps through the soil, are thrown out "floats" and specks of the seminal plasm. The seed-tuber comes into contact with the plasm in the soil, which is absorbed and becomes developed in the shape of sclerotia, and thus the life-cycle is completed. From the tuber or seed to the conidia is the parasitic arc. From the conidia to the tuber is the non-parasitic arc. The author illustrated his position by what happens in the case of cereals, wherein the plasm, say, of smut or rust, is absorbed by the cells of the scutellum or cotyledon, passes through a period of gestation and then germinates. Mr. G. Murray observed that a microscopical examination did not clearly reveal any organic connection between the sclerotia and the peronospora mycelium, and thought that possibly they might prove to be glandular bodies of some kind, and belonging to the potato itself. Moreover they could not be true sclerotia in the fungoid sense, as the latter are a compact mycelium.—*Retinospora pisifera* and *R. plumosa*: Mr. Noble sent a specimen exhibiting sprays of both of these supposed species on the same plant. Dr. Masters remarked that the latter is the young form, while the former is the adult, and that a microscopical examination showed a correspondingly different distribution of the stomata, being more numerous in *R. plumosa*.—*Juniperus Chinensis*: He also sent a male spray taken from a female plant; the sexes in this species being normally quite distinct.—*Garrya elliptica grafted on Aucuba Japonica*: Mr Noble forwarded a specimen showing the stock and the graft united. Mr. Henslow observed that this was an instance where physiological affinity corroborated the morphological; in that while Endlicher had placed Garrya between the hop and the plane, Bentham and Hooker assigned its position in the "Gen. Plantarum" next to Aucuba; but the discovery of its power of grafting on Aucuba was purely accidental, having been made by a gardener in Mr. Veitch's nurseries.—*Carica*, hybrid: Mr. Green, gardener to Sir G. Macleay, sent ripe fruits and foliage of a plant grown from seed furnished by M. Van Volxem of Brussels. It is a hybrid of the second generation, the first being raised from *C. erythrocarpa*, impregnated with the pollen of *C. cundinamarcensis* (from Colombia). From the fruit of this cross seedlings were raised, which were impregnated with pollen from the last named species, or from the hybrid itself. Some of the fruits supplied by Mr. Green contained apparently good seed. Mr. Henslow has tried the effect of the foliage on meat, that of the "Papaw," *C. papaya*, having the well known property of rendering it tender. He wrapped a piece of steak in a leaf for twenty-four hours, and it was quite effectual in softening it, and when cooked was pronounced excellent, though some thought there was a somewhat peculiar flavour as compared with a similar piece not wrapped up.

Manchester

Literary and Philosophical Society, January 9.—H. E. Roscoe, F.R.S., &c., president, in the chair.—Dr. Joule said that he had, in December, 1882, made a fresh determination of the freezing-point in a sensitive thermometer constructed thirty-nine years ago. During that time the point had risen about 1° Fahrenheit, and although now rising very slowly, was not even yet quite stationary, having risen 1/40 of a degree Fahrenheit since November, 1879.

January 23.—J. P. Joule, F.R.S., vice-president, in the chair.—Remarks on the simultaneous variations of the barometer recorded by the late John Allan Broun, by Prof. Balfour Stewart, F.R.S.—A paper was read entitled "Jeremiah Horrox and William Crabtree, the Observers of the Transit of Venus in 1639," by Mr. John E. Bailey, F.S.A.

February 6.—Prof. Balfour Stewart, F.R.S., in the chair.—Note on the vapours of incandescent solids, by Henry Wilde.—Remarks on Prof. Osborne Reynolds' paper on isochronous vibrations, by Robert Rawson, Hon. Member. Assoc. I.N.A., Mem. of the London Mathematical Society.

February 20.—H. E. Roscoe, F.R.S., &c., president, in the chair.—Mr. R. D. Darbishire, F.G.S., read a note upon the Mammoth Cave, by Mr. G. Darbishire.

Berlin

Physiological Society, February 23.—Prof. Du Bois Reymond in the chair.—Prof. Lucae, induced by the perception of a low noise when, in the open country, a strong wind blew against his ear, has long experimentally studied this phenomenon, investigating sounds and noises which arise on blowing into the external auditory meatus. He observed in normal ears which were closed with a sound tympanic membrane a moderately high noise, the pitch of which could not exactly be determined. When the tympanic membrane was stretched, the noise was somewhat higher and piping; when, on the other hand, the tympanic membrane was broken through or was absent, so that in the experiment the large air-space formed by the middle ear with the large cellular air-spaces beyond was blown into, he then heard a very deep noise. This great difference between the proper tone of the external auditory meatus and that of the large irregularly-formed air-space behind the membrane Prof. Lucae has verified both in all suitable patients and in dead bodies. An estimate of the relation of resonance of the ear cavities was obtained when, upon a spherical resonator which gives the tone $c_{,}$ on blowing, a short open cylinder was placed, which, blown into separately, gives the tone $c_{,,}$; when this combination was jointly blown into, the considerably deeper tone H was heard. When, however, between sphere and cylinder, a stretched membrane of caoutchouc was introduced, and the system blown into, there was heard again a higher tone, $f_{,}$. The influence here exercised by the degree of tension of the membrane could not be determined. To bring this schema of the air-spaces of the ear still nearer to the natural conditions, dry sponge was placed in the spherical resonator, the cavities of this material corresponding to the bone-cells communicating with the middle ear; the pitch of the tone on blowing was not thereby much altered. The determination of the proper tone of the tympanum and the influence of these conditions on audition are further engaging the author's attention.—Dr. Pohl Pincus had explained at a previous meeting of the Society that in the non-vascular frog heart two groups of muscular fibres with different action must be distinguished. The one class of fibres surrounds the fissures of the heart-wall, which perform the function of the vessels and admit the nutritive liquid to the tissue (vessel-muscles); the others, by their regular contractions and dilatations, act in the way of moving the blood (proper heart-muscles). The contraction of the first kind of muscles closes the fissures and produces paleness of the heart-wall, and their dilatation opens the fissures, lets the blood penetrate into the substance of the heart, and reddens the heart-wall; while the action of the second group of muscles produces systole and diastole of the heart. Now the actions of these two kinds of muscles—the heart-vessel muscles and the proper heart-muscles—are not simultaneous and similar under the influence of local stimuli, removal of the brain, section of the spinal cord in different places, and poisons; sometimes the heart-walls were observed to be pale in diastole and deep red in systole, and there were various other local differences of behaviour. This led the author to seek also an anatomical difference of the two groups of muscles, and he found one such on microscopical examination, for the proper heart-muscle fibres were cross-striped throughout and had long cell nuclei, whereas in the others the cross-striping did not comprise the whole width of fibres, and the nuclei were oval. With this anatomical difference the different mode of reaction of the two kinds of muscles and their different function is intelligible.

Vienna

Imperial Academy of Sciences, January 4.—The following papers were read:—G. Haberlandt, on the physiological anatomy of milk-tubes.—T. Wiesner, on the entering of the winter-buds of creeping blackberry-shoots into the soil, and on the mechanical cause of this process.—F. Rathay and B. Haas, on Phallus and Caprinus.—A. v. Obermayer, on diffusion of gases (third paper).

January 11.—F. Enrich, on the action of bile acids on albumen and peptones, and on their antiseptic effects.—T. Haubner, on the logarithmic potential of an uninsulated elliptic plate.—A. Lieben and S. Zeisel, on the products of condensation of propionaldehyde and its derivatives.—F. Anton, determination of the orbit of the Cassandra planet (114).—T. Ehrmann, on the formation of adipose tissue by the fat-organs, named winter-sleep-glands.

January 18.—C. Rabl, contribution to the history of development of Prosobranchiata.—F. Brauer, systematic studies based on the Diptera-larvæ, with a description of new species (third part).—R. Andreasch, on the oxidation of bases obtained by the action of halogen-compounds on thio-urea.—T. Freydl, note on the dry distillation of tartaric and citric acid with an excess of lime.—C. Pelz, on the determination of the outlines of warped screw-planes.—G. Goldschmidt, on the products of decomposition of the anhydrides of salicylic acid by distillation.—F. N. Dafert, on amylbenzol.

February 1.—W. Biedermann, contributions to general nerve and muscle physiology (tenth communication); to the knowledge of secondary contraction.—A. Belohoubek, on crystallised potassium hydroxides.—T. Blaas, contributions to the knowledge of natural water containing double sulphates.—T. Hepperger, determination of the orbit of Schmidt's nebulæ.—M. Kretschy, on the oxidation of kynurine and kinurenic acid.

CONTENTS

Page
The American Association. By Prof. T. G. Bonney, F.R.S. . . . 501
Pringsheim's Botanical Year-books. By Prof. W. R. McNab . 502
Our Book Shelf:—
  Brocklehurst's "Mexico To-day" 503
Letters to the Editor:—
  The Matter of Space, II.—Prof. A. S. Herschel (*With Diagram*) 504
  Mr. Stevenson's Observations on the Increase of the Velocity of the Wind with the Altitude.—E. Douglas Archibald . . . 506
  On the Formation of Mudballs.—Prof. G. H. Darwin, F.R.S. .. 507
  Snow Rollers.—G. J. Symons, F.R.S.; F. W. Grey 507
  Incubation of the Ostrich.—Prof. H. N. Moseley, F.R.S. . . . 507
  Holothurians.—J. G. Grenfell 508
  The British Circumpolar Expedition.—Dr. J. Rae, F.R.S. . . . 508
  Meteor.—E. Brown 508
  Mimicry.—Dr. Paul Henry Stokoe 508
  Threatened Extinction of the Elephant.—Edward E. Prince . 509
  A Curious Case of Ignition.—M. 509
Singing, Speaking, and Stammering, I. By W. H. Stone, M.D., F.R.C.P. 509
Acclimatisation of Edible Mollusks. By Dr. J. Gwyn Jeffreys, F.R.S. 510
The Alfianello Meteorite 511
The Shapes of Leaves, IV. By Grant Allen (*With Illustrations*) 511
Fossil Algæ (*With Illustration*) 514
Notes 515
Our Astronomical Column:—
  The Comet 1883 *a* 517
  The Minor Planet No. 228 518
  Binary Stars 518
Electrical Transmission of Force and Storage of Power. By Dr. C. William Siemens, F.R.S. 518
Fauna and Flora of the Aleutian Islands 520
Physical History of the Dead Sea, the Jordan Valley, and Palestine 520
Scientific Serials 521
Societies and Academies 521

THURSDAY, APRIL 5, 1883

FIRE-FOUNTAINS

Fire-Fountains; the Kingdom of Hawaii, its Volcanoes, and the History of its Missions. By C. F. Gordon Cumming. In two vols. 8vo. (Edinburgh and London: William Blackwood and Sons, 1883.)

MISS GORDON CUMMING has, in the work before us, given a most lively and interesting account of the Sandwich Islands. The large amount of experience which she has gained during five years of almost continual travel among the islands of the Pacific has enabled her to make careful comparisons between the physical features, the productions, and the populations of the different groups. In her two previous works, "A Lady's Cruise in a French Man-of-War," and "At Home in Fiji," our authoress has given us her impressions of Tahiti and the Fiji Islands respectively.

It is evident that Miss Gordon Cumming's first sentiments on arriving in the islands were those of disappointment. In productiveness, in the picturesque character of their scenery, in the beauty of their coral reefs, and in richness of flora, the Hawaiian Islands must certainly yield the palm to the Archipelagos of the Pacific. Even Kilauea itself failed to satisfy the traveller's expectation, for at the time of her first visit the fires of Halemaumau seemed to be almost extinct. Fortunately these first feelings of disappointment were to some extent removed by what the authoress subsequently witnessed during her long sojourn in the country.

The title of "Fire-Fountains" may perhaps lead a geologist to anticipate a more than usually exact account of the volcanic phenomena of these interesting islands. The extreme liquidity of the Hawaiian lavas enables them—as Dana, Brigham, Coan, and others have so well shown—to be thrown up into actual "fountains," and such jets have been witnessed both in Kilauea and Mauna Loa, rising to the height of several hundred feet. Any expectations of scientific accuracy in the account of the volcanic phenomena are, however, dispelled when we turn to the work itself. Miss Gordon Cumming's descriptions are wonderfully graphic, and a small amount of geological training would have enabled her to avoid popular errors, and employ accurate instead of misleading terms, thus making them valuable records of the phenomena she witnessed. Unfortunately, as in so many similar cases, this small amount of previous training was wanting.

The first part of the work consists of descriptions of the physical features of the group and of the characteristics of the inhabitants, and here the authoress largely relies upon her own observation, and furnishes us with many instructive comparisons with Tahiti and Fiji.

The second part of the book, which contains a history of the islands and of missionary enterprise in them, is of course compiled from published works, the information thus acquired being supplemented by facts derived from independent sources, such as letters and conversations.

The visit to Kilauea has been so often described that it may seem difficult to understand how any ordinary traveller can find anything new to say on the subject. But Miss Gordon Cumming had the good fortune (though she does not seem to have appreciated it at the time) to see the crater under somewhat exceptional conditions, as the following account will show (vol. i. pp. 164, 165):—

"After traversing three miles of this strangely varied lava-bed we reached the base of that inner circle of crags which within the last few months have been thrown up all round the central crater—*i.e.* the Halemaumau. So rapidly have they been upheaved, that they now form a ring 600 feet in height; and up this steep ascent we had to climb in order to look into the Lake of Fire.

"It was a toilsome ascent over very brittle lava; but Roback kept cheering me by telling me what a grand sight awaited me, and that he had never seen the lake in finer action than last week. So we climbed over coils of huge hollow vitreous lava-pipes, which constantly broke beneath our weight, and over ridges which looked to me like gigantic sugarsticks pulled out and twisted—and at last we gained the summit, and looked eagerly for the much-described Lake of Fire.

"THERE WAS NONE! at least nothing worth speaking of, in the first instance. I turned to look at my guide, and he stood staring in stupefied, bewildered amazement. He could not believe his own eyes. Only a few days had elapsed since he had led a party of Americans to the very spot where he now stood beside me in speechless wonder at the change.

"They had watched the blood-red waves dashing in scarlet spray against the cliffs on the farther side of the lake of molten fire, then rushing back to form a mad whirlpool in its centre, and thence, as if with a new impulse, flinging themselves headlong into a great cavern which undermined the lava-terrace just below the spot where I was now standing."

This was written on October 29, 1879, but three days afterwards the authoress has a very different state of things to chronicle (vol. i. pp. 186-189):—

"*November 1st.*

"Last night was Hallowe'en—the great fire-festival of our ancestors—and here it has been celebrated in right royal style, for the fire-spirits have broken loose and are holding high revel.

"The flow is increasing rapidly and is magnificent. The fire has burst out at so many points together that it has formed a new lake in the outer crater, in which fire-jets are spouting and molten lava thrown high in mid-air, great masses of red-hot solid lava being tossed to a height of from forty to fifty feet, while from the overflowing rim, or from weak points in the sides of the lake-basin, flow rivers of lava, forming a network of living, rushing fire, covering fully two square miles of the very ground over which I was walking only two days ago. It is a scene of marvellous beauty and is inexpressibly fascinating.

"From the edge of the crater-wall I have watched each stage in the growth of this strange new lake. I have seen it gradually rise higher and higher, till at last it overflowed in glowing streams, like rivers of golden syrup, but brighter far—an indescribable colour. The centre of the lake is oftenest of a silvery grey, only crossed by zigzag lines of flame colour and deep rosy red; but all round its shores it is continually surging and upheaving great crested billows, which break in fiery surf and toss up clouds of fire-spray. Sometimes the whole lake appears to be in a tremendous commotion—heaving and trembling as if acting obedient to some pressure from the furnace below.

"About a dozen cones have formed in and around the lake, each a distinct fire-fountain, yet all flameless—only merrily flinging about the molten metal: a bouquet of rare fireworks.

"These cones are miniature volcanoes—spouting liquid lava in the most sportive manner, playing gracefully like true fountains—spouting like intermittent geysers, and falling in showers of red hail—sometimes silently, sometimes with puffing and spluttering, varied with a roar like an angry bull; then a hush, followed by low moaning sobs.

"Some of these explosive forces have not built themselves chimneys, or, if they have, the lake has melted them, for they only betray their existence by suddenly bursting beneath the surface, like torpedoes, and tossing up red rockets.

"From the crag above I looked down upon a heaving, restless expanse of dull red almost entirely coated over with a silvery-grey scum, intersected by flowing rivers of red gold. The ceaseless movement beneath the surface kept up a glancing, gleaming play of white and red light, glistering like quicksilver in motion. Sometimes there came a swirling eddy, like the rush of a Highland stream.

"Then, again, the lava seemed to writhe and twist as if in agonised contortions, and then commenced a violent boiling and bubbling preparatory to its bursting into active fire-fountains These play sometimes singly, sometimes alternately, sometimes a dozen burst into simultaneous action—like some marvellous display of rockets, flinging their fiery rain on every side, then dying away altogether, till the silvery coating spreads so evenly over the surface of the lake, that, but for the sulphureous exhalations and columns of smoke, it might almost be mistaken for some cool refreshing pool. In truth, the white vapours which play so eerily among those black rock-masses, might well be morning mists floating upward from a quiet mountain-tarn.

"This, however, is a delusion not to be cherished for long, especially towards sunset; for then the lake appears in its true glory, and all the wonderful chemical colours which were lost in the full light of day reveal themselves, the difference of the scene before and after sundown being that of any huge smelting works, as seen by day or by night, only magnified ten thousand times. Then the scale of colour varies from deepest chocolate, crimson, and scarlet, to orange, yellow, and primrose tints, and the silvery grey becomes tinged with pink and violet, while the solid rocks become ever more intense in their blackness; and the many-tinted sea plays around them, and dashes over them, and from time to time detaches some huge fragment, which falls with thunderous crash, reverberating from crag to crag.

"As the twilight faded away, my kind landlord rigged up blankets and lanterns to make me a snug sketching-point on the hill above this house, whence I could watch the glory undisturbed, and attempt to preserve notes in colour, which may give you and others an idea, however faint, of the amazing scene before me. A full moon added its cool, pure light to the lurid crimson glow, which was reflected on all the overhanging clouds, as well as on the column of white steam which for ever rises from the Halemaumau itself; and these clouds, being visible at a distance of many miles, must have declared plainly to our friends in Hilo that there was unusual activity at Kilauea."

The authoress of this work did not reach the summit crater of Mauna Loa, but at the end of her book she has collected from various sources a tolerably complete account of the great outbursts of 1880 and 1881.

The details given in this volume concerning the aboriginal inhabitants and their manners and customs—or rather, we should say, of the total want of the former and the utter "beastliness" of the latter—is interesting to the anthropologist. The judgments of the authoress upon historical questions are by no means unfair, and if she does not follow American writers in treating Capt. Cook's visit as an act of piracy and his fate as a just retribution, she clearly points out that the death of the great navigator followed as a natural consequence of the sad misunderstanding between the English and the natives. From the traditions of the natives we can now fill in many details of the story, and explain certain matters which Cook, in his total ignorance of the language of the people, could scarcely guess at. In this and in the subsequent transactions between the English under Capt. Vancouver, and the Hawaiians, it must be confessed that the natives were treated with but scant justice at the best, and in too many instances with wanton cruelty and tyranny.

The admirable illustrations of this work constitute one of its most valuable features. They are reproduced by the autotype process from the sketches of the authoress. The frontispiece, showing the low rounded dome of Mauna Loa, with Kilauea on its flanks, is one of the best representations of this most wonderful district which we remember to have met with. The indefatigable traveller who has now become an acknowledged favourite with the public may be heartily congratulated upon the success of this latest production of her busy pen and pencil.

OUR BOOK SHELF

Africana, or the Heart of Heathen Africa. By the Rev. Duff Macdonald. 2 vols. (London: Simpkin, Marshall, and Co., 1882.)

NOTWITHSTANDING a large amount of professional commonplace, this work rises considerably above the level of ordinary missionary productions. The author, who administered the Church of Scotland Mission at Blantyre, south of Lake Nyassa, during the years 1878-81, applied himself diligently to the study of his dusky flock, and has embodied his experiences chiefly in the first volume, devoted to the "native customs and beliefs." The second is occupied more specially with "mission life," and with the inevitable difficulties and troubles entailed upon the writer in consequence of his accepting a position which from the first he felt to be untenable.

Since his enforced retirement from active work, Mr. Macdonald has usefully occupied his time in arranging for publication some of the rich materials collected during his stormy missionary life. Most of these materials, being the result of original observation in a new field not yet disturbed by contact with Europeans, possess great scientific value. The descriptions of the native manners, customs, beliefs, superstitions, and traditions are as interesting as they are trustworthy, and they are supplemented by two appendixes, which may be specially commended to the attention of all lovers of folk-lore. These comprise numerous selections of original "native tales" and "cosmical tales," literally translated from the author's manuscript collection of tales, songs, enigmas, &c., the whole of which it is to be hoped he will be induced to publish. Some of the tales accounting for natural phenomena have at least the merit of brevity, as, for instance, that about the wind: "A great man had a daughter, and she said, 'Father, in this country I am hot, I sweat.' Then her father said, 'Come here, my child, I have pity, I will blow with my breath.' So he blew, and thence came wind" (i. 283).

It is sad to learn that trial by ordeal and torture is still as universally practised as it was in Europe during mediæval times. "When a Magololo suspects his wives, he places a stone in a jar of boiling water or oil, and orders them to fetch it up with their bare arms. He then judges of their guilt by the amount of injury they sustain. When a woman is thus convicted, he makes her confess who seduced her. In vain does the helpless creature protest that she is innocent. Notwithstanding that her arm is severely scalded, she is subjected to the most cruel torture by a kind of thumbscrew (*mbanilo*), which is applied to her head. A small tree is partly divided along the middle, the skull of the poor woman is inserted as if it were a wedge for splitting the tree still farther. Great pressure is exerted by forcing the halves of the tree together with the aid of pulleys" (i. 201). This of course has the wished-for effect, and as in the "processus inquisitorii," the wretched victims "dum propria sua confessione contra se pugnare coguntur sui ipsius proditores tortæ constituuntur."

A. H. K.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I READ with interest, in NATURE, vol. xxvii. p. 362, the reply made by Dr. Romanes to a letter of mine which, although not originally addressed to a scientific organ, found hospitable reception in your columns. It was not much out of place there, for it was essentially an inquiry whether certain inferences may or may not *scientifically* be drawn from certain premisses. I am not wholly without hope of making it clear that the criticisms which I ventured to bring forward are grounded in reason; and confining my rejoinder strictly to the issue joined, I may hope not to be long nor very tedious. Let me trust that no curtness of statement will imply any want of the great respect which I entertain for an able investigator and writer, whose view may be imperfectly apprehended, or may bear an interpretation I should accede to.

The issue is a narrow one, and there is no need to widen it. Dr. Romanes is understood to derive from scientific premisses the conclusion that evidence of design is not legitimately derivable from the structure and adaptations of plants and animals, and, more particularly, that the theory of natural selection has destroyed the evidence of special design in organic nature, so that now the facts of organic nature furnish no other and no better evidence of design than do the facts of inorganic nature.

The first of these conclusions was derived from the proposition that there is no point of logical contact between natural science and the idea of design, wherefore no inference can legitimately be carried from the facts of the one to the conceptions of the other. I suggested that the maintainer of that position could not consistently argue that a particular scientific theory has annihilated an inference admittedly beyond its logical range. The reply is that, "If a man believes that there is no logical connection between one thing and another, I do not understand why he should be deemed inconsistent because he endeavours to show the fictitious character of the logical connection which has been erroneously supposed to exist." But the point of the objection was that, while insisting that any inference from the one to the other was invalid from the nature of the case, he actually inferred that certain scientific facts and theories completely overthrow and destroy the theory of particular design in organic nature. This may be. Only one would think that whatever may be legitimately overthrown may be as legitimately supported.

Moreover, if I rightly understand, there was not long ago a legitimate ground of inference (whether scientific in the narrower sense or philosophical need not here be inquired) from organic nature to design. "For it would be proof positive of intelligent design if it could be shown that all species of plants and animals were created"; and therefore proof presumptive while the theory of special creation was accepted and probable. At least—and this is the point—the argument from structure and adaptation to design was then admissible and even cogent.

Now, from the scientific side, upon which we are standing, special creation means only that the forms were scientifically inexplicable, and to be taken as original; their adaptations to their surroundings and their relations of means to ends in themselves equally as primary endowments. And whatever evidences of intellectual origination these manifested, *were seen in the things themselves*, and we suppose are to be seen there still. The inference was not one from an intellectual originator to design in the organic world, but from marks and operations in the latter which indicated design to an intellectual originator. The inference to most minds was convincing; at least it was legitimate. The recognised laws and operations of nature—a better knowledge of which has destroyed so many crude notions—were not thought to interfere with it.

It used to be so, but we have changed all that. How?

First, by the declaration of the principle that the facts of organic nature, in all their multiplicity and variety, yield no other and no better indications of design than do any of the facts of inorganic nature. That is to say, a stratum no more than a structure, a crystal than a chrysalis, living things and their responses than lifeless things simply acted upon, things which are intelligible only when contemplated as means and ends, no more than things of which ends are predicable, if at all, only by remote implication. Not only is the one as good as the other, but any one is said to be as good as all. Because of "the universal prevalence of laws and sequences of cause and effect, . . . they are not really or logically strengthened by a mere enumeration of particular instances. . . . The so-called law of causation as a whole being known, and its universality recognised, its true argumentative value to the theory of theism is not influenced by the explicit formulation of any number of its specific cases."

Here "law of causation," or the way how something comes to pass, is mixed up with "evidence of design," or what it was for. And we are to conclude that the immense variety and multiplicity of adaptations of particular means which accomplish particular ends in organic nature bring no contributory and cumulative evidence as to there being any design in them. In palliation of the charge of "damnable iteration," to which the teleologists are thus exposed, it may be pleaded that, although possibly one good witness or one good observation may be as convincing as many for certifying a fact, surely the more and the more varied the better for proving an underlying intervention—of which the evidence must always be circumstantial, and the conclusion a judgment or belief.

The old belief that adaptation of means to ends in plants and animals gives evidence of intellectual origination, had not been seriously unsettled by the scientific belief of the universality of the law of causation. It remains to be seen whether it will survive the establishment of the belief that the forms in which these adaptations are recognised have themselves been slowly evolved and diversified in a way that is partly explained by the doctrine of natural selection; and this is the gist of the question.

Dr. Romanes thinks that we have, in natural selection, "a cause other than intelligence competent to produce the adaptations," one which supersedes intelligence by working gradually. For, "if the adaptations have been effected gradually, and by *the successive elimination of the more favourable variations by a process of natural causation*, we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design." "The progressive adaptations of structures to functions by such a purely physical cause as natural selection, when once clearly revealed, must destroy all special or particular *evidence* of design, even supposing such design to exist." This phrase, "such a purely physical cause as natural selection," and the preceding phrase italicised by its author as specially significant and as being its equivalent, show that the term is used in its strict sense. So the substitute for intelligence, that which is said to account for all the adaptations in living nature, is the successive destruction of the less favourable variations by natural causes, leaving the most favourable to survive! Here "we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design,"—equally destitute, one would say, of any pretensions to act as its substitute until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done. Not till this, or something like it, is done, can natural selection pure and simple claim to give scientific explanation of the adaptations and the forms at whose birth it has assisted.

When I before insisted that "to make the purely physical explanation tenable it must be shown that natural selection scientifically accounts for the adaptation," and that it has not done this, that no reasons have been given why the organisms must have responded in the ways they do, or have responded at all to the environment, I meant only that the theory ought to fulfil the conditions which other physical theories are bound to satisfy, *i.e.* to account for the principal facts of the case. I had no reference to any subsidiary hypothesis which might help the matter. Dr. Romanes rightly says that it lies not with the evolutionist to show that variations may not have been intellectually planned or guided. But when he assigns the whole results to known physical causes and discards the factor of intelligence, he is bound to render their adequacy at the least conceivable.

It may now be seen, I trust (and the context might have made it clear), that, in asking Dr. Romanes if he was quite sure that any other cause than intelligence could adapt organisms to their environment gradually, I was not inviting him to guesses "about the possibilities of supernatural creation," but to a reconsideration of his antithesis between special (and as he will have it, sudden) creation, requiring intelligence, and gradual evolution, which might dispense with it; and I was intimating that he had not shown how the latter could dispense with it. The problem was: Given plants and animals with certain structures and certain adaptations to their environment, to be changed into other forms with other structures equally well adapted to a more or less changed environment, how to do this, solely by the action of said environment. Answer: By the killing out of all which have not somehow or other acquired the particular structure and adaptation they needed.

But now comes an important qualification: "The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may have occurred along favourable lines as responses of the organisms to their physical surroundings"; and Dr. Romanes calls my attention to a statement of his that it may be so in an essay which I regret that I have not read. He continues, however: "But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this: that they are of a physical or natural kind."

"Physical or natural kind." The agency which explained away all implication of design was in the strict sense physical, being the action of the environment on the organisms. It is now extended to whatever is *natural*, that is, to whatever occurs in the course of nature, presumably under established laws; and it is assumed that whatever so occurs is thereby void of all *evidence* of intellectual intention (we need not regard the difference—if any there be in such relations—between general and special design, the question being wholly one about the grounds of any *evidence* of design in nature). To me it is wholly probable that existing species and their special adaptations became what they are in the course of nature. And my argument is that, if "such a purely physical cause as natural selection" leaves these adaptations still unaccounted for, whatever implication of designed origination there formerly was still holds, and may hold, although the series of natural causes be practically endless.

Then as to such causes being all of a piece, so that pure physics may explain all biology. Doubtless in a certain sense all nature is of a piece. But in another sense—the very one we are concerned with—it is of at least two pieces; no matter how it came to be so. One of them is pervaded by an element of its own—that of *direction of action to ends*—which is more and more manifested as we rise in the scale of being, but is characteristic of all organisms. That seems to lay a foundation for a difference in the quality of the "inference which can be drawn by the human mind [*quoad* design] from the province of natural science." This difference might have made Dr. Romanes hesitate to draw, from scientific premisses, the downright conclusion that "the facts of organic nature present no evidence of design of a quality other or better than any of the facts of inorganic nature."

Here lies our whole contention. We agree that natural science leaves aside the question whether evolution and design in nature are compatible or not, this being only a phase of the enigma which was as puzzling before evolution was dominant as it is now. We suppose, too, that the difficulty of conceiving how design can coexist with the natural evolution of organisms is fairly balanced by the difficulty of conceiving how the phenomena of organic nature can be accounted for without it. The point which we have laboured over is that, if science has no call to settle the question, it has none to prejudge it. It was only because Dr. Romanes seemed to me unwittingly to have done so, that I ventured the criticisms which opened this discussion.

Cambridge, Mass., U.S. ASA GRAY

P.S.—A brief note upon Mr. Hannay's letter, NATURE, vol. xxvii. p. 364, referring to my supposition of successive generations slowly changing, "*yet always so as to be in compatible relations to the environment.*" He remarks, this "is just such a statement as 'Design' would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species?" Surely it could be held by the soundest of evolutionists, for it is of the very essence of Darwinism. Are not the individuals which compose the present fauna and flora in compatible relations to the environment, and is not the extinction of species going on? In human society do we consider that the unmarried and the childless members of the community are not in compatible relations to their surroundings? Is there any reason to suppose that the individuals of a flora of earlier times—say of the Miocene—were not on the whole in as orderly and compatible relations as the existing flora is? It is not *chaos* but *cosmos* that true Darwinism has in mind, common though the contrary impression be. A. G.

PROF. ASA GRAY is kind enough to remark that he has read my reply to his previous communication with interest. I should like to say, *in limine*, that I have read his reply to me not only with interest but with profit; for it is not often that one meets with an argument so carefully thought out and so clearly presented. Therefore, if I seek to meet his further criticisms, it is not in any spirit of controversy that I do so, but solely for the sake of endeavouring to help, so far as I am able, in determining the true logical position of an important question.

This question, as Prof. Gray observes, is a narrow one, and I shall keep to it. Without therefore trespassing upon the wider question of Theism as a whole, our discussion is confined to "an inquiry whether certain inferences may or may not *scientifically* be drawn from certain premisses."

First, I have to meet the dilemma which is put to me when I am told that, having said there is no point of logical contact between natural science and natural theology, I ought not forthwith to say that natural science is competent to destroy an inference belonging to natural theology. But in stating it as my opinion that natural science had shown the inference previously drawn to be invalid, I did not myself, as my critic asserts, draw any inference (even of a negative kind) from natural science to natural theology; I merely endeavoured to point out that an inference previously drawn from the one to the other was illegitimate, that inasmuch as the inference proceeded from natural science it was liable at any time to be overturned by natural science, and that it had now actually been overturned. Whether or not, therefore, I was right in saying that there is no point of logical contact between natural science and natural theology, at least I did not myself endeavour to institute such contact.

But I am told, you admit that long ago the inference in question was valid, and even cogent. Well, I answer in one sense it was, but in another and a truer sense it was not. For its cogency arose from the hypothesis of special and sudden creation on which it rested; grant this hypothesis, and the inference from organic adaptation to intelligent design becomes not only cogent but inevitable. The hypothesis, however, was not one that really belonged to natural science, and it was just this hypothesis that constituted the "fictitious logical connection" alluded to in the passage which Prof. Gray quotes from my previous letter. The facts presented by science remain, of course, very much the same as they were; but it does not follow that, in the absence of the special creation hypothesis, "whatever evidences of intellectual origination these manifested were *seen in the things themselves*, and we suppose are to be seen there still." Let us take an illustration. In the last issue of NATURE there is a letter from Prof. Darwin describing the formation of mudballs by a suitable and rare combination of natural causes. He and his brother did not see these balls in process of formation, and therefore he says, "On seeing the first one or two, they looked to us like the handiwork of some boy with an enthusiasm for mud pies"; but their number and the constancy of their situation on the slopes of hills—*i.e.* further knowledge of the inferred conditions of their origin—afterwards disposed of the teleological hypothesis in favour of a physical one. Now here it is equally true that "whatever evidences of intellectual origination these manifested were *seen in the things themselves*," and after the hypothesis of their physical origin had been arrived at, were "to be seen there still." Yet we should have deemed the brothers Darwin very unworthy representatives of their family if, after having arrived at the physical hypothesis, they had continued to argue in favour of a teleological enthusiasm for mud pies, on the ground that "the inference was not one from an intelligent originator to design in the (in-)organic world, but from marks . . . in the latter which indicated design to an intelligent originator." In other words, a change in the hypothesis concerning the *origination* of the mudballs entirely changed the logical cogency of the teleological inference.

Now I have purposely chosen this illustration because it is of so simple a character, and therefore serves in a clear manner to show how greatly a teleological inference may be modified by a change of hypothesis concerning the mode of origin of a structure, even though the structure remains the same; if there had been no evidence of a purely physical mode of origin in this case, it might truly have been said of the teleological interpretation, "the inference to most minds was convincing; at least it was legitimate." Of course in organic nature the apparent marks of design "in the things themselves" are much more numerous, varied, and complex than any that we meet with in inorganic nature; but no matter how numerous, varied, and complex such marks of design may be, if we see good reason to conclude that they have *all been produced by physical causes*, they are no more available as *evidences* of special design than are the mudballs—although both they and the mudballs, being alike formed under an orderly system of causation, may be due to a general design pervading the cosmos. And here I understand that Prof. Gray is in agreement with me, for he says that when I assign the whole results to known [or unknown] physical causes and discard the factor of intelligence, I am bound to render their adequacy at least conceivable. This appears to show that Prof. Gray is at one with me in holding that physical causes as such do not constitute other or better evidence of design in the organic than in the inorganic world; and it is only because he cannot conceive how such causes are adequate to produce the results observed in the former that he deems these results unique as evidence of "the factor of intelligence." In other words, supposing for the sake of argument that all these results have been due to purely physical causes, and supposing further that all these causes were as perfectly well known as the less complicated physical causes of the inorganic world, then I take it Prof. Gray would agree with me in saying that under such circumstances the former would constitute no other or better evidence of design than the latter.

If so, our only difference resolves itself into a difference in the estimate which we respectively form of the probable adequacy of purely physical causes to produce all the results which are observable in organic nature. To me the probability appears overwhelming that in respect of method "all nature is of a piece," and therefore that the terms "physical" and "natural," when applied to causation, are logically, as well as etymologically, convertible. To Prof. Gray, on the other hand, the probability appears to be that such is not the case, but that, when we meet with the "*direction of action to ends*," we have special evidence of "the factor of intelligence," which therefore makes nature "of at least two pieces," and so makes the term "natural" to mean more than the term "physical."

Supposing that I am right in understanding this as the only difference between us, I may point out that if, while following my ideas of probability, I have erred on the side of rashness in drawing "the downright conclusion" that the facts of organic nature present no other or better evidence of design than the facts of inorganic, Prof. Gray, in following his ideas of probability, can scarcely be able to shut out the suspicion (more especially in view of abundant historical analogies) that, in resorting to "the factor of intelligence" as a hypothesis wherever physical causation is found to be complex or obscure, he may be merely supplementing our present ignorance of such causation by an inference which is at least as rash as my statement.[1] And here I should like to observe, with special reference to the natural or physical causes summed up in the term "natural selection," that although I speak with all the respect which I sincerely feel for so distinguished a naturalist and so able a dialectician, I am not able to follow Prof. Gray in his understanding of this subject. For he says of the theory of natural selection that it is destitute of any pretensions to act as the substitute of the theory of special design, "until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done." But surely it is no part of the theory of natural selection to suppose that the *physical destruction* of unfit organisms is, or has any need to be, the *cause* of advantageous variations arising in other and allied organisms. The theory merely supposes that variations *of all kinds and in all directions* are constantly taking place, and that natural selection seizes upon the more advantageous. Therefore, so far as this theory is concerned, there is no call to explain why promiscuous variation occurs; it is simply a fact that it does occur, though not necessarily *made to occur* by the destruction of other organisms. Neither is there any call to explain why the variations occur in special and advantageous ways, for they are not supposed to occur in special and advantageous ways, but only to appear to do so on account of all other variations being eliminated, while those which happen to occur in the specially advantageous ways are preserved. Again, Prof. Gray says in his postscript that the theory of natural selection supposes successive generations to be slowly changing, "yet always so as to be in compatible relations to the environment." Now it is true that where the changes in the environment are gradual, and the variations of specific type are being slowly accommodated to them, each generation is, on the whole, in compatible relations with its environment. But it is not true that such continuous compatibility in itself points to design; it only points to the plasticity of the varying type, which, if not sufficiently plastic to meet the new demands upon it in this respect, simply becomes extinct.

In conclusion, I agree that "natural science leaves aside the question whether evolution and design in nature are compatible or not," and I agree that, "if science has no call to settle the question, it has none to prejudge it." But I do not agree that I have prejudged this question by saying that in my opinion the theory of evolution, in supplanting the theory of special creation, has necessarily removed the special evidence of design in organic nature, by showing that in respect of causation organic nature and inorganic nature are one. GEORGE J. ROMANES

The High Springs of 1883

THE high springs of the present year, consequent upon the excessive rainfall of the past winter, are an event that ought not to pass unrecorded in the pages of NATURE. I can speak only of phenomena which I have observed upon my native chalk hills of Hampshire, but I doubt not that similar facts have attracted attention elsewhere.

The Candover, a confluent of the Itchen from the north, burst forth this year in a field near Preston Candover, where it has not been known to rise for the last fifty years, and has flooded the road between Preston Candover and Chilton Candover. The Itchen itself rose in the valley above Cheriton beyond its recognised source, and has flooded fields on the road to Kilmeston, where no one recollects to have seen water before.

The Hampshire tributaries of the Thames have acted in exactly the same manner. The Whitewater has issued forth in the valley just below Upton Grey, far above its usual origin even in the highest springs, and has flooded the whole road between Bidden and Greywell. Another branch of the same stream has risen in the fields on the left of the main road from Odiham to South Warnborough, where spring water has never been known within the memory of the oldest inhabitant. In like manner the Wey, which, in wet seasons, takes its rise in the meadows adjoining Chawton House, has issued forth this year at a much higher level in the fields below Farringdon.

These facts are the more worthy of notice because it has been generally believed that, in the Hampshire hills at least, owing to more efficient drainage and other causes, the springs were getting lower every year, and would never again attain the level that they once had according to the traditions of past generations. It should be added that the springs were at their highest about the commencement of this month, and are now gradually falling.

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[1] I suppose it will be admitted that the validity of an inference depends upon the number, the importance, and the definiteness of the things or ratios known, as compared with the number, importance, and definiteness of the things or ratios unknown, but inferred. If so, we should be logically cautious in drawing inferences from the natural to the supernatural; for although we have the entire sphere of experience from which to draw an inference, we are unable to gauge the probability of the inference when drawn—the unknown ratios being confessedly of unknown number, importance, and degree of indefiniteness: the whole orbit of human knowledge is insufficient to obtain a parallax whereby to institute the required measurement or proportion between the terms known and the terms unknown. Or, otherwise phrased, we may say—As our knowledge of a part is to our knowledge of a whole, so is our inference from that part to the reality of that whole. Who, therefore, can say, even upon the supposition of Theism, that our inferences or "idea of design" would have any meaning if applied to the "All-Upholder," whose thoughts are not as our thoughts?

P. L. SCLATER

Hoddington House, Odiham, March 31

Scorpion Suicide

I AM sorry that my experiments on scorpion suicide has given pain to some of your correspondents. Allow me to explain in a few words the object of my investigation. It is commonly believed in this colony and elsewhere that scorpions commit suicide; Dr. Allen Thomson, in a letter to NATURE, lent the weight of his scientific name to this view; and Dr. G. J. Romanes, in his "Animal Intelligence," treats it as an open question. Now if this habit of committing suicide be an established fact, we have in scorpions a highly persistent type of creature that inherits a habit detrimental alike to the individual and the species. *Scorpion suicide, therefore, if a fact, is one of the strongest individual cases against the Theory of Evolution by Natural Selection that is presented to us in the animal kingdom.* It seemed to me that the only way of settling this question was by the direct appeal to experiment. But is the Theory of Natural Selection of sufficient importance in its bearing upon human life and human progress to justify the infliction of pain upon, say, sixty scorpions? I am one of those who believe that it is. I am one of those who believe that the theory of evolution has enormously influenced human thought and action, and is destined to influence it in a constantly increasing degree. I believe that much of the moral and intellectual progress of our race is indissolubly associated with this theory of evolution. I may be wrong in that opinion, but that is the opinion I hold. And holding that opinion it became to me a duty to do something towards settling a question which seemed to me to be of great importance in its bearing on the evolution theory. And it was my object to do the work, as far as I could, thoroughly and once for all. I believed that if I could show that even under torture scorpions do not commit suicide, the view that they do so when irritated by the bright light of a candle-flare became highly improbable. To establish a negative in the face of positive assertions is, however, difficult, and I considered it necessary to experiment upon a number of individuals. *Hinc illæ lachrymæ!* One of my friends, however, protested as follows: "The theory of evolution," he said, "is now so strongly established, that scorpion suicide is *a priori* impossible." But I hold it to be dangerous in the extreme, in the present position of science, to set up the theory of evolution as a doctrine from which to draw deductions, *unchecked by an appeal to nature where such appeal is possible.*

C. LLOYD MORGAN

Rondibosch, March 12

Nesting Habits of the Emu

I AM able fully to confirm Prof. Moseley's statement of the habits of the emu in nesting at Blenheim. Some years ago my father was very successful in rearing these birds at his place at Brockham Lodge, near Dorking. The first egg was usually laid shortly after Christmas; the total number of a brood being from fifteen to twenty, laid usually at intervals of about forty-eight hours. Some time before the full number was laid the cock bird would commence the incubation by carefully drawing them under him. When the hen bird was ready to add to their number she would sit down by his side, produce the egg, and her mate would then carefully draw it under him with his foot. As soon as the number was completed, it became necessary to seclude the hen bird, as she was from this time "vicious" towards her mate and towards her own eggs; and the seclusion continued until the young birds had attained a considerable size, as she showed every disposition to destroy them. The number of eggs laid was often too large for the cock bird to get comfortably under him. Still during several years that my father kept the birds a considerable number of eggs were annually hatched, and the young birds reared to the breeding age. No brood from native birds was, however, obtained. They showed no disposition to change the breeding season from January to July. In captivity the birds strikingly exhibited their singular inquisitive propensities. They were not usually vicious, except during the breeding season, but were very easily frightened.

London, March 31 ALFRED W. BENNETT

The Recent Cold Weather

THE excessively severe and prolonged cold weather of the month of March has hardly a parallel in this century. It appears to have been felt throughout Europe, and has even reached the shores of Africa. Frost, snow, and wintry gales we expect at a season proverbial for its fitful severity, but the scarcely interrupted sweep of the frigid atmospheric waves which have overwhelmed us for three successive weeks is an experience of weather so remarkable that I conceive the record will probably interest some of your readers.

In position, altitude, and in its freedom from the sheltering influence of large towns, this station may be accepted as favourable for giving an accurate account of the weather in the centre of England. Our instruments are on a proper meteorological stand, and are by Negretti and Zambra. I may add that, in its blighting influence on vegetation stimulated into activity by a mild and moist period in February, this weather has proved more destructive to early fruit blossoms, certain shrubs and plants accepted as hardy, than from any weather previously experienced in March in other years; but apart from vegetation, and acting on the upturned fallows and soddened clods of clay, the penetrating winds, frequent frosts and falls of snow have pulverised the land, so that it falls before the plough or harrow like calcined limestone, and in respect to the preparation of land the weather has had a beneficial action.

Record of Weather, March, 1883, at Belvoir Castle, Leicestershire

March.	Min	Max.	Grass.	Wind	Rain.	Snow.
4	27	50	27	S. to N.	—	—
5	27	51	20	N.	—	—
6	33	52	29	N.	—	0″·2
7	26	40	22	N.	—	0″·2
8	24	41	24	N.	—	0″·25
9	20	35	14	N.	—	0″·12
10	9	37	4	N.	—	0″·5
11	20	38	10	N.	—	—
12	25	39	23	N.W.	—	0″·2
13	25	39	20	W.	—	0″·1
14	29	40	22	W.	—	—
15	27	39	20	N.	—	0″·5
16	26	38	19	W.S.W.	—	—
17	28	38	24	S.W.	—	0″·9
18	25	40	20	S.	—	0″·1
19	28	42	21	N.	—	0″·1
20	31	40	31	E.N.E.	—	0″·31
21	32	37	31	N.E.	—	—
22	28	35	27	E.	—	—
23	28	35	26	N.E.	—	—
24	18	42	5	W.	—	—
25	26	45	16	N.W.	0″·4	—
26	26	41	19	N.W.	0″·5	—
27	27	40	18	N.	—	—
28	26	43	16	N.W.	—	—
29	24	41	12	S.	—	—
30	35	48	35	S.	0″·3	—
31	30	55	24	S.W.	0″·11	—

Belvoir Castle Gardens WILLIAM INGRAM

Sap-Flow

A REMARKABLE instance of the strong up-rush of sap in trees at this time of the year occurred here during the late severe weather. The boughs of a sycamore overhanging a road were trimmed on the 21st of this month during a very keen frost, and next day icicles of frozen sap, varying in length from a couple of inches to a foot, were hanging from the severed ends. The icicles were semi-opaque in appearance and slightly iridescent, like the sheen on the moonstone, and, when put in a bottle and melted, the product was pure sap.

The sycamore, being one of the earliest trees to develop leaves, had its sap rising, notwithstanding the intense cold and late season, while a beech, which is much later in coming out, and an ash, which is usually latest of all, whose boughs had also been lopped, showed no signs of bleeding, and the cuts remained dry and bare.

The icicles have been melted, reformed, and melted again since the 21st, and still the sap is dropping from the cuts.

Highfield, Gainsborough, March 28 F. M. BURTON

Foamballs

To artificial snowballs and mudballs will you permit me to add an experience of foamballs. We were staying at Biarritz in early spring, and one morning on going down to the beach we found it covered with such balls. A strong wind was blowing off the bay, which caught the wave-crests, and threw off little masses of foam. These, though quite small at first, accumulated, and, in some cases, conglomerated as they rolled inland, until they gradually attained a size of two to three feet in diameter; and as many of these balls of various sizes were drifted along by the wind, they presented a most singular appearance. This was made more curious by some of the town dogs catching sight of the objects, and taking to chevying them along the sand, until a sort of steeplechase was established. Every now and then a dog would overtake and dash into a flying sphere, only to find it, to his manifest disappointment, of a very unsubstantial character. The beach was covered far and wide with the debris of the broken balls.

Guildown, March 31 J. RAND CAPRON

Meteor; the Transit; the Comet

As you have on previous occasions deemed it of sufficient interest to record notices of striking meteors observed, I send you an account of a singularly brilliant and unusual form which appeared here about half-past 8 p.m. on the 29th inst.

I happened to be looking at a portion of the sky a little below the constellation "Orion," that is to the southward and eastward, when suddenly a brilliant meteor became apparent. Unlike ordinary meteors, it did not move, at least to my vision; it simply increased in size and brilliancy, till it appeared like a fine "Roman candle" or "blue light," intensely blue, and emitting rays at about two hundred yards' distance. It appeared to illuminate the country with a pale blue light.

It disappeared as suddenly as it came. Could its stationary appearance and increasing brightness have been caused by its *approaching* me in a direct line? I have thought so.

I saw the transit of Venus splendidly from my hilltop, through my binocular, an ordinary hand-telescope, and even with the naked eye, protected of course in each instance by coloured glass.

The comet also was a glorious object for several weeks. It was first seen here on September 23. I noticed very plainly the dark line near the right edge of the tail, as if there had been a *fold* in a luminous substance; that was the idea that the appearance gave me. Fig. 3, p. 610, vol. xxvi. of NATURE, most resembles what we saw here, but the shadow, or dark part, from the V-like incision at the end, should be longer and darker.

Not being a scientific observer, I did not trouble you with any notices of either, feeling sure you would have plenty.

British Consulate, Noumea, January 31 E. L. LAYARD

Ticks

CAN none of your readers be prevailed on to take up the study of the Ixodes (Ticks), of which there are several British species? I feel sure their life-history, if fully worked out, would prove both interesting and instructive, and might throw some light on a mysterious and deadly disease amongst cattle and sheep, which prevails extensively in Scotland, and in some districts in England. It is a curious fact that Ixodes are almost invariably, if not always found infesting sheep where this disease prevails, and it becomes an important question whether their presence is merely a coincidence, from the rough coarse natural grasses forming a congenial habitat, or whether they are not the carriers or inoculators of vegetable or other poison. I should be very glad to give further information to any one disposed to take up the study. W. E. L.

Ignition by Sunlight

"M." MAY like to have the following case:—I went once at sunrise (at Kishnagar, Bengal) into my coachhouse, which opened east. I saw smoke ascending from the tops of the two carriage lamps. I jumped hastily to the conclusion that my syce (groom) had been using the carriage candles illegitimately, and taxed him. His defence obliged me to examine closer, and to see that the two wicks had been ignited to smouldering point by the horizontal rays of the sun condensed by the parabolic reflectors at the backs of the lamps. A notable enough example of Indian heat, was it not? W. J. HERSCHEL

Collingwood, March 31

WHEN driving along the Beaumaris Road on Tuesday last at half-past three, I observed smoke issuing from the top of one of the carriage lamps. I stopped to examine the cause, and found that the reflector had concentrated the sun's rays on the wick of the candle lamp and caused it to smoulder.

Rhianva, Bangor, April 2 EDMUND H. VERNEY

Mimicry

REFERRING to Mr. Stokoe's letter in NATURE, vol. xxvii., p. 508, and to his remarks on the defective vision of the Teleostei as proved by the very poor imitations of insects which are sufficient to entrap them, have not bats and swallows—animals of certainly more than normal acuteness of vision—been hooked on several occasions by the flyfisher? H. J. MORGAN

Exeter, March 31

Braces or Waistband?

CAN you or any of your readers answer the following:—Which method of suspending the trousers is the least interference with *nature*—their suspension from the *hips* or from the *shoulders*, the wearing of braces, or a tight waistband? R. M.

March 16

SINGING, SPEAKING, AND STAMMERING[1]

II.—SPEAKING

IN the first lecture the musical and emotional side of human utterance; in the second, the colloquial and intellectual aspect of speech was adverted to. Speaking in modern times, and in England especially, is a more neglected art than singing. Even in Shakespeare's days there must have been a state of things not very dissimilar; for he makes Dogberry, who always manages to state the wrong proposition, say, "Readin' and writin' come by nature," and there is a quaintly satirical passage in that graceful and ethereal play, the "Midsummer Night's Dream," which goes straight to the point. Theseus, in commenting on the Clown's blunders of diction, says:—

> "Where I have come, great clerks have purposed
> To greet me with premeditated welcomes;
> Where I have seen them shiver and look pale,
> Make periods in the midst of sentences,
> Throttle their practised accents in their fears,
> And in conclusion dumbly have broke off,
> Not paying me a welcome."

It cannot be too often reiterated that speech is essentially an acquirement, and that it must be learned. At first, indeed, it is picked up by imitation in early childhood, and later on in life is commonly neglected and left to take its chance; though much can be done with little labour to correct defects both of this and of the handwriting, the two first things by which a man's intellectual status is judged of. It is unlike singing, in that pleasant and articulate speaking does not require the gift of a musical organ, but is open to all alike. There exists, however, in some quarters a prejudice against fluent speaking. Ineffableness is held to indicate grasp of thought; taciturnity to be the cloak of profundity. This would be correct if fluency were to supersede accuracy; but such an antagonism is by no means necessary, or it would reach its natural limit in the case of the sailor's parrot, which "could not talk, but thought the more."

Some other hindrances to correct speech require passing comment. In the first place its acquirement is too much mixed up with recitation and dramatic representation. Neither exaggeration nor servile imitation produce good speaking, the one salient feature of which is naturalness and spontaneity. Elocutionary teaching has also been hindered by an over-cultivation of poetical rhythm, which tends to reduce speech to a kind of singsong. The same may be said of punctuation, which is not elocutionary but grammatical; though the absurd rule has been formulated to "pause one for the comma, two for the semicolon, three for the colon, and four for the full stop." It is sufficient to test this pedantic error by reading any piece of nervous or pathetic English on the system, and thus to show its full absurdity.

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[1] Abstract by the Author of three Lectures at the Royal Institution, by W. H. Stone, M.B., F.R.C.P. Continued from p. 510.

It has been said above that whereas in singing the musical note is predominant, in speaking it is secondary and subsidiary to the words; but it still exists, and its function is well described by Cicero in his treatise, "De Oratore." He says, "Est in dicendo etiam quidam cantus obscurior." An appreciation of this fact is of the greatest value to the public speaker, since the imperfect regulation of the laryngeal element often renders the voice indistinct and even inaudible. Many speakers drop their voices with a descending inflection, and from want of musical ear fail to raise it again: others err from excess of noise, and in their anxiety to be audible, shout and labour, with the result of enveloping the significant sound in an overwhelming mass of heterogeneous and meaningless vibration.

It has several times been attempted to reduce speech to a definite musical notation like that of singing. To a certain extent this was done in the Ecclesiastical Plainsong; but it was carried to its extreme limit in a work of the last century, the "Prosodia Rationalis" of Joshua Steele. It is sufficient to glance at the vague and complicated symbols there employed to realise its practical uselessness.[1] Indeed, so far from being an advance, it is really a step of retrograde character. Mr. Deacon, in "Grove's Dictionary of Music," gives very clearly the four chief differences between song and speech:—1. The isochronism of vibration is never present long enough to make a musical note. 2. Little more than the lower third of the singing voice comes into play in speech. 3. In singing short syllables do not exist. 4. Singing tends to preserve intact purity of language; speaking, to split it up into dialects and idiosyncracies.

A common defect in speaking in large buildings is inability to catch the keynote or resonance vibration of the inclosed space. All large areas have such resonance notes, and in some it is very marked: Westminster Abbey, for instance, consonates to G sharp, and intoning on this note is much more audible than on one a semitone above or below it. Personally the lecturer prefers the use of an open chest-voice as little vocalised as may be. It is less laborious, less liable to accidents, less liable to develop the affection commonly known as "clergyman's throat," and, by removing the sensation of effort, more easy and sympathetic.

He then proceeded to analyse the constituents of a good delivery; and first, pauses. Haste is one of the commonest faults in speech. It has two defects; the one in overtaxing the complex muscular mechanism of the speaker; the other in adding to the intellectual labour of the listener. The former would be considered in the third lecture; the latter needed a few words. The rapidity of reception of ideas through the ear differs materially in different persons, even excluding those distinctly "hard of hearing." It is not great among the uneducated, whence it had been paradoxically said that all illiterate persons are deaf. But they do require a longer time to arouse them to a state of attention than the more cultivated. Naval officers had defended the practice of swearing, or as it was euphemistically termed, "shotting their speech," with sailors; the expletive rousing attention and preparing the mind for the succeeding command. Mr. Hullah had on a similar ground explained the refrains or fal-lal-las of the older music, that they dilute the too concentrated sense of the words, and give time for the perception of the music.

When the great actor Salvini was in this country in 1875, the lecturer made some experiments on this point. Salvini's voice was one of the most remarkable ever heard for its power of travelling; even suppressed phrases coming up to the distant gallery with perfect clearness. He spoke on a note about D in the bass, from the chest, and in a sort of recitative; there were distinct periods from accent to accent, and the inflections were very large, running over an interval of more than a fifth. The individual words came about one a second, and the pauses were astonishingly long. They frequently amounted to four, several times to five, and at the two great crises of the play to seven continuous seconds. And yet there was no sense of delay or of interruption, but quite the reverse. The lecturer incidentally noted another thing, which the recent development of Wagner's musical theories had invested with additional interest. In the play "Il Gladiatore," the four principal characters, a young Christian virgin, a Roman matron, the hero a Roman officer, and the gladiator, formed an unintentional though perfect vocal quartett of soprano, contralto, tenor, and bass. At times the alternations of dialogue produced a distinctly musical effect, an observation which to his mind strongly corroborated the views of the great musician lately deceased, that dramatic music, instead of being conventional, should be the outflow of passion and emotion, and that this result could be attained as well from the elocutionary as from the strictly melodic side.

Pronunciation, under which is included respiration as well as vocalisation, was then spoken of, schemes of the vowels and consonants by Dr. Bristowe and Melville Bell being distributed among the audience. The latter being unfamiliar in this country, may be reproduced in this abstract.

GENERAL VOWEL SCHEME. MELVILLE BELL.

Lingual.	Labio-Lingual	Labial.
1. Eel	U (German)	Ooze
2. In	U (French)	O (Provincial)
3. Ale	Û (French)	Old
4. Ill (Scotch)	Zur (Provincial)	Ore
5. Ell	Eu (French)	Awe
6. An	Er Ir (English)	Urge (Scotch)
7. Ask	Er Ir (variety)	Urge
8. ,,	Ah	,,

ARTICULATIONS OR CONSONANTS.

			Oral.		Nasal.
Obstructive. Complete contact.		… … …	P	B	M
			T	D	N
			K	G	Ng
Continuous	Approximation	Firm …	Ph	Bh	
			Rh	R (smooth)	
			Ch	Gh	
			Wh	W	
			S	Zh	
			Sh	Z	
			Yh	Y	
		Relaxed	KRh	Gr (burr)	
			Rh	R (rough)	
	Partial contact	… …	F	V	
			Th	Th	
			Ll (Welsh)	L	
			L (Gaelic)		

The aspirate was briefly described as being no fixed articulation, but simply a vowel sound first whispered and then pronounced aloud. Accent has for its object to make one syllable or several more prominent than those around. The English language tends to throw it as far back in a word as is practicable. A long word may have one strong, and one or even two weaker accents in it.

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[1] "King's College Lectures on Elocution," Plumptre p. 112.

Inflexion is either rising, falling, or a compound of these. As a rule, rising tones appeal, falling tones assert, compound tones suggest; a complete balance of the two is the antithesis, which can be heard in such a remark as "It was not so much what you said—as your manner of saying it, which struck me." The contrasted effect of the two accents may be reproduced by reading this sentence aloud and intelligently.

When inflexion is applied in this way to sentences, three cases occur: the sentence either asserts, asks, or orders, and the nature of the inflexion depends on the relative circumstances of the speaker and listener.

Delivery and modulation are combinations of pausing and of pitch. The conversational pitch being taken as a medium, all below this denotes sadness or solemnity; all above it joy or levity. Force, expression, and sentiment, thus developed, are infinite in their variety.

Emphasis can only be attained and regulated by a full perception of the point to be brought out; as a rule it marks the predicate of a logical expression. False emphasis is the foundation of many quaint stories in common currency. Speaking generally, new, contrasted, or antithetical ideas are marked by emphasis.

In conclusion, the lecturer gave three general rules by which any one can speak. The first, in the words of Horace: "Dicendi rectè principium est sapere, et fons;" that is, "Know exactly what you are going to say." The second, "Endeavour to forget yourself." This frame of mind had been formulated by old elocutionists as "Have a contempt for your audience." He preferred to state it in a less obnoxious way as "Consider yourself one of your audience." The third, "Be natural and unaffected."

By bearing in mind these simple injunctions any man free of congenital or acquired defects, though he might not be a brilliant, could hardly fail in being an agreeable and sympathetic speaker.

PROFESSOR SCHIAPARELLI ON THE GREAT COMET OF 1882

READERS of NATURE will be glad to have a full report of the interesting popular lecture which Prof. Schiaparelli, the well-known Italian astronomer, gave in Milan on February 4, on the great comet of 1882. Referring to the national misfortune which had given origin to his and other lectures, he began by showing that while a connection between the comet and the inundations which wasted, in October, 1882, many Venetian provinces, was not absolutely impossible, it was at least very improbable, both because the comet was yet a great distance from the earth when the floods rose, and from the difficulty of understanding why the supposed influence of the comet should have acted only on that little part of the globe. After this preamble M. Schiaparelli gave the public a rapid and elementary account of our planetary system, and of the comet's trajectory during its passage near the sun and planets. The orbit of the comet, in the position which could be subjected to astronomical measurement, is parabolic, in a plane inclined 30° or 40° to planes of the solar system. The greater portion of the orbit is in the southern regions; for in the austral hemisphere the comet was sooner and better observed than in the boreal, where it never was very high above the horizon. The vertex of the parabola is very near the sun, and only when the comet was approaching to this position with an extraordinary rapidity, astronomers could perceive it,—at Auckland (September 2), at the Cape of Good Hope, in Australia, the Argentine Republic, and Brazil. The direction of its movement was perhaps towards the sun; but the inconceivable rate which the comet acquired in its falling towards the sun (480 km. in a second, sixteen times the mean velocity of the earth in its orbit), and the lateral rush coming from it, were enough at that time to overcome the attractive power of the sun, and to hinder the great luminary from swallowing it. The attraction of the sun failed not to produce its effect, slackening successively its flight; but being animated by this great velocity, the comet could escape in security to where the sun's action is very feeble, and whence it will not return for many years.

The Cape astronomer had the opportunity of witnessing this rare spectacle of a heavenly body which, rushing headlong from extraplanetary depths, went directly on the sun, as if it would fall in, and notwithstanding, in a few hours delivered itself, changing completely its direction of motion. At that time the earth was placed very obliquely in respect to the arc described by the comet about the sun, so that astronomers observed it with a great foreshortening of perspective. In those hours the comet, being exposed to an extraordinary heat, swelled and became so luminous, that the Cape astronomers, and afterwards some in Europe, could see it near the sun. They could make the absolutely new observation of a comet's transit before the solar disk, thus satisfying an ancient desire of astronomers, who have wished to know if in those bodies' head, which often appears as a very bright star, is hidden an obscure perceptible nucleus, and to judge of the density of the shining atmosphere whose splendour produces the star's appearance. In this case it was not possible to be deceived by an illusion, as happened in 1819. Messrs. Finlay and Elkin, at the Cape, saw the comet gradually approach the sun's limb, touch it, and disappear; so that their searches to find the comet in the place where it obviously was were vain. The comet then was so thin and clear, that the most slender cloud would more obscure the sun: its solid nucleus (if it had a nucleus, as was very likely) was so small that the observer's telescope could not perceive any spot or shade. After it left the neighbourhood of the sun, the effects of the enormous heat began to appear in the development of that splendid tail, which everybody could see in the morning hours of October and November.

The orbit of the comet (continued the Professor) is not easily deducible from the very little portion which we know. Both because to assign a trajectory observed in a small branch is very difficult, and sometimes impossible, and because exact and definitive calculations will not be undertaken before the vanishing of the comet; the notes which at present can be given are only approximative. On observations of last September, October, and November, it was stated that the period of the comet is included between eight and nine centuries, and the aphelion is nearly six times farther than Neptune from the sun (175 times the earth's mean vector radius), the rate of velocity in aphelion and perihelion being as 1 to 23,000.

On the brightness of the comet, M. Schiaparelli observed that it could be attributed to three causes: the strong illumination of the sun, its own light, and electrical discharges, which take place continually in similar bodies, in the opinion of expert physicists. Those causes united to make that very splendid appearance of a matter clearer and less dense than the rarefied air of our best pneumatic engines. The density of the tail was so small that an astronomer estimated it at no more than a few kilogrammes, while its dimensions were larger than were ever before observed in comets. It is true that other comets (that of 1861, for example) showed an apparently longer tail, their position in respect to the earth being more favourable to observation; but in the annals of astronomy we have never found a comet's tail really as long as this.

I leave out the detailed description of the comet's aspect, because NATURE has given full accounts and sketches, and I come to the most interesting part of M. Schiaparelli's lecture, on the production of those magnificent phenomena. I translate literally from Signor Schiaparelli's manuscript.

The proper nucleus of the comet is a solid or liquid body so small as rarely to be seen: in the greater number of comets, as in this, it seems to be not large enough to be visible even in powerful telescopes. It seems also that in some comets there are several nuclei, very small and close, whose particular atmospheres in their development at last unite in one. As long as such a body (or system of bodies) remains far from the sun, in extraplanetary regions, where temperature is less than – 140° C. (according to the most moderate estimates), and where the sun has perhaps no power to heat it, the matter must be wholly solid or at least liquid; and, if a small quantity is gaseous or vaporous, it must have a great density and a small volume. The progressive approach to the sun by its descending orbit will obviously swell the enveloping atmosphere, or give rise to one if it does not yet exist, with materials generated by the surface. Shortly the nucleus begins to appear surrounded by a blaze of light, feeble at first, but afterwards more and more brilliant, which is the star or head of the comet Many comets do not go beyond this first phase, both because they have not matter enough to make an atmosphere, and because they do not come near enough to the sun to be subject to a great heat. Some comets do not enter the earth's orbit, others cannot reach that of Mars, and we know that the comet of 1729 got only a little way into the orbit of Jupiter. The most part of those comets, being exposed very moderately to the solar influence, cannot increase, and remain telescopic; and it is very probable that a large number stop at Jupiter or Saturn's orbit (or even further) in their descent upon the sun: none of those are seen, and we can speak of them only by conjecture.

When a comet, however, as the present, pierces through the interior part of the planetary system, it is in the best condition to develop its atmosphere if it contain matter enough to do so. But the sun, while attracting to himself the nucleus, has the property to repel some of the matter of the atmosphere. It is not well known how and why this matter is repelled, and to expound the various hypotheses on this point would take too long a time. The effect of repulsion is nevertheless undoubted, and manifested by the fact that those parts of the cometary atmosphere, under the sun's impulse, almost as if under a gale blowing from it, leave the nucleus and fly in an opposite direction away from the sun, producing the tail, which, nourished successively by incessant evaporations of the nucleus, more and more increases in length, till the atmosphere of the nucleus, wholly repelled, overflows into the tail, and thus exhausts itself. This happens usually when the nucleus, after the perihelion, receding from the sun and being then exposed again to the cosmical cold, is no longer able to supply with new evaporations the part of the atmosphere which the tail absorbs. Deprived thus of its former envelopes, and unable to engender others, the nucleus is reduced again to itself, and the comet disappears.

The tail of the comet consists then of matters repelled by the sun with a mysterious power. But, during the above described period of conflagration, other interesting events occur in the comet. It is so much swollen and convulsed by solar heat that the little nucleus is not able sometimes to keep together the fragments by its own very feeble attraction. Violent eruptions take place at the surface, so that pieces of nucleus are raised and thrown out of the principal body's attraction. Those fragments then pass through the heavens as independent bodies, and their orbits are not very different from the orbit of the nucleus. Sometimes one of the broken pieces is great enough to engender another separate comet: that is, the several times observed phenomenon of a divided comet. But most generally it seems that separated pieces are very small and numerous, like the sparks of a piece of salt thrown on the fire; and extend along the trajectory of the nucleus like a current or projection of corpuscles, which gradually invade all the orbit of the comet. Many comets (probably all) engender in their course a similar retinue; and the planetary intervals are peopled by those corpuscles produced by a comet's partial disintegration. When the earth in its yearly revolution passes through one of these processions it meets with several pieces, which get inflamed by contact with the terrestrial atmosphere, and burn in a very short time, producing a falling star.

An example of such a process of separation was given by the present comet. In effect, a little before October 15 M. Schmidt, the astronomer at Athens, observed an irregular and very feebly shining thin cloud leaving the comet, withdrawing, and finally disappearing. It was more dense and luminous in some places than in others, but it looked not like a comet, having rather the aspect of a mass of corpuscles exploded by the principal nucleus. The atmosphere also enveloping the principal nucleus offered analogous phenomena, being not round and symmetrical, but lengthened spindle-fashion, with several more luminous centres of different intensity spread in an oval cloud. We have, besides, reason to believe that another little comet, which was observed in the beginning of 1880 in the austral regions of the earth, running in an orbit very similar, was previously separated from our great comet.

M. Schiaparelli passed afterwards to another question, on the chemical constitution of comets, explaining the principle of spectrum analysis and its application to celestial chemistry. He remarked that the present and Wells's comet only, by their coming so near the sun, could present the lines of sodium, whilst all the comets before observed gave only lines of hydrocarbons in the spectroscope; and it is very probable, according to modern theories, that comets contain also some matters which are made apparent in falling stars and in aërolites, as iron, nickel, silicium, magnesium, aluminium, and others. This confirms the induction as to the similarity of their chemical composition to that of the earth; and the common origin of comets in the planetary system is evidently proved by their accompanying the sun in its progressive movement towards the constellation of Hercules. It seems that comets belonging to the solar system would have the function of continually dissipating matter in space, as a compensation to the attractive power of the greatest centre, the sun.

Pressure of space obliges me to leave out the very eloquent conclusion of this lecture, in which the lecturer refuted the apprehensions as to the shock of a comet with the earth, and its probable consequences, discussing the great moral importance of these studies as an antidote to the fears and superstitions of ignorant people. Referring to Anaxagoras and Galileo, he concluded with these words: "A science which suffered such noble condemnations, and is able to awake such noble hopes, cannot be considered as futile and idle; it will always be dear to the friends of truth; dear to every one who is convinced that man lives not by bread alone." FRANCIS PORRO

R. Observatory of Brera in Milan

THE SOARING OF BIRDS

THE recent correspondence in NATURE upon this subject ought not to close without some reference to a possible explanation of *soaring* which does not appear to have been yet suggested.

I premise that if we know anything about mechanics it is certain that a bird *without working his wings* cannot, either in still air or in a uniform horizontal wind, maintain his level indefinitely. For a short time such maintenance is possible at the expense of an initial relative velocity, but this must soon be exhausted. Whenever therefore a bird pursues his course for some time without working his wings, we must conclude either (1) that the course is not horizontal, (2) that the wind is not horizontal, or (3) that the wind is not uniform. It is probable that the truth is usually represented by (1) or (2); but the question I wish to raise is whether the cause suggested by (3) may not sometimes come into operation.

In NATURE, vol. xxiii. p. 10, Mr. S. E. Peal makes very distinct statements as to the soaring of pelicans and other large birds in Assam. The course is in large and nearly circular sweeps, and at each lap some 10 or 20 feet of elevation is gained. *When there is a wind*, the birds may in this way "without once flapping the wings" rise from a height of 200 to a height of 8000 feet.

That birds do not soar when there is no wind is what we might suppose, but it is not evident how the existence of a wind helps the matter. If the wind were horizontal and uniform it certainly could not do so. As it does not seem probable that at a moderate distance from the ground there could be a sufficient vertical motion of the air to maintain the birds, we are led to inquire whether anything can be made of the difference of horizontal velocities which we know to exist at different levels.

In a uniform wind the available energy at the disposal of the bird depends upon his velocity *relatively* to the air about him. With only a moderate waste this energy can at any moment be applied to gain elevation, the gain of elevation being proportional to the loss of relative velocity squared. It will be convenient for the moment to ignore the waste referred to, and to suppose that the whole energy available remains constant, so that however the bird may ascend or descend, the relative velocity is that due to a fall from a certain level to the actual position, the certain level being of course that to which the bird might just rise by the complete sacrifice of relative velocity.

For distinctness of conception let us now suppose that above and below a certain plane there is a uniform horizontal wind, but that in ascending through this plane the velocity increases, and let us consider how a bird sailing somewhat above the plane of separation, and endowed with an initial relative velocity, might take advantage of the position in which he finds himself.

The first step is, if necessary, to turn round until the relative motion is to leeward, and then to drop gradually down through the plane of separation. In falling down to the level of the plane there is a gain of relative velocity, but this is of no significance for the present purpose, as it is purchased by the loss of elevation; but in passing through the plane there is a really effective gain. In entering the lower stratum the actual velocity is indeed unaltered, but the velocity relatively to the surrounding air is *increased*. The bird must now wheel round in the lower stratum until the direction of motion is to windward, and then return to the upper stratum, in entering which there is a second increment of relative velocity. This process may evidently be repeated indefinitely; and if the successive increments of relative velocity squared are large enough to outweigh the inevitable waste which is in progress all the while, the bird may maintain his level, and even increase his available energy, without doing a stroke of work.

In nature there is of course no such abrupt transition as we have just now supposed, but there is usually a continuous increase of velocity with height. If this be sufficient, the bird may still take advantage of it to maintain or improve his position without doing work, on the principle that has been explained. For this purpose it is only necessary for him to descend while moving to leeward, and to ascend while moving to windward, the simplest mode of doing which is to describe circles on a plane which inclines downwards to leeward. If in a complete lap the advantage thus obtained compensates the waste, the mean level will be maintained without expenditure of work; if there be a margin, there will be an outstanding gain of level susceptible of indefinite repetition.

A priori, I should not have supposed the variation of velocity with height to be adequate for the purpose; but if the facts are correct, some explanation is badly wanted. Mr. Peal makes no mention of the circular sweeps being inclined to the horizon, a feature which is essential to the view suggested. It is just possible, however, that the point might escape attention not specially directed to it.

However the feat may be accomplished, if it be true that large birds can maintain and improve their levels without doing work, the prospect for human flight becomes less discouraging. Experimenters upon this subject would do well to limit their efforts for the present to the problem of gliding or sailing through the air. When a man can launch himself from an elevation and glide long distances before reaching the ground, an important step will have been gained, and until this can be done, it is very improbable that any attempt to maintain the level by expenditure of work can be successful. Large birds cannot maintain their levels in still air without a rapid horizontal motion, and it is easy to show that the utmost muscular work of a man is utterly inadequate with any possible wings to allow of his maintenance in a fixed position relatively to surrounding air. With a rapid horizontal motion, the thing may perhaps be possible, but for further information bearing upon this subject, I must refer to a paper on the resistance of fluids published in the *Philosophical Magazine* for December, 1876.

March 22 RAYLEIGH

PHILIP CHRISTOPH ZELLER

ENTOMOLOGY has just sustained an irreparable loss by the death of Prof. Zeller, which took place at Grünhof, near Stettin, on March 27, suddenly, from heart disease. Zeller was born on April 9, 1808, at Steinheim, in Württemburg. For many years he was attached to official educational establishments in Germany, especially at Glogau in Silesia, and Meseritz in Posen. While at the former place the honorary title of Professor was bestowed upon him by the Government on account of his eminent scientific researches, and some time afterwards he retired from official duties, and settled near Stettin, where much of his leisure was devoted to the Entomological Society that has its headquarters in that town, of which he was acting secretary, and of which Dr. C. A. Dohrn is president. Zeller's fame as an entomologist is more especially based upon his publications on *Lepidoptera*, more particularly of Europe, and chiefly on the smaller moths. His first recorded paper appeared in Oken's *Isis* for 1838, and consisted of a critical determination of the *Lepidoptera* in Réaumur's "Memoirs," a prize essay, in which the author took first place. From that time a continuous stream of valuable papers by him appeared, and on the day of his death he was engaged in scientific work. It is utterly impossible to give here even the titles of his more important works. It is with regret that we are obliged to admit that the title of "entomologist" does not always enable us to take for granted that the entomologist is also a naturalist. Zeller was both, in the fullest acceptation of the terms. While his purely descriptive work is of the highest character, his investigations into the natural history of his subject were persistent, and he never ceased to deprecate the "slop-work" so painfully evident in the writings of some entomologists. For many years he made almost annual excursions in pursuit of his favourite science, especially in the Alps of Central

Europe, and so long ago as 1844 a more extended tour in South Italy and Sicily. In this country he was so well known that British entomologists will feel that in his death they have lost one of themselves; it is nearly thirty-five years since he was elected an Honorary Member of the Entomological Society of London, and he was one of the editors of Mr. Stainton's magnificent "Natural History of the Tineina." There are those amongst us in this country who in Zeller's death have lost one of their dearest friends. Scientific entomology has lost one of its most shining lights. R. McLACHLAN

THE GREAT INTERNATIONAL FISHERIES EXHIBITION

HER MAJESTY THE QUEEN has recently appointed the 12th of May for the opening of the International Fisheries Exhibition, which an influential and energetic committee, under the active presidency of the Prince of Wales, has developed to a magnitude undreamt of by those concerned in its early beginnings. This magnitude is perhaps as great a matter of agreeable surprise to Mr. Birkbeck and its other Norwich founders as it will be to those who have very naturally become accustomed to class all specific exhibitions together upon a standard formed by the unfortunate annual exhibitions of which the public has, not without reason, grown weary.

The idea of an *international* Fisheries Exhibition arose out of the success of the show of British fishery held at Norwich a short time ago; and the president and executive of the latter formed the nucleus of the far more powerful body by whom the present enterprise has been brought about.

The buildings are well advanced towards completion, and will be finished long before the opening day; the exhibitors will, it is hoped, support the executive by sending in their goods in time, and thus all will be ready for the 12th proximo.

The plan of the buildings embraces the whole of the twenty-two acres of the Horticultural Gardens: the upper half, left in its usual state of cultivation, will form a pleasant lounge and resting-place for visitors in the intervals of their study of the collections. This element of garden accommodation was one of the most attractive features at the Paris Exhibition of 1878.

As the plan of the buildings is straggling and extended, and widely separates the classes, the most convenient mode of seeing the show will probably be found in going through the surrounding buildings first, and then taking the annexes as they occur.

On entering the main doors in the Exhibition Road, we pass through the Vestibule to the Council Room of the Royal Horticultural Society, which has been decorated for the reception of marine paintings, river subjects, and fish pictures of all sorts, by modern artists.

Leaving the Fine Arts behind, the principal building of the Exhibition is before us—that devoted to the deep sea fisheries of Great Britain. It is a handsome wooden structure 750 ft. in length, 50 ft. wide, and 30 ft. at its greatest height. The model of this, as well as of the other temporary wooden buildings, is the same as that of the annexes of the great Exhibition of 1862.

On our left are the Dining Rooms with the Kitchens in the rear. The third room, set apart for cheap fish dinners (one of the features of the Exhibition), is to be decorated at the expense of the Baroness Burdett Coutts, and its walls are to be hung with pictures lent by the Fishmongers' Company, who have also furnished the requisite chairs and tables, and have made arrangements for a daily supply of cheap fish, while almost everything necessary to its maintenance (forks, spoons, table-linen, &c.), will be lent by various firms.

The apsidal building attached is to be devoted to lectures on the cooking of fish.

Having crossed the British Section, and turning to the right and passing by another entrance, we come upon what will be to all one of the most interesting features of the Exhibition, and to the scientific student of ichthyology a collection of paramount importance. We allude to the Western Arcade, in which are placed the Aquaria, which have in their construction given rise to more thoughtful care and deliberation than any other part of the works. On the right, in the bays, are the twenty large asphalt tanks, about 12 ft. long, 3 ft. wide, and 3 ft. deep. These are the largest dimensions that the space at command will allow, but it is feared by some that they will be found somewhat confined for fast going fish. Along the wall on the left are ranged twenty smaller or table-tanks of slate, which vary somewhat in size; the ten largest are about 5 ft. 8 in. long, 2 ft. 9 in. wide, and 1 ft. 9 in. deep.

In this Western Arcade will be found all the new inventions in fish culture—models of hatching, breeding and rearing establishments, apparatus for the transporting of fish, ova, models, and drawings of fish-passes and ladders, and representations of the development and growth of fish. The chief exhibitors are specialists, and are already well known to our readers. Sir James Gibson Maitland has taken an active part in the arrangement of this branch, and is himself one of the principal contributors.

In the north of the Arcade where it curves towards the Conservatory, will be shown an enormous collection of examples of stuffed fish, contributed by many of the prominent angling societies. In front of these on the counter will be ranged microscopic preparations of parasites, &c., and a stand from the Norwich Exhibition of a fauna of fish and fish-eating birds.

Passing behind the Conservatory and down the Eastern Arcade—in which will be arranged Algæ, Sponges, Mollusca, Star-fish, worms used for bait, insects which destroy spawn or which serve as food for fish, &c.—on turning to the left, we find ourselves in the Fish Market, which will probably vie with the Aquaria on the other side in attracting popular attention. This model Billingsgate is to be divided into two parts, the one for the sale of fresh, the other of dried and cured fish.

Next in order come the two long iron sheds appropriated respectively to Life-boats and Machinery in motion. Then past the Royal Pavilion (the idea of which was doubtless taken from its prototype at the Paris Exhibition) to the southern end of the central block, which is shared by the Netherlands and Newfoundland; just to the north of the former Belgium has a place.

While the Committee of the Netherlands was one of the earliest formed, Belgium only came in at the eleventh hour; she will, however, owing to the zealous activity of Mr. Lenders, the Consul in London, send an important contribution worthy of her interest in the North Sea fisheries. We ought also to mention that Newfoundland is among those colonies which have shown great energy, and she may be expected to send a large collection.

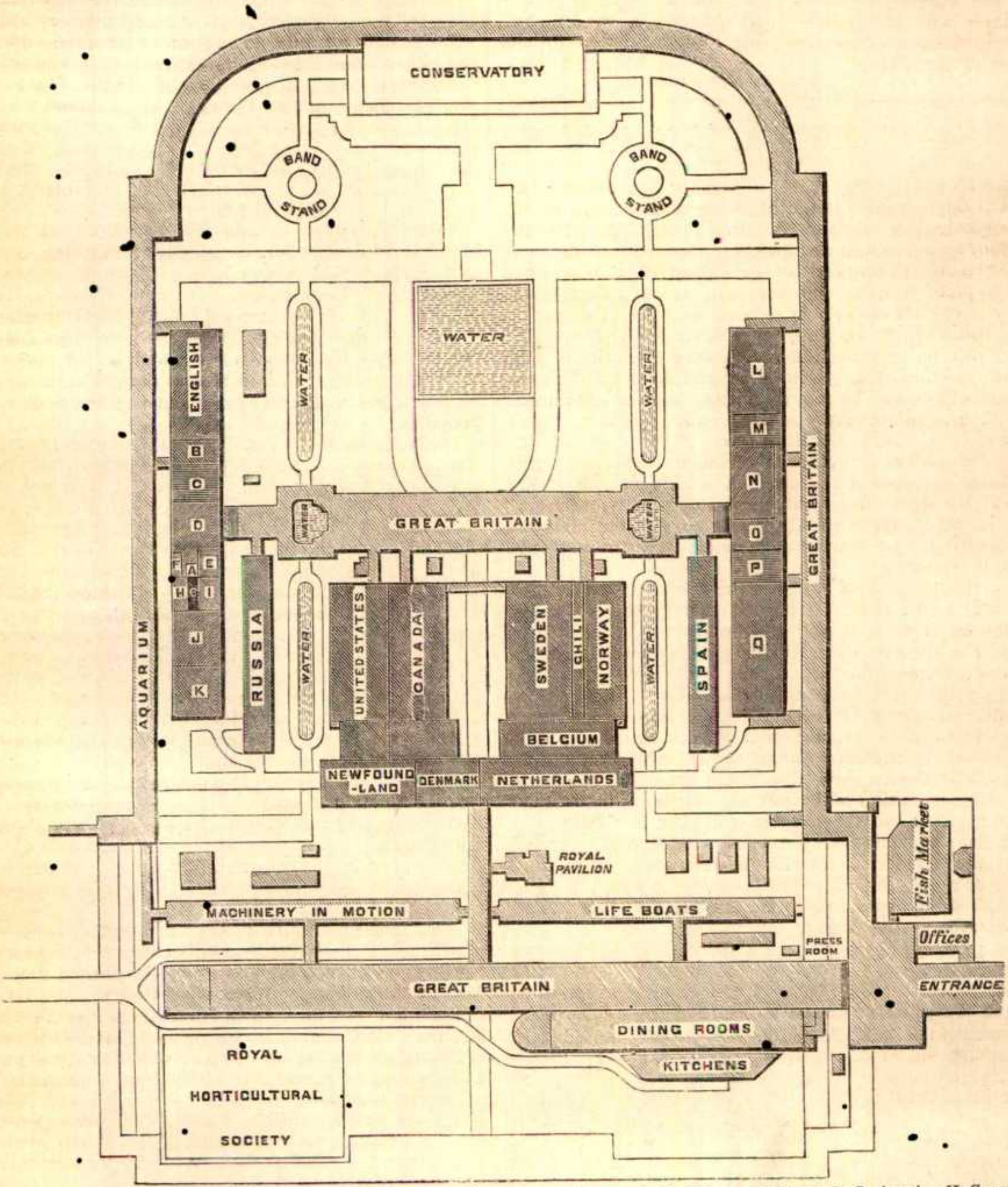
Passing northward we come to Sweden and Norway, with Chili between them. These two countries were, like the Netherlands, early in preparing to participate in the Exhibition. Each has had its own Committee, which has been working hard since early in 1882.

Parallel to the Scandinavian section is that devoted to Canada and the United States. While the American Government has freighted a ship with specimens expected daily, the former has entered heart and soul into the friendly rivalry, and will occupy an equal space—ten thousand square feet.

In the Northern Transept will be placed the inland fisheries of the United Kingdom. At each end of the building is aptly inclosed a basin formerly standing in the gardens: and over the eastern one will be erected the daïs from which the Queen will formally declare the Exhibition open.

Shooting out at right angles are the Spanish annexe, and the building shared by India and Ceylon, China and Japan, and New South Wales: while corresponding to these at the western end are the Russian annexe and a shed allotted to several countries and colonies. The Isle of Man, the Bahamas, Switzerland, Germany, Hawaii, Italy, and Greece—all find their space under its roof.

After all the buildings were planned, the Governments of Russia and Spain declared their intention of participating; and accordingly for each of these countries a commodious iron building has been specially erected.



Block Plan.—A. Switzerland; B. Isle of Man; C. Bahama and W. I. Islands; D. Hawaii; E. Poland; F. Portugal; G. Austria; H. Germany; I. France; J. Italy; K. Greece; L. China; M. India and Ceylon; N. Straits Settlements; O. Japan; P. Tasmania; Q. New South Wales.—Scale, 200 feet to the inch.

The Spanish collection will be of peculiar interest; it has been gathered together by a Government vessel ordered round the coast for the purpose, and taking up contributions at all the seaports as it passed.

Of the countries whose Governments for inscrutable reasons of state show disfavour and lack of sympathy, Germany is prominent; although by the active initiative of the London Committee some important contributions have been secured from private individuals: among them, we are happy to say, is Mr. Max von dem Borne, who will send his celebrated incubators, which the English Committee have arranged to exhibit in operation at their own expense.

Although the Italian Government, like that of Germany, holds aloof, individuals, especially Dr. Dohrn of the

Naples Zoological Station, will send contributions of great scientific value.

France, the other day only, consented to the official appointment of her Consul to look after the interests of the oyster cultivators who are contributing an important feature.

In the Chinese and Japanese annexe, on the east, will be seen a large collection of specimens (including the gigantic crabs) which has been collected, to a great extent, at the suggestion of Dr. Günther of the British Museum.

It is at the same time fortunate and unfortunate that a similar Fisheries Exhibition is now being held at Yokohama, as many specimens which have been collected specially for their own use would otherwise be wanting; and on the other hand, many are held back for their own show.

China, of all foreign countries, was the first to send her goods, which arrived at the building on the 30th ultimo, accompanied by native workmen, who are preparing to erect over a basin contiguous to their annexe models of the summer-house and bridge with which the willow-pattern plate has made us familiar; while on the basin will float models of Chinese junks.

Of British colonies, New South Wales will contribute a very interesting collection placed under the care of the Curator of the Sydney Museum; and from the Indian Empire will come a large gathering of specimens in spirits under the superintendence of Dr. Francis Day.

Of great scientific interest are the exhibits, to be placed in two neighbouring sheds, of the Native Guano Company and the Millowners' Association. The former will show all the patents used for the purification of rivers from sewage, and the latter will display in action their method of rendering innocuous the chemical pollutions which factories pour into rivers.

In the large piece of water in the northern part of the gardens, which has been deepened on purpose, apparatus in connection with diving will be seen; and hard by, in a shed, Messrs. Siebe, Gorman & Co. will show a selection of beautiful minute shells dredged from the bottom of the Mediterranean.

In the open basins in the gardens will be seen beavers, seals, sea-lions, waders, and other aquatic birds.

From this preliminary walk round enough has, we think, been seen to show that the Great International Fisheries Exhibition will prove of interest alike to the ordinary visitor, to those anxious for the well-being of fishermen, to fishermen themselves of every degree, and to the scientific student of ichthyology in all its branches.

The economic question of the undertaking we have left untouched.

NOTES

IT will be seen from a communication in another column that the Council of the British Association have virtually decided that that body is bound to hold its meeting in Canada in 1884. From Sir A. T. Galt's letter it is evident that our Canadian fellow-subjects have already arranged to give the Association a hearty and generous welcome; and now that Canada seems inevitable, we hope that as many members as possible will make up their minds to be present. The expenses for visitors will be reduced to a minimum, and the travelling expenses of officials, to the number of fifty, to *nil*. A magnificent programme for three weeks' excursions has been sketched, and the expenses connected with them will be confined to hotel charges, carriages, &c., the railway companies having handsomely offered to convey members free of charge.

THE Academy of Sciences held its Annual Meeting on April 2, M. Jamin in the chair. He pronounced the *éloge* of the three Academicians who died last year, viz., MM. Liouville, Bussy, and Decaisne. M. Blanchard, filling the room of M. Dumas, who, although present, was unable to deliver any speech, read the list of laureates. The number of prizes offered for public competition is yearly enlarging; not less than three of them—Monti, Machedo, and Francœur—were delivered for the first time. The number of verdicts which the commission had to render was thirty-three. In nine cases the commission declared no memoir was worthy to take a prize; the competitions were in general adjourned to 1885, and a certain sum of money was given to some semi-successful candidates. In two instances the merit of the candidates was acknowledged so great that two prizes were delivered instead of one. These two cases were in statistics and mathematics; the question put was to give a theory of the partition of numbers in five squares. Amongst the prizes lost is included the famous Prix Breaux, for the cure of cholera. The interest was divided amongst four pupils of M. Pasteur's. The Poncelet Prize has been taken by M. Clausius, and the Voltz Prize by Mr. Huggins and M. Crüls, a Brazilian, for their spectroscopic work.

IT was announced at the above-mentioned meeting that the great mathematical prize of the French Academy had been awarded to the late Prof. H. J. S. Smith for his dissertation on the representation of a number as the sum of five squares. The subject for the prize was announced in the *Comptes Rendus* of the Academy in February of last year, and, according to custom, the essays were to be sent in before June 1—each dissertation bearing a motto and being accompanied by a sealed envelope having the motto on the outside and the writer's name inside. The envelopes of the unsuccessful candidates are destroyed unopened. Prof. Smith's dissertation bore as its appropriate motto:—

> "Quotque, quibusque modis possint in quinque resolvi
> Quadratos numeri pagina nostra docet."

There were three candidates, and the value of the prize is 3000f. The theory of numbers, to which the prize subject related, is one to which Prof. Smith had devoted the greater part of his life, and in which he occupied an almost unique position; with the exception of Prof. Kummer of Berlin, there is no one whose contributions to the science could be compared to his, and this posthumous mark of the appreciation on the Continent of the value of his work is all the more satisfactory as the great prize has never before, we believe, been awarded to an English mathematician. The complete solution of the important problem proposed by the French Academy had been obtained by Prof. Smith sixteen years ago as part of a far more general investigation, and the results were published by him in the *Proceedings of the Royal Society* in 1868, but without demonstration. These researches seem, however, to have escaped the notice of the French mathematicians. When the subject of the prize was announced last year, Prof. Smith extracted from his manuscript books the demonstrations of the propositions relating to the five-square problem, and it is to the dissertation so formed that the prize has been awarded. No more striking instance of the extent to which Prof. Smith had carried his researches, or of his great mathematical genius, could be given than is afforded by the fact that a question considered by the French Academicians of so much importance to the advancement of mathematical science as to be chosen for the subject of the "Grand Prix" should have been completely solved by him as only a particular case in the treatment of a general and even more intricate problem. In 1868 Prof. Smith won the Steiner Prize of the Berlin Academy, so that had he but lived till now he would have been "laureate" of the Academies of both Paris and Berlin.

THE removal of the natural history collection from Great Russell Street to its new quarters at South Kensington, on the site of the Great Exhibition of 1862, has been proceeding gradually during the last two years, and is now rapidly approaching completion. Several of the rooms, formerly stocked with birds, fishes, &c., have been already emptied.

LIEUT. SAMUEL W. VERY, U.S.N., and Dr. Orlando B. Wheeler, the two principal members of the expedition sent by the United States Government to Santa Cruz, Patagonia, to observe the recent transit of Venus, arrived in Liverpool on Friday, by the Pacific Steam Navigation Company's mail steamer *Patagonia*. Lieut. Very, who had charge of the expedition as chief astronomer, states that the expedition arrived off the mouth of the Santa Cruz River on November 2. The weather during the first fourteen days was very encouraging, but this was succeeded by nine days of overcast, disagreeable weather, with frequent and sharp showers of hail and rain. Fine weather again followed until the eventful morning of December 6, which broke cloudy and hazy. By half-past seven a.m., however, the clouds began to weaken, half an hour later the sun shone out dimly, and as the day advanced the weather improved, so that when it was time to take up stations for the first contact, the sun was almost entirely clear. All four of the contacts were observed both by Lieut. Very, with the large equatorial, and by Mr. Wheeler, with a smaller one; and during the transit 224 photographs were taken, with a continuous improvement in the results. By sunset the weather changed again for the worse, and the sun was not seen, except at intervals, for four or five days, during which time Lieut. Very was looking anxiously for observations for rating his chronometers. While the expedition was in camp the temperature changed to the extent of 19° in the course of every twelve hours. In the daytime the heat occasionally was oppressive, while at night the air was very cold, and the party had to sleep with double blankets and heavy clothing upon them. The Lieutenant speaks in the highest terms of the kindness and consideration shown to him by the Pacific Steam Navigation Company and the Customs authorities, both of whom, when they were informed of his business, put all possible facilities in his power.

THE next ordinary General Meeting of the Institution of Mechanical Engineers will be held on Wednesday, April 11, and Thursday, April 12, at 25, Great George Street, Westminster. The chair will be taken by the president, Percy G. B. Westmacott, at three o'clock on Wednesday afternoon, and at ten o'clock on Thursday morning. The following papers will be read and discussed:—On the strength of shafting when exposed both to torsion and end thrust, by Prof. A. G. Greenhill, of Woolwich; On modern methods of cutting metals, by Mr. W. Ford Smith, of Salford; On improvements in the manufacture of coke, by Mr. John Jameson, of Newcastle-on-Tyne; On the application of electricity to coal mines, by Mr. Alan C. Bagot, of London.

THE 21st meeting of the delegates of the French Learned Societies took place last week at the Sorbonne. M. Ferry, the French Premier, presided over the final meeting on March 31, and delivered, as is customary, an address. The Minister dwelt much on the circumstance that he had added to the four sections in existence a fifth, devoted to political economy, so that the meeting of the Learned Societies included every subject in human knowledge. He praised the Trustees of the British Museum for their fair dealing towards France in the matter of the Ashburnham manuscripts, and eulogised the French Government for their zeal in the promotion of knowledge, declaring that 60 millions of francs had been already spent for the rebuilding of French universities, and that 40 millions were to be spent shortly for the same purpose. The presidents of the several sections omitted to deliver their reports, and the proceedings terminated somewhat abruptly. The address was well received, but the unexpected silence of the presidents has taken the public by surprise, and has been unexplained as yet.

M. HERVÉ MANGON, President of the Bureau Central of French Meteorology, opened the Session of the Congress of Meteorologists on March 29 by reading a report on the working of the institution. This document states that, from a comparison made by the Bureau, its forecasts have been acknowledged good 83 times in each 100; that for the warning of tempests 207 had been sent to the seaports, out of which 100 had been fulfilled entirely, 65 partly, and 42 had not been warranted by the event. The president, who is a member of the French Legislative Assembly for La Manche, announces the intention of asking from Parliament an augmentation of credit.

MR. MUYBRIDGE has issued a prospectus of "a new and elaborate work upon the attitudes of man, the horse, and other animals in motion." As the expense of conducting these experiments is very great, Mr. Muybridge naturally waits until he obtains a sufficient number of 100-dollar subscriptions before entering upon them. Each subscriber of the sum will receive a large album containing the photographic results of the experiments. Their scientific and artistic value is so great that we trust Mr. Muybridge will receive sufficient encouragement to put his plan into execution. His address is Scovill Manufacturing Company, Publishing Department, 419–421, Broome Street, New York.

THE Warwick Museum has been enriched by the very valuable collection of local Liassic and Keuper fossils formed by the late Mr. J. W. Kirshaw, F.G.S., which it is intended to keep as a separate collection. The whole of the collection in the Museum has lately been classified and arranged by Mr. R. Bullen Newton, of the Natural History Museum, South Kensington.

HARTLEBEN'S "Elektrotechnische Bibliothek" has been further augmented by three volumes. They consist of two little books by Dr. Alfred von Urbanitzky, viz. "Die elektrischen Beleuchtungs Anlagen" and "Das elektrische Licht," and one by Herr W. P. Hauck, "Die galvanischen Batterien, Accumulatoren, und Thermosaulen."

ACCORDING to latest accounts, the eruption of Mount Etna is resuming activity. Enormous quantities of gas are thrown out, and slight shocks are again felt in the neighbourhood of Nicolosi.

THE second number of *Timehri*, the journal of the British Guiana Agricultural and Commercial Society is to hand; it completes the first volume. Among the contents we note the following:—Forest Conservancy in British Guiana, by M. McTurk, G. M. Pearce, and the Hon. W. Russell; Mount Russell in Guiana, by the Editor, Mr. Im Thurn; The Aspect and Flora of the Kaieteur Savannah, by G. S. Jenman; Notes on West Indian Stone Implements, by the Editor, with several coloured illustrations; British Guiana Cave-Soils and Artificial Manures, by E. E. H. Francis. There are also several interesting notes, and the reports of the Society's meetings. Among the notes is a letter from Dr. R. Schomburgk, of Adelaide, giving some interesting autobiographical details. Stanford is the London agent.

WE have received the first number of the new American monthly, *Science*, to which we heartily wish all success.

WHILE Western Europe enjoyed a mild autumn, very severe weather was experienced on the Ural. At Ekaterinburg the average temperature of October was four degrees lower than the average for forty five years, that is, − 3°·9, instead of + 0°·9, the lowest temperatures in October witnessed since 1836 having been but − 2°·4 and − 3°·2. For nineteen days the thermometer did not rise above zero, and it fell as low as − 19°·2 and − 17°·9.

Entomologists generally, as well as those more particularly interested from their geographical position, will be pleased to learn that the long-expected Yorkshire List of Lepidoptera—on which Mr. Geo. T. Porritt, F.L.S., of Huddersfield, has for some time past been engaged—is now completed, and that the MS. is now being set up for the *Transactions of the Yorkshire Naturalists' Union*. Mr. Porritt, who has been assisted by the leading entomologists of the county, and who has also paid attention to the literature of the subject, has written what will probably be regarded as one of the best county catalogues of Lepidoptera extant. The diversity of soil and climate, geological and physical conformation, for which Yorkshire is famous, is once more illustrated by the richness in species which the lepidopterous fauna shows, 1344 out of the 2031 species recognised as British finding places in Mr. Porritt's catalogue.

The following occurrence is worth notice:—The Weymouth and Channel Islands Steam Packet Company's mail steamer *Aquila* left Weymouth at midnight on Friday for Guernsey and Jersey on her passage across Channel. The weather was calm and clear, and the sea was smooth. When about one hour out the steamer was struck violently by mountainous seas, which sent her on her beam ends and swept her decks from stem to stern. The water immediately flooded the cabins and engine-room, entering through the skylights, the thick glass of which was smashed. As the decks became clear of water, the bulwarks were found to be broken in several places, one of the paddle-boxes was considerably damaged, the iron rail on the bridge was completely twisted, the pump was broken and rendered useless, the skylight of the ladies' cabin was completely gone, and the saloon skylight was smashed to atoms. The cabins were baled out with buckets, while tarpaulins were placed over the skylights for protection. Fives minutes after the waves had struck the steamer the sea became perfectly calm. Several of the crew were knocked about, but none were seriously injured.

At 10 p.m. on March 27 an earthquake occurred in and around the town of Miskolcz, Hungary. There were two separate shocks, and so distinctly were they felt that in the theatre, where the performance was going on, a panic ensued, the entire audience rising and rushing in terror towards the outlets. Many persons were injured, but, happily, no lives were lost. An earthquake was observed on March 12 in various parts of Italy. Reports now state that it was principally noticed in the Pellice valley, in the Po district, at Gessi, Varcita, Stura, and Coni. The direction of the shocks was from N.E. to S.W. In the plains the shocks were far less severe than in the mountains, where the foundations of the houses were shaken. Nobody, however, was hurt.

An interesting discovery has been made at St. Pierre Quiberon (department of Morbihan). It consists of a new dolmen, one of those stone monuments of grey antiquity. It contained several entire human skeletons, besides a number of skulls, stone axes, a bronze pin, and some fragments of vessels.

The large gold Cothenius medal, which the Imperial "Leopoldinisch-Carolinische" German Academy of Naturalists at Halle awards every year, has this time been given to Prof. F. Eilhard Schulze of Graz.

The Berlin Mining Academy has purchased for the Mineralogical Museum of this Institution a so-called lightning tube or fulgurite, which was recently found near Warmbrunn. It measures nearly 2 metres in length. It is specially interesting, inasmuch as it shows a branch formation, about 30 centimetres from its end, measuring half a metre in length. The fulgurite was found after a severe thunderstorm in a sandhill and in a vertical position.

A brilliant meteor was observed at Carlsruhe on March 5 at 8.9 p.m. It was about twice as bright as Venus at her greatest brilliancy. Its direction was S.S.W. to N.N.E.; it left a trail of yellowish red colour and of several degrees in length. The phenomenon finally disappeared in the constellation of Cassiopeia, developing little cloudlets at its disappearance.

At Salez (canton of St. Gallen) some sixty bronze hatchets have been found imbedded in the ground only one metre deep. Their age is stated to be at least 2500 years.

The additions to the Zoological Society's Gardens during the past week include an Arabian Baboon (*Cynocephalus hamadryas* ♀) from Arabia, presented by Mr. F. E. Goodner; a Sharp-tailed Grouse (*Tetrao phasianellus*) from North America, presented by Mr. Henry Nash; two Sea Mice (*Aphrodite aculeata*) from British Seas, presented by Mrs. A. Browning; an Olive Weaver Bird (*Hyphantornis capensis*) from South Africa, presented by Mr. Edward Ling; two Bonnet Monkeys (*Macacus radiatus* ♂ ♀) from India, deposited; a Red-vented Parrot (*Pionus menstruus*) from South America; a Sordid Parrot (*Pionus sordidus*) from Venezuela, purchased; a Long-eared Fox (*Otocyon lalandii*) from South Africa, received on approval; a Sambur Deer (*Cervus aristotelis* ♀), an Axis Deer (*Cervus axis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Great Comet of 1882.—Herr Stechert has continued his ephemeris of this comet from the elliptical elements by Dr. Kreutz, which still agree pretty nearly with observations. We extract as follows:—

At Berlin, Midnight

	R.A. h.	m.	s.	Decl. °	′	Log distance from Earth.	Sun
April 9	5	59	32	−8	43·1	0·5973	0·5787
11	6	0	30	8	29·5		
13	–	1	30	8	16·5	0·6084	0·5843
15	–	2	33	8	3·9		
17	–	3	37	7	51·9	0·6191	0·5898
19	–	4	43	7	40·4		
21	–	5	50	7	29·3	0·6294	0·5952
23	–	6	59	7	18·8		
25	–	8	10	7	8·7	0·6393	0·6005
27	–	9	22	6	59·1		
29	–	10	35	6	50·0	0·6487	0·6057
May 1	6	11	50	−6	41·3		

Assuming the intensity of light = 1, on February 8, when Prof. Schmidt last saw the comet with the naked eye at Athens, the intensity on April 9 will be 0·234, and on April 29, 0·163.

From September 8, the date of the first accurate observation at the Royal Observatory, Cape of Good Hope, to the middle of last month, the comet had described a heliocentric or orbital arc of 339½°; no other comet since the celebrated one of 1680 has passed over so large an arc of its orbit while under observation. Between Kirch's first observation on the morning of November 14, 1680, and the last observation by Sir Isaac Newton on March 19, 1681, that body traversed a heliocentric arc of 345°.

Variable Stars.—Mr. G. Knott has observed three more minima of Ceraski's variable U Cephei. The resulting times of minima are—

	h. m.			
1883, March 12,	11 49	G.M.T.	...	Magnitude 9·4
,, 22,	11 10	,,	...	,, 9·45
April 1,	10 29	,,	...	,, 9·45

Mr. Knott remarks that the star is not a very easy one to observe, and it is not therefore an easy matter to disentangle errors of observation from real irregularities in the light curve.

On March 31 and April 1 he found the variable star R Coronæ Borealis very visible to the naked eye, and nearly equal to π Coronæ. "It has presumably brightened up further since Schmidt's observations towards the end of last year" (*Ast. Nach.* No 2491). π Coronæ is a sixth magnitude according to Argelander and Heis. The variability of this star was established by Pigott in 1795, but its fluctuations are exceedingly irregular.

Schönfeld in his last catalogue gives, as the limits of variation, 5·8m. and 13m. The actual position is in R.A. 15h. 43m. 45s., Decl. + 28° 31'·0. Schmidt found that a star which precedes R Coronæ by 2 seconds, and 7½ minutes N. varies from 11m. to 13·12m. in a period of perhaps 1½–2 months (see *Ast. Nach.* No. 1915).

Bradley 396 has been so discordantly rated in our catalogues that variability appears highly probable, and the period may not be a long one. The estimates are from 4·5m. to 7m. It is Groombridge 580, Fedorenko 473 and B.A.C. 906. The position for 1883·0 is in R.A. 2h. 53m. 40s., Decl. +81° 1'·0.

Prof. Pickering reports that a careful study of the fluctuations of Sawyer's variable by Mr. Chandler shows that it belongs to the Algol class, and has a period of little over 20 hours. A long series of observations of the light curve and successive minima gives 20h. 7m. 41·6s. ± 1·3s.

THE LATE TRANSIT OF VENUS.—Prof. Pickering has published the results of contact observations in the transit of Venus made at the observatory of Harvard College; the times are as follow:—

	h.	m.	s.		
First external contact ...	2	4	32	G.M.T.	by 3 observers.
,, internal ,, ...	2	24	43	,,	by 4 ,,
Last internal ,, ...	7	47	40	,,	by 6 ,,
,, external ,, ...	8	7	52	,,	by 6 ,,

These differ from the times given by the equations of reduction inserted in this column by + 58s., + 11s., + 22s., and – 25s. respectively, a very close accordance, considering that observations of the first external contact are less certain than the others.

GEOGRAPHICAL NOTES

AT a recent meeting of the Geographical Society of Copenhagen, Capt. Irminger in the chair, Dr. Oscar Dickson was present to give an account of the proposed Swedish expedition to Greenland. The chairman referred to Dr. Dickson as the Mæcenas who enabled Nordenskjold to carry out his ideas, while both had an ardent supporter in King Oscar. Of the Arctic expeditions, which wholly or partly owed their origin to Oscar Dickson, he mentioned the following:—The expedition of 1868 to Spitzbergen was almost entirely paid for by him; the expedition of 1870 to Greenland was entirely paid for by him; the expedition of 1872–73, which wintered at Spitzbergen, was partly paid by him, while the great deficiency subsequently arising was covered by him; the expedition of 1875 to the Yenissei was entirely paid by him; the expedition of 1876 to the same river, by sea and by land, was chiefly paid by him; the *Vega* expedition of 1879-80 was paid to the extent of one-third by him, and if the vessel had not succeeded in rounding Asia he would have borne the entire cost of this expedition; and eventually the cost of the Swedish expedition of 1883 would be borne by him. It should also not be forgotten that, at the time when the despatch of the *Dijmphna* expedition was nearly frustrated for the want of 20,000 kr. (1150*l.*), Oscar Dickson came forward to supply the deficiency, and although it was most generously contributed by Mr. Gamél, every lover of geographical discovery ought to appreciate his noble action. Dr. Oscar Dickson next addressed the meeting. He began by stating that the King of Denmark had sanctioned the new expedition. Nordenskjold had not desired that the programme of the expedition should be made public too soon, as he was much occupied with preparations for his journey and his duties as a senator, and if his plans should be questioned by *savants*, he would have no time for discussing them. He next referred to the oldest accounts of Greenland, its colonisation from Iceland, and "Esquimauxising" from America. After this, Greenland was for a time forgotten, until the west coast was rediscovered. The speaker then mentioned the achievements of Hans Egede, and the founding of a commerce. The west coast was one of the best known Arctic countries, both geographically and ethnologically; but not so the east coast. In spite of several expeditions and researches, only the southern portion was known. The interior was a *terra incognita*. These tracts were, however, too important to remain unknown. He then referred to the wanderings of Nordenskjold and Lieut. Jensen on the inland ice. From these expeditions it was impossible to infer that the interior of Greenland was entirely covered with ice, while in the constant advance of the glaciers and their melting off he (the speaker) found a corroboration of this theory. By the geographical appearance of Greenland, and more especially by the circumstance that the country gradually rose in the interior, it was more than probable that the interior was not entirely covered with ice. Even in the temperature and moistness of the air there seemed a proof that the country would answer to its name. In any case the exploration of the interior of this country was most important, and it was for this purpose that the expedition would make its researches. To these belonged the ascertaining of the extent of the drift ice between Cape Farewell and Iceland, the study of the inland ice, the fossil remains, and the cosmic dust in the island. Eventually it was hoped that, while Nordenskjold made his expedition across the ice, another party of the members would pay a visit to the west coast, where there were some very peculiar blocks of ironstone. The expedition would possess a complete staff of scientific specialists. The expedition had also one more object in view, viz. to settle the question as to where the Österbygd had been. Every one who read without prejudice the oldest accounts could but come to the conclusion that its remains must be found on the east coast. After excursions on the inland ice, it was the intention to attempt to penetrate northwards along the east coast. In May next the expedition would start in a well-equipped steamer, and, if the state of the ice would permit, first land on the east coast; but as this was not expected to be the case the party would land on the west coast, and when the researches here were at an end they would proceed along the east coast in a channel between the land and the drift ice. In September next the expedition would return.

THE changes of level of the Caspian are still a puzzling problem for Caucasian geographers. It is known that the late M. Khanikoff was of opinion that the level of this sea has been rapidly falling during our century. After having been, in 1780, 13 feet above the level of 1852, and 10 feet in 1820, he said, it was only 3·3 feet higher in 1830, and has almost regularly decreased since. Sokoloff maintained that it had risen and fallen at irregular intervals since 1744, but was 10 feet lower in 1830 than in 1780. Lenz admitted that it had fallen about 10 feet during the years 1816 to 1830. In any case, for the benefit of subsequent measurements he made permanent marks at Baku showing the level of the sea in 1830, and since that time measurements of level were carried on at Baku. But their results were unsatisfactory—as it appears from M. Filipoff's paper in the last number of the Caucasian *Izvestia*—and the only sure result is that on May 30, 1853, the level of the Caspian was 2 feet 1·3 inches lower than in March, 1830. In September, 1854, at high water it already had risen 1 foot 6 inches *above* the mark of May 30, 1853. On June 4, 1882, that is, at high water, it was also higher than in 1830 by 10·5 inches, so that it may be admitted, according to M. Filipoff, that since 1830 the level of the Caspian, although subject to fluctuations (such as a rapid rise after 1847), has not sensibly fallen during the last fifty years.

ACCORDING to the recent explorations of M. Yadrintseff, the situation of the aborigines throughout Northern and Middle Siberia is very precarious. The Bakaharians and Tartars, who were formerly a privileged class of merchants, and number at present 43,670 souls in Middle Siberia, are decreasing, and belong to the poorest population of the country. The Voguls in the Government of Tobolsk number 6070, and their increase is insignificant. As to the 23,070 Ostyaks and Samoyedes, they are in the worst imaginable position; the rate of increase is very low, while in other parts they obviously decreasing. They are accustomed now to eat bread, but have no means to provide it in necessary quantities owing to its high price. As to the southern Tartars, who have maintained their pasture lands, they are in a better position; those of Barnaoul and Biysk, who are agriculturists, and those of Kuznetsk, living on trade, are on the increase; and M. Yadrintseff quotes an instance of ten families who have maintained their land and occupy now seven villages, making a total of 1270 souls. The dying out of these aborigines is the more regrettable, as M. Yadrintseff proved by numerous instances that they display a high degree of intelligence, and might adapt themselves to new conditions.

IN the April number of *Petermann's Mittheilungen* there is a full account, by Dr. Rink, of recent Danish researches in Greenland,—on the geography of the interior, the ice-formation and glaciers, geology and mineralogy, botany and archæology; accompanying the paper is a map of the west coast between Godhavn and Pröven, coloured geologically. Baron von Richthofen discusses the value of the copy of "Marco Polo" recently discovered in the royal library of Stockholm.

It is reported that Dr. Emil Holub is at last about to start again for the dark continent. As before, so will Dr. Holub now go to Africa without one penny State assistance; and the only support he could obtain is that a special train will carry his cases to the Austrian frontier, and, if the German Government permit, to Hamburg, where they will be embarked for South Africa. The money for his expedition he acquired himself by lecturing in Vienna, Berlin, London, &c. He will leave Austria after he hears that his cases have arrived in Africa, in about two months, and he contemplates remaining on the African continent about four years. The 150 cases and about 100 other packages which he takes contain all that is necessary for a scientific expedition, including scientific instruments which the Austrian War Ministry lends him. He has also a transportable iron cart, which can be taken to pieces, and an iron boat on Stanley's celebrated models, both gifts of Austrian manufacturers or private persons. The remainder of the cases are filled with utensils, arms, stuffs, cotton goods, &c., for the natives, and all other necessaries.

The Museum for Commercial Geography was opened at Berlin in the Architekten House on April 1.

The Imperial Geographical Society of St. Petersburg has awarded its highest distinction, viz. the Constantine medal, to Dr. Hermann Abich of Vienna, for his work, "Geological Researches in the Caucasus."

FACTS AND CONSIDERATIONS RELATING TO THE PRACTICE OF SCIENTIFIC EXPERIMENTS ON LIVING ANIMALS, COMMONLY CALLED VIVISECTION[1]

[*Issued by the Association for the Advancement of Medicine by Research*]

§ 1. MEDICINE, as the art of preventing and curing disease, depends first, upon Anatomy and Physiology, or knowledge of the structure and working of the human body in health; secondly, upon Pathology, or knowledge of the origin, course, and results of disease; and thirdly, upon knowledge of the effects of various mechanical, physical, or chemical means which prevent or modify diseased processes, and are thus available for preventive or curative Treatment

As in every other practical art, the application of scientific (that is to say, exact and general) knowledge to particular cases must be checked and controlled by practical experience. But the history of medicine abundantly proves that experience is productive only in so far as it is guided by the habit of scientific inquiry and quickened by physiological knowledge. The foundation of efficient medicine was laid by the discoveries of the sixteenth century in anatomy, and of the seventeenth century in physiology, and its rapid progress in modern times has been chiefly the result of discoveries in physics, in chemistry, and in general biology.[2]

Medicine then, including Hygiene, or preventive medicine, and Therapeutics, or curative medicine, whether it acts by operative and mechanical measures,[1] by the administration of drugs, or by other means, does not depend upon arbitrary dogmas, or upon the theories of one or another school; it depends upon accurate knowledge of the structure and functions of the body in health and disease, and of the effects of various agents upon it, applied in each case by the aid of bedside experience—*καθ' ἕκαστον γὰρ ἰατρεύει.*

The relation of medicine to physiology and pathology is the same as that of navigation to astrometry and meteorology, or of engineering to applied mathematics, or of dyeing and other manufactures to chemistry. A seaman may safely direct a vessel who is ignorant of the construction of a quadrant; a bridge may be built without knowledge of theoretical mechanics, and a watch may be "cured" or a musical instrument "tuned" by a workman who is unacquainted with mathematics or acoustics. In the same way many men are useful practitioners of medicine who are imperfectly acquainted with the scientific basis of their practice. But it is only the most ignorant of sailors who sneer at natural science, and the most presumptuous of watchmakers who rail at mathematics.

§ 2. The knowledge of the functions of the body in health, or Physiology; the knowledge of the origin and course of diseases, or Pathology; and the knowledge of the action of remedies, or Pharmacology, like other branches of natural science, depend entirely upon observation and experiment. Mere observation at its best is but careful noting of such experiments as natural laws or accident may present; experiment, or observation of events under intentionally varied conditions, is absolutely necessary in addition.[2] Indeed, it would be as unreasonable to expect the "Institute of Medicine" (as physiology and pathology are rightly called) to advance without laboratories and experiments on animals, as to hope for progress in chemistry or physics by allowing only observation upon metals and gases and forbidding the performance of experiments.

It is true that there are special difficulties in the study of the natural laws of living bodies. The conditions are far more complicated than those of the inorganic world, and observations and experiments must be proportionately numerous, well-devised, and cautiously interpreted. Fallacies of observation and deduction are difficult to avoid, and often results are seemingly contradictory until their true meaning is perceived by help of fresh experiments and more careful reasoning. But the great and assured results which have been already obtained prove that these difficulties are far from insurmountable. All our present knowledge has been achieved in spite of them, and thereby the path to future discoveries has been cleared. No reasonable person would disparage experimental inquiry into the functions of plants and the cultivation of crops, because the laws of vegetable life are more complicated and obscure than those of mineralogy: or would call the experiments of the botanist useless because they are difficult.

That experiments on living creatures, like all other experiments made by fallible persons, have sometimes misled, is an obvious truth. Many errors attended the first application of the stethoscope, of the microscope, and of chemical analysis to medicine, so that impatience and ignorance pronounced that each of these valuable methods of investigation was useless.

§ 3. The future progress of medicine, in the widest sense of the word, of the art which prevents disease, promotes health, relieves sickness and prolongs life, depends upon the same cause which has led to its present position—upon more complete acquaintance with the laws of health and disease. These laws have been, and can only be, successfully investigated by observations and experiments.

This conclusion is not only the inevitable result of reasoning, but is also enforced by the unwavering testimony of those best qualified to judge,—not only of scientific workers themselves, but of the medical profession in all civilised countries. There is not the smallest danger that the ninety-nine hundredths of the medical profession who are engaged in the daily effort to prevent or relieve disease will undervalue practical medicine in comparison with the more abstruse branches of experimental physiology and pathology; the danger is the other way. With few exceptions physicians and surgeons are not themselves experimenters in physiology or pathology. Their business is to prevent disease and to relieve their patient's sufferings: but they know the benefits which their art has derived from the work of the laboratory, and understand the nature and value of experiments. They are thus at once the most disinterested and the most competent witnesses, and their constant and unanimous testimony ought to be conclusive.[1]

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[1] The term "Vivisection" is open to objection. As a question-begging epithet, it produces an unfounded prejudice against experiments, of which the majority are painless, and of which the object is to relieve the sufferings of both man and brutes Moreover, the term is at once too narrow and too wide: too narrow, since it excludes painful experiments which do not involve cutting, such as exposure to disease; and too wide, since it includes painful procedures upon animals for other than scientific or humane objects, for food, as in preparation for the table, for convenience, as in horse and cattle breeding, or for amusement, as in certain sports. The same operation which, if performed for the acquirement of knowledge, is called a vivisection, is not called a vivisection when performed for a less worthy object.

[2] Some otherwise well-informed persons have expressed doubt as to the reality of the great progress of medicine during the present century. This doubt arises partly from an arbitrary separation of what is called internal medicine from surgery (la médicine opératoire) and from preventive medicine. The world fully appreciates such triumphs of medicine as the cure of Aneurism and prevention of Small-pox, the discovery of Anæsthetics and the success of Ovariotomy, the results of Antiseptic surgery, the vastly decreased mortality after operations, and the protection of cattle from pestilence by inoculation. But in the treatment of fevers, inflammations, and other internal diseases, conventionally called medical, progress is less striking, because, being more obscure, these maladies have not yet been brought under the complete influence of scientific investigation

In proof, however, that the scientific spirit of modern medicine has not failed to advance the treatment of even the more obscure diseases, and that practical advance in medical treatment has not been limited to operative surgery, may be adduced as instances: the greatly lessened mortality in Fevers, owing to physiological observations and scientific treatment, the improved diagnosis and more successful results in cases of paralysis and other diseases of the Nervous System; the far shorter and less painful course of acute Rheumatism; the advance in treatment of Diabetes, Consumption, Dropsies, and affections of the Heart, and the successful cure of numerous forms of disease now proved to be due to animal or vegetable parasites.

"Looking back over the improvements of practical medicine and surgery during my own observation of them in nearly fifty years (writes Sir James Paget) I see great numbers of means effectual for the saving of lives and for the detection, prevention, or quicker remedy of diseases and physical disabilities, all obtained by means of knowledge, to the acquirement or safe use of which experiments on animals have contributed. There is scarcely an operation in surgery of which the mortality is now more than half as great as it was forty years ago, scarcely a serious injury of which the consequences are more than half as serious; several diseases are remediable which used to be nearly always fatal; potent medicines have been introduced and safely used; altogether, such a quantity of life and working power has been saved by lately-acquired knowledge as is truly past counting."

[1] "Forasmuch as the Science of Physick doth comprehend, include, and contain the knowledge of Chirurgery as a special member and part of the same."—Statute 32 Hen. VIII. c. 40

[2] "L'observateur écoute la nature, l'expérimentateur l'interroge."—Cuvier.

The International Medical Congress of 1881, where upwards of 3,000 physicians and surgeons assembled in London, among whom were the ablest and most respected leaders of the profession in the three kingdoms, in America, and in foreign countries, passed, without a dissentient voice, the following resolution:—

"That this congress records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable to its future progress. That, accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion, alike in the interest of man and of animals, that it is not desirable to restrict competent persons in the performance of such experiments."

§ 4. A moral question, however, arises, from the fact referred to in the resolution just quoted, that some of the necessary experiments of physiology and pathology involve the infliction of pain or of death upon certain of the lower animals.

The better informed opponents of experimental medicine do not dispute its scientific and practical value, but assert that no probable benefit to man or animals justifies the infliction of pain.

No one would succeed in closing the laboratories of the chemist, or the observatories of the astronomer, however strong his disbelief in the experimental method of inquiry might be, however cordially he disliked or dreaded the advance of science, or however obstinately he persisted that the useful arts do not depend on scientific data.[2] It is obvious, however, that the fact of pain or death being inflicted in the course of experiments cannot alter their scientific importance and necessity; it only imposes on us the duty of making a comparison between the injury to a sentient creature and the probable benefit to mankind, or to others of its own species. This comparison we will attempt to make.

Happily, the amount of pain inflicted in the course of scientific experiments need only be small, and the destruction of life insignificant. That, from carelessness or want of forethought, experiments have been performed which were "cruel," because the pain produced was excessive and unnecessary, may be admitted. In many countries consideration for the brute creation is still little developed, and the vice of cruelty lightly regarded; even in England, until comparatively lately, the torture of harmless animals was thought an innocent pastime. Men of science have not always risen above the average humanity and moral enlightenment of their age and country. But speaking of this country, and of modern times, it may safely be said that no charge of wanton, needless, or excessive sacrifice of animals can be, or indeed has been, seriously alleged against the small number of experimental physiologists and pathologists at work in the three kingdoms.[1] Science has herself provided the means by which pain is reduced to a minimum. The beneficent discovery of anæsthetics is one cause of the great difference between the sufferings inflicted by Harvey, Boyle, Hales, Haller, Hunter, Magendie and Bell, and the generally painless experiments of a modern laboratory. These may be classified as follows, with reference to the suffering inflicted:—

(1) Many physiological experiments are entirely unaccompanied by pain, and can therefore be performed, according to convenience, either upon animals or upon man himself. Such are many experiments upon vision, taste, smell and touch; experiments on the value of different kinds of food, experiments on the effect of exercise, temperature, and other conditions on the excretions; many experiments on bodily heat, on the pulse, and on respiration.

(2) In still more numerous cases, observations and experiments can be made on the tissues and organs after the death of an animal: *e.g.*, the relative tenacity of the different textures, the mechanical effects of violence upon the bones, the action of the heart (which in cold-blooded creatures continues long after their death) and the whole of a long and important series of experiments on the functions of muscles and nerves, which cause no pain, since they are performed on the tissues of a dead organism.

(3) Next, but far less in number, comes a third class of experiments which are performed on animals rendered insensible by various anæsthetic agents. These can be, and were, by the practice of physiologists long before legislative sanction was added, carried out without any pain or even discomfort to the animal, which being killed before awakening, is deprived of life in probably the most painless manner possible.

(4) There are, however, certain observations, for which it is necessary to allow an animal to recover from insensibility, and to live for a longer or shorter time. In such cases the severest pain, that of the operation, is abolished, and the subsequent suffering is sometimes quite insignificant, usually that of a healing wound, and occasionally that of inflammation, colic, or fever. In many of these experiments the initial pain is so trifling that it would be absurd to give an anæsthetic; such are acupuncture and inoculation. It would be unreasonable to give a rabbit chloroform for such observations as bleeding, vaccination, or pricking with the needle of a subcutaneous syringe, for which no human being would take it.

(5) There remain a small number of experiments in which anæsthetics would be impracticable. These are chiefly the experimental production of various diseases, such as tubercle, glanders, cattle-plague, where the pain is that of the subsequent

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[1] It would be invidious to dwell upon the very few exceptions to this almost universal testimony. One only deserves special mention. Sir William Fergusson was one of the most skilful and successful operators, but he had no authoritative claim to give an opinion upon the sources or the methods of surgical science, and even he in his evidence before the Royal Commission admitted the use of experiments on animals.

The testimony of the late Professor Claude Bernard has been often adduced against that of all other physiologists because he once wrote, "Nous venons les mains vides, mais la bouche pleine de promesses légitimes." This phrase occurs in an elaborate exposition of the necessity of experiments on living animals not only for knowledge but for use. Bernard well understood the bearing of experiments upon medicine, but he foresaw future developments of scientific treatment, in comparison with which his own eminent services would appear insignificant. The following quotation shows that his evidence on the whole question did not differ from that of other competent witnesses:

"On voit que la physiologie, ou médecine scientifique, comprend à la fois ce qu'on a artificiellement séparé sous les noms de physiologie normale, de physiologie pathologique, et de thérapeutique. Au point de vue pratique, c'est certainement la thérapeutique qui intéresse au plus haut degré le médecin; or, c'est précisément la thérapeutique que doit le plus de progrès à la physiologie expérimentale."—*Leçons de Physiologie Opératoire*, p. 20.

[2] On the other hand, it is almost as clear that no serious obstacle would be put in the way of even painful experiments in the cause of science, if all their opponents were convinced of their utility, and were acquainted with the methods of science in general, or the facts of medical science in particular. This seems to follow from the very moderate opposition to, or tacit acquiescence in, the infliction of pain for desirable objects which obviously cannot be otherwise attained, such as more delicate food, more docile horses, increased wealth and comfort, or the pleasurable excitement of chasing and killing animals.

[1] The following extract is taken from the Report of the Royal Commission, which was drawn up after a prolonged and patient examination of witnesses and documents, and was signed by all the Commission—Lord Cardwell (Chairman), Lord Winmarleigh, the Rt. Hon W. E. Forster, the late Sir John Karslake, Professor Huxley, Mr. Erichsen, and Mr. R. H. Hutton:—

"That the abuse of the practice by inhuman or unskilful persons, in short, the infliction upon animals of any unnecessary pain, is justly abhorrent to the moral sense of your Majesty's subjects generally, not least so of the most distinguished physiologists and the most eminent surgeons and physicians."

The imputation of cruelty which has always been indignantly repudiated, has not been substantiated by a single authentic instance. In their evidence, given before the Royal Commission, the Royal Society for the Prevention of Cruelty to Animals state, through their Secretary, that they do not know a single case of wanton cruelty.

On the occasion of the present Act (39 and 40 Vict. cap. 77) being passed, all teachers of physiology, in a memorial addressed to the House of Commons, said:

"We repeat the statement which most of us have made before the Commission, that within our personal knowledge, the abuses in connection with scientific investigation, against which in this Bill it is proposed to legislate, do not exist, and never have existed in this country." Signed by the late Prof. Sharpey (University College, London); Dr. William Carpenter, C.B. (formerly Lecturer on Physiology at the London Hospital), Professor G. Humphry (Cambridge); Professor Rutherford (Edinburgh); Dr. Pavy (Guy's Hospital), Dr M. Foster (Trinity College, Cambridge); Dr. Burdon Sanderson (University College, London); Dr. Robert McDonnell (Dublin); Prof. Redfern (Belfast), Prof Cleland (Galway); Prof Charles (Cork); Prof. McKendrick (now of Glasgow); Dr. Pye-Smith (Guy's Hospital); Prof. Yeo (King's College, London); Mr. Charles Yule (Magdalen College, Oxford); Prof. Gamgee (Owens College, Manchester).

disease, and more justly described as discomfort than as torture; and the trial of certain modes of treatment, as inoculation, and of various drugs, where the suffering produced is less than the familiar effects of corresponding remedies in human beings. Probably the most painful scientific experiments ever performed have not been vivisections at all. Such are those of ascertaining the effect of starvation, carried out abroad many years ago; observations of great value and importance, but happily not needing repetition.

Vivisections in the popular sense of the word, experiments comparable to surgical operations, involving cutting and irritation of sensitive parts, can, with few exceptions, be performed without the slightest pain. Hence the results of acutely painful experiments, comparable with the pain endured by rabbits and weasels caught in ordinary traps, by young animals being gelded, by wounded birds, or by rats poisoned with strychnine or phosphorus, are not to be found in our physiological laboratories.

That the utmost possible limitation of the infliction of pain has always been the object and practice of scientific workers in England,[1] is sufficiently proved by a Report which was drawn up by a Committee of the Physiological Section of the British Association for the Advancement of Science, in 1871, several years before the appointment of the Royal Commission.

While the suffering caused to animals by scientific experiments has been enormously exaggerated, both absolutely and relatively, no one denies that both pain and death are and must be inflicted thereby. Otherwise there would be no more reason for licensing and inspecting the physiologist's laboratory than that of the chemist. The whole question is one of justification for causing the pain or death of brutes. Few who compare the extent of suffering and of slaughter thus caused with that generally recognised as right in other cases by enlightened Christian morality, or who compare the objects for which animals commonly suffer pain and death (for food, for dress, for profit, for convenience, or for amusement) with those of the scientific observer (for advance of knowledge and for relief of human suffering) will hesitate to conclude that so long as the principles and practice of scientific men in this country continue what they now are, their investigations should rather be fostered than impeded.

But any possible danger of abuse is prevented by the Act passed in 1876, by which not only are all physiological laboratories placed under the inspection of the Home Office, and exist only by its license, but, in addition, no experiment involving pain can be performed without a special, elaborate, and carefully guarded certificate. Indeed, so stringently has the law been administered that more than one investigation of great practical value has been prevented, others have been injuriously hampered or delayed, and a serious check has been given to medical science in England. In two instances eminent members of the profession found it necessary to go abroad in order to carry out investigations of great importance. The object of one was to decide a question in relation to treatment of wounds; that of the other was to determine the action of certain new drugs.

This was certainly not the intention of the Royal Commission in recommending, or of Parliament in passing, an Act for the purpose of preventing possible abuses without hindering scientific and useful work. What is now needed is such an expression of opinion in Parliament as will permit the Act to be worked in the spirit in which it was framed and loyally accepted, and according to its strict provisions. It may be remarked that attempts have been made, by the same methods of agitation, to check physiological research by legislation in Germany, Denmark, Sweden, and the United States: but in each case the humane and enlightened judgment of the country has refused to impede researches of which the usefulness is beyond dispute.

§ 5. It has been imagined that students of medicine perform operations upon living animals in order to gain manual dexterity; such a practice would be as useless as it would be reprehensible, and has never, we believe, been thought of. For our veterinary surgeons it would be quite unnecessary, and they have always reprobated the practice.

It has also been supposed that students might, for amusement, perform physiological experiments upon living animals. This would be practically impossible, since not only are knowledge and skill necessary, but a properly equipped laboratory and suitable appliances.[1] If, however, any ill-disposed person without scientific object or training should be guilty of cruelty most alien from the practice and the training of the profession, there is no doubt that every member of it, teacher or student, would help to detect and punish such conduct.[2] The case has never arisen; if it did, it could be efficiently dealt with under the law known as "Martin's Act."

§ 6. The real objects of scientific experiment on living animals are briefly as follows:—

i. *To extend, correct, and define our knowledge of the functions of the living body.*

Even apart from ulterior advantage to medicine, physiology must be held to be a branch of science of at least equal importance with chemistry or geology; and to be successfully cultivated, it must be cultivated for its own sake, without perpetual or premature inquiry as to the immediate and material results which increased knowledge of the laws of Nature will bring. In physiology, as in other natural sciences, the investigator must have primarily in view the discovery of truth; for, in the words adopted by the Royal Commissioners, "if in the pursuit of science he seeks after immediate practical utility, he may generally rest assured that he will seek in vain." There must be, to quote the words of an older authority, "light-bearing," as well as "fruit-bearing experiments."

As examples of this first kind of experiment, and of their success in extending useful knowledge, we may refer to the following:—

(1) The great discovery of the circulation of the blood by Harvey, the firstfruits of the experimental method.[3] Upon this as the foundation depends all the subsequent progress in the surgical treatment of hæmorrhage and of aneurisms, and the recognition and treatment of diseases of the heart, the arteries, and the veins.

(2) The discovery of the effects of electricity on animals by Galvani and Volta, from which have resulted not only the development of one great branch of electrical science, but also important means of diagnosis and treatment in cases of paralysis.

(3) Artificial respiration, invented and improved in the case of animals with purely scientific objects by Vesalius, Hooke, Lower, and others, and long afterwards applied with complete success to resuscitation from drowning.

(4) The experiments of the Rev. Dr. Hales on pressure of the blood in the arteries.

(5) Those of Boyle, Hooke, Mayow, and other natural philosophers on respiration.

(6) Transfusion of blood from one animal to another, accomplished by Sir Christopher Wren and others of the early Fellows of the Royal Society in the seventeenth century, but only recently, owing to fresh physiological knowledge, applied with success to the saving of human life.

(7) Experiments by a Committee of Physicians at Dublin, in 1835; showing the way in which the sounds that attend the action of the heart are produced, and enabling physicians to judge of the condition of the organ by the alterations of the sounds.

(8) The discoveries of reflex action and of the separate endowments of motor and sensory nerves, on which much of our present knowledge of the functions and disorders of the nervous system is founded.

(9) The discovery of vasomotor nerves.

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[1] The following quotation, from a Manual of Physiological Experiment by a well-known German physiologist, will serve to show that humane consideration for animals is not confined to this country:—"An experiment involving vivisection should never be performed, especially for purposes of demonstration, without previous consideration whether its object may not be otherwise attained," and, as a second rule, "Insensibility by chloroform or other drugs should be produced whenever the nature of the experiment does not render this absolutely impossible."—Cyon, *Physiologische Methodik*, p 9

[1] It is obvious that this sound general principle admits of exceptions when the skilled person with suitable appliances must, from the nature of the case, carry out his researches on board ship, as for instance for investigation into the functions of jelly-fish, or the electric torpedo, or in the open fields, as in inquiries into means of protection from epidemic diseases of cattle.

[2] For the real sentiments of medical students, see Dr. Pavy's evidence before the Royal Commission, *Blue Book*, p. 114.

[3] Some persons have ventured to deny that Harvey's discoveries were due to vivisection, on the faith of a reported statement of his to the Hon. Robert Boyle (another eminent vivisector), and in contradiction to Harvey's express words Others have denied that the circulation was proved by vivisection, because Harvey having proved all but one point by a series of experiments on living animals, Malpighi completed the demonstration by another experiment on another living animal. The full account of the matter is contained in Harvey's own treatise, "De Motu Cordis et Sanguinis." It is briefly referred to in the article *Harvey* of the "Encyclopædia Britannica," and in the evidence of Professor Turner, of Edinburgh, before the Royal Commission (Blue Book, pp. 157, 158), where also are given the account of the discovery by vivisection of the great system of lymphatic vessels, by Aselli and Pecquet, and of the discovery of motor and sensory nerves by the same means by Bell and Magendie.

ii. *To obtain direct and exact knowledge of the processes of disease.*

The following examples may be cited :—

(1) Experiments relating to the nutrition of the body and the maintenance of its constant temperature constitute the basis of the existing knowledge of fever.

(2) Experiments relating to the mechanism of the circulation, and to the influence of the nervous system thereon, have served to explain the nature and mode of origin of the various forms of dropsy.

(3) Experiments as to the effect of plugging arteries (Embolism) have afforded explanations of diseased processes previously not understood, and in particular of many obscure cases of sudden death.

(4) Experimental investigations of the functions of the liver and other secreting glands have materially advanced our knowledge of diabetes and of the affections known as Bright's disease.

(5) Knowledge gained from experiments relating to the mode of action of the muscles, and of the nervous system which regulates them, constitutes the basis of the pathology and diagnosis of convulsive and paralytic diseases.

(6) Experiments on animal grafting and as to the nature of the processes by which wounds are healed and injured parts restored. Among the best known are those which relate to the mode of repair of fractured or otherwise injured bones, particularly the researches of Duhamel (1740), Sir Astley Cooper (1820), and Syme (1831). In recent times such inquiries have been pursued much more completely by Ollier and others, and with practical results of ever-increasing value.

(7) The dangerous form of blood poisoning after operations has been investigated by strictly physiological experiments, with the result of almost complete protection from it.

(8) Researches into the origin and nature of inflammation, by Redfern, Cohnheim, Von Recklinghausen, and others, have been of necessity conducted by means of experiments on animals, and have proved of great practical value.

(9) Our recently extended knowledge of the locality of diseases of the brain, and of their accurate diagnosis and treatment, has been due, partly to clinical observations, partly to pathological investigations, but also, and not least, to direct experiments upon the lower animals.[1]

iii. *To test various remedial measures directly.*

The utility of the greater number of the older remedies and methods was first learnt empirically: but many of them were not applied to the best purpose until they have been investigated by observations on the lower animals. As regards the remedies and appliances of modern times, they have, in almost every instance, been investigated first and brought into use afterwards. For example :—

(1) Subcutaneous injection was used in the laboratory for years before it was applied in practice.

(2) The useful property of the well-known anodyne chloral hydrate was first investigated in the laboratory, and then introduced into practice.

(3) Pepsin and pancreatin were known for years as physiological agents before they were applied in practice.

(4) The action and mode of administration of such important new drugs as nitrate of amyl, physostigma, and the anæsthetic, methylene, were discovered entirely by physiological experiments.

(5) The better appreciation and more useful application of some of the most valuable remedies were gained by experiments, such as those by Traube on digitalis, by Magendie on strychnia, and by Moreau and others on saline purgatives.

(6) The application of various practically useful methods of checking hæmorrhage was tested upon animals before being tried on human beings, with the result of saving innumerable lives.[2]

(7) Similar preliminary trials of subcutaneous and other operations, especially those of tenotomy, have helped in the relief of numerous deformities; while the trial of such formidable operations as excision of the kidney and tentative improvements in ovariotomy have led to some of the most brilliant results of modern surgery.[3]

In cases where new drugs are to be introduced, or new operative methods tried, the first experiments must be made either upon living animals or upon living men. Where circumstances excluded the former alternative, members of our profession have not hesitated to make themselves the subject of often hazardous experiments: but happily, in most instances, the sacrifice of a few guinea-pigs or frogs will suffice to help in saving human life.

iv. *To ascertain the means of checking contagion, and preventing epidemic disease both in man and in brutes.*[1]

An experiment of this kind, inoculating the udder of a cow so as to produce a vaccine pustule, was one of the links in the great discovery of Jenner. Among more recent examples may be mentioned :—

(1) The experimental investigations of the last fifteen years, as to the origin and nature of the infective diseases which spring from wounds and injuries (pyæmia and septicæmia), the results of which constitute the basis of antiseptic surgery.[2]

(2) The discovery by experiments of the infective nature of tuberculosis (1868), of its relation to chronic inflammation, and finally of the dependence of its infectiveness on a living parasitic organism (1881).

(3) Discovery of the mode of origin, and consequently of the prevention, of various parasitic entozoa (hydatids, trichina) which infect the human body, by inference from investigation of their development in the bodies of animals.

Among diseases of animals may be mentioned :—

(1) Silkworm disease, which has been brought completely under control by the experimental discoveries of Pasteur.

(2) Small-pox of sheep, against which preventive inoculation has been long used.

(3) Cattle-plague, the prevention of which is entirely founded on the knowledge of its mode of spreading gained by experiment.

(4) Pleuro-pneumonia of cattle, and foot and mouth disease, of which, although experiment has not as yet yielded a satisfactory mode of prevention, it has furnished exact knowledge as to the method of its propagation.

(5) Splenic fever of cattle, and the analagous diseases of horses, sheep, and other animals, against which experiment has recently furnished a mode of prevention, now successfully used in countries in which this disease has most fatally prevailed, particularly in France.

(6) Farcy and glanders, the early detection and prevention of which has been greatly promoted by experiments.

v. *For instruction.*

It is not necessary to insist on the well-known difference between book-learning and demonstration. Like chemistry, physiology must be taught practically if it is to be taught well, and it is necessary that all students of medicine to whom the care of the human body will be intrusted should have a practical and thorough familiarity with the most important functions of that body. For this purpose no painful experiments are necessary, and none are performed in our medical schools and colleges. Most of the demonstrations of what is called "practical physiology" are demonstrations of the microscopical structure of the tissues, or of their chemical properties and processes, or of their physical endowments, and the remainder apply to the organs of insensible or recently killed animals. Whether the occasional repetition of an experiment of great importance, and involving very little pain, would be morally justifiable may admit of question; but, as a matter of fact, it is not and cannot be done. Apart from the provisions of the Act, this question was decided long before by the resolution quoted above.

vi *For the detection of poisons.*

The fact that some of the most subtle and dangerous poisons cannot be certainly identified by ordinary testing (*i.e.* by recognition of their physical and chemical properties), is well known. In such cases the physiological test, or the effect of the poison upon the lower animals, is the only means by which the guilt of murder can be brought home to a criminal, or the innocence of a wrongfully accused person established. This, like many other scientific facts, has been disputed by ill-informed persons: but it is beyond serious question.[3]

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[1] See an article in NATURE, vol. xxv., p 73

[2] See an article in the *Nineteenth Century* for December 1881, p. 926.

[3] See a paper by Mr Spencer Wells, *Trans. Internat. Med Congr.* vol. ii p. 226.

[1] For details on this part of the subject, see the Address by Mr. Simon, C.B , F.R.S., entitled "Experiments on Life as fundamental to the Science of Preventive Medicine." ("Transactions of the International Medical Congress 1881.")

[2] For details, see a paper in the *Nineteenth Century* for March, 1882, by Mr. George Fleming, President of the Royal College of Veterinary Surgeons: "Vivisection and Diseases of Animals"

[3] See on this subject a paper by Prof. Gamgee, of Owens College, "The Utility of Physiological Tests in Medico-Legal Inquiries."

It was found necessary to insert a clause in the Act allowing a judge to order any needful experiments by a medical jurist. But this may cause, and has already caused, injurious delays, and it would be desirable for each person engaged in this department of scientific work to take out the necessary license beforehand.

§ 7. The above is only a brief enumeration of some of the more striking and illustrative cases in which the objects proposed by experiments on animals have been attained. In some of these success has been brilliant and complete, in others comparative and needing fuller development. In some the results have been the direct and exclusive consequence of the experiments, in others they have been due to these either as confirming or correcting previous conjectures, or as guiding clinical research, or as suggesting fruitful investigations by other methods.

Without exaggerating its extent and cogency, the evidence is ample to show, what no one conversant with the subject doubts, that the great strides made in the practice of medicine during the last fifty years have been chiefly due to the exact scientific experimental inquiries of this epoch. In fact, experience fully bears out what reason demonstrates and authority confirms, that medicine rests chiefly upon physiology, and that physiology cannot advance without experiments.

The prejudices excited by the account of long past or distant abuses of the right and duty of experiment will, it may be hoped, be dispelled (as in many cases they have been) by increased knowledge of the facts; while those which have been raised by reckless misstatement will subside on candid investigation. If any fear remain that evils which do not now exist may possibly arise in future, it may be dispelled by a consideration of the stringent regulations of the existing law, even if carried out with the utmost desire not to obstruct demonstrably useful scientific work.

But it is on the scientific investigator himself that the responsibility must ultimately rest of determining what is the best method of accomplishing a given scientific result, and by what means *the greatest possible result may be obtained at the least possible cost of suffering*. If restrictions are supposed to be necessary to control the conduct of careless individuals, let them be continued; but so long as scientific men exercise their responsibility in the humane spirit which has hitherto guided investigation in this country, they have a right to ask that no unnecessary obstacles should be placed in their way.

It is therefore hoped that such a decided and influential expression of opinion will be made in Parliament as will not only rebuke ill-advised attempts to totally abolish one of the most important methods of natural knowledge, and an indispensable method for the improvement of medicine; but will also strengthen the hands of the Government in administering the law, so as not to interfere with the just claims of science and with the paramount claims of human suffering.

THE BRITISH ASSOCIATION AND CANADA

THE following circular is being sent out by the British Association for the Advancement of Science:—

"22, *Albemarle Street, London, W., March* 19

"SIR,—We have been instructed by the Council of the British Association to communicate to you the accompanying letter from Sir A. T. Galt, G.C.M.G., High Commissioner for Canada. This letter was written in reply to one addressed by us to him, making certain inquiries with reference to the invitation to visit Montreal in 1884, which was accepted by the General Committee at the Southampton meeting last year. In that letter it was our endeavour to obtain information as accurate as possible concerning the probable expense of the journey to and from Montreal, including a stay of a fortnight or three weeks in Canada (in addition to the period of the meeting), and excursions to some of the more interesting localities. From the statements in Sir A. T. Galt's letter, the members will be able to form an opinion as to the probable cost of the expedition, the amount of which must obviously, to a considerable extent, depend upon the length of time which they are willing to devote to the visit.

"It is obviously most important to secure that the Montreal meeting should be attended by a strong and thoroughly representative body of members, so that the gathering may be both creditable to the Association and gratifying to our Canadian hosts. Further, many arrangements must be made prior to the meeting, and these must be settled considerably in advance of the usual dates. It will therefore greatly aid the Council and those who will have to carry out their instructions in detail, if you will be so good as to state your intention concerning the visit to Montreal by filling up the annexed form and returning it as addressed before April 14.

"We remain, sir, your obedient servants,
"C. W. SIEMENS, President
"A. W. WILLIAMSON, General Treasurer
"DOUGLAS GALTON, } General
"A. G. VERNON HARCOURT, } Secretaries
"T. G. BONNEY, Secretary"

"9, *Victoria Chambers, London, S.W., March* 3, 1883

"Dear Sir,—I have to refer you to your letter of November 28 on the subject of the visit of the British Association for the Advancement of Science to Montreal in 1884, in accordance with the decision of the general committee, at their meeting at Southampton on August 28 last, and to inform you that I have received a communication from the Chairman of the Montreal Invitation Committee (T. Sterry Hunt, M.A., LL.D., F.R.S.), containing some detailed information on the different matters you mentioned to me.

"It is my pleasant duty to state that the inhabitants of the city of Montreal received with satisfaction the intimation that the Association had decided to honour them with a visit, and much public spirit has already been manifested in the desire that everything should be done to make the occasion worthy of the illustrious body and of the country. Committees on invitation, on finance, and on conveyance have already been formed, and a guarantee fund opened very satisfactorily; while the Government of the Dominion, in view of the widespread interest which the matter has awakened, will ask Parliament during its present session to vote a considerable sum ($20,000) as a contribution to the funds that will be subscribed by the public. Montreal, I may add, is not without experience of the requirements of an important meeting of the kind, having twice been favoured with visits from the American Association, the last occasion being in 1882, when an attendance of more than 900 members and associates was registered, and the Association, with its nine sections, found ample accommodation in the buildings of McGill University.

"I propose to answer your questions in the same order as that in which they were placed in your communication, but it will not be possible for me to do so in such full detail as I should like so far in advance of the time of their application, especially in regard to the cost of conveyance and the various expeditions to be arranged. I trust, however, the following information will be sufficient for the purpose of giving to the members of your distinguished Association an idea of the probable expenses they may be called upon to defray during their stay in Canada.

"(I.) 'The cost of the journey to and from Montreal to one who makes it as a member of the Association or as the near relative of a member.'

"Dr. Sterry Hunt desires me to say that the committee will arrange fifty free passages for the conveyance of the officers of the Association whose attendance is indispensable at its annual meetings. The funds at the disposal of the committee will also enable it to negotiate with the steamship companies for the reduction of the ordinary ocean passages in favour of *bonâ fide* members of the Association. Two courses are open in which this can be done.

"(1) To arrange for a number of passages to be offered at the single rate for the double journey—say 15*l*. 10*s*.

"(2) For a general reduction, so far as the funds will permit.

"Either of these plans can be adopted, but the steamship companies, although fully disposed to entertain the matter, do not care to make any definite engagements so far in advance, which will, I am sure, be readily understood. I am to state, however, that the committee is prepared, with the aid of the Government grant, to devote 3000*l*. to these purposes alone.

"(II.) 'The cost of board and lodging per head per diem for the above during the week of the meeting at Montreal.'

"I cannot do better than quote a paragraph from Dr. Sterry Hunt's letter, in regard to this inquiry:—'In reply to Prof. Bonney's question as to the expenses of board and lodging for members of the British Association during the meeting in Montreal, the committee will give assurance that free entertainment will be provided for at least 150, and probably for all other members who may attend.'

"I may amplify this by stating for your information that the tariff of the Montreal hotels ranges from $2 50c. to $4 per day inclusive, and that private accommodation can be obtained at much lower prices than in England.

"(III.) 'A scheme of expeditions which would occupy from two to three weeks subsequent to the meeting, and the cost of each of them.'

"Dr. Sterry Hunt says:—'As to the proposed excursions, we are prepared to say that the Grand Trunk, the Canada Pacific, and the Intercolonial Railways will furnish free transportation over their lines throughout the dominion of Canada from Nova Scotia to the North-West. The Canada Pacific will also arrange an excursion to the Rocky Mountains, and the Grand Trunk one to the Great Lakes (note: this will include Niagara) and Chicago; while the South-Eastern Railway will do the same for the White Mountains and Portland and Boston. For an excursion of this kind, occupying three or four weeks, tourists should be provided with, say, 20*l.* in money for hotels, carriages, and other incidental expenses, though it is possible that a less sum than this would be needed.'

"I am inclosing a copy of a circular that has been prepared by the Montreal committee. It contains interesting information, and it will be seen that the arrangements are in the hands of representative and eminent men.

"I believe from the information that reaches me that the Association will receive the addition of a considerable number of associates in Canada, and that the visit will give an impetus to scientific research in the Dominion such as it has not experienced before. It is confidently anticipated also that the American Association will hold its meeting in 1884 at a convenient time and place, affording an opportunity for scientific intercourse that I imagine does not often occur.

"I will gladly supply any further information you may require if it is in my power to do so, and shall readily cooperate in any measures having for their object the success of the meeting of the British Association for the Advancement of Science at Montreal in 1884.

"I am, dear Sir, your obedient servant,
"A. T. GALT,
"High Commissioner for Canada, and Vice-Chairman
of the Montreal Citizens' Committee
"Prof. T. G. Bonney, M.A., F.R.S., F.G.S., &c.,
"22, Albemarle Street, W."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Savilian Professorship of Geometry in the University is vacant, and an election to the office will be held before the end of Trinity Term (July 7, 1883). A Fellowship in New College is now annexed to the Professorship. The duty of the Professor is to lecture and give instruction in pure and analytical geometry. The combined emoluments of the office from both sources will be, for the present, 700*l.* a year, but may possibly hereafter be increased to an amount not exceeding 900*l.* a year. The qualifications required in candidates for the Savilian Professorships by the existing Statutes of the University are as follows:—"Hos Professores sive lectores, prout voluit fundator, statuimus et decernimus fore perpetuis temporibus eligendos ex hominibus bonæ famæ et conversationis honestæ, ex quacunque natione orbis Christiani, et cujuscunque ordinis sive professionis, qui in mathematicis instructissimi sunt, et annos ad minimum sex et viginti nati; et, si Angli fuerint, sint ad minimum Artium Magistri." Candidates are requested to send to the Registrar of the University their applications, and any documents which they may wish to submit to the Electors, on or before Thursday, May 31.

VICTORIA UNIVERSITY.—At a meeting of the University Court on March 30, Vice-Chancellor Greenwood laid on the table the supplementary charter, dated March 20, 1883, enabling the University to confer degrees and distinctions in medicine and surgery. After some discussion it was resolved that the Council be empowered and instructed to appoint external examiners in medicine and surgery for a limited period, and to appoint certain lecturers of the University to act as University examiners; also to prepare, after a report from the General Board of Studies, a statute or statutes and regulations relating to degrees in medicine and surgery for the consideration of the Court, and also to report of the subsequent appointment of external examiners in medicine and surgery, in accordance with the recommendation of the University Council. The Council were instructed to ascertain whether the University charter would allow of the same facilities that had been given to Owens College students being extended to the students of other colleges when those colleges sought admission to the University. The Council were of opinion that such facilities should certainly be given.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, March 15.—Frank Crisp, treasurer and vice-president, in the chair.—Dr. T. S. Cobbold read a paper on *Simondsia paradoxa*, and on its probable affinity with *Sphærularia bombi*. Thirty years ago Prof. Simonds discovered a remarkable parasite within cysts in the stomach of a wild boar which died in the Zoological Gardens, London. Prof. Simonds regarded the worm as a species of *Strongylus*, but Dr. Cobbold in 1864 suggested its affinities might probably be nearer the genus *Spiroptera*, then naming it *Simondsia*. The original drawings unfortunately were lost, and only quite lately, along with the specimens, they have turned up and have enabled Dr. Cobbold to investigate them more closely. He arrives at the conclusion that *Simondsia* is a genus of endoparasitic nematodes in which the female is encysted and furnished with an external and much enlarged uterus, whose walls expand into branches terminating in cæca. The male is $\frac{1}{2}$ inch and the female $\frac{6}{10}$ inch long. Moreover, it is now found that what was at first regarded as the head turns out to be the tail, so that supposed Strongyloid character is incorrect. Taking into account what is known of *Sphærularia bombi* as interpreted by Schneider, and whose views are universally accepted, it appears that *Simondsia*, though unique, yet approaches towards *Sphærularia* in respect of the enormously developed female reproductive organ, which in both lies outside the body proper. Until Sir J. Lubbock's memoir on *Sphærularia* appeared, the so-called male had never been indicated; but, judged by Schneider's interpretation of that genus, the male is still unknown. Dr. Cobbold points out that the so-called rosette in *Simondsia* is morphologically a prolapsed uterus furnished with two egg-containing branches; he regards the external branched processes as homologous with the sphærules of *Sphærularia*, whilst the ultimate cæcal capsules have nothing comparable to them in nature.—A paper was read on the moths of the family Urapteridæ in the British Museum, by Arthur G. Butler. The author, basing distinctions on wing neuration and other characters, redistributes the family, and indicates the following new genera:—*Tristrophis*, *Gonorthus*, *Sirinpteris*, *Nepheloleuca*, *Thinopteryx*, *Xeropteryx*, and *Æschropteryx*.—The eighteenth contribution to the mollusca of the *Challenger* Expedition, by the Rev. E. Boog-Watson was read, in which the author treats of the family Tornatellidæ, therein describing six new species of the genus *Actæon*.

Geological Society, March 7.—J. W. Hulke, F.R.S., president, in the chair.—Messrs. Thomas Gustav Hawley, Richard Lydekker, and J. O'Donoghue were elected Fellows, and M. F. L. Cornet, of Mons, a Foreign Correspondent of the Society.—The following communications were read:—On Gray and Milne's seismographic apparatus, by Thomas Gray, B.Sc., F.R.S.E. Communicated by the President. This apparatus was stated to have for its object the registration of the time of occurrence, the duration, and the nature, magnitude, and period of the motions of the earth during an earthquake. The instrument was made by Mr. James White, Glasgow, and is to be used by Prof. John Milne in his investigations in Japan. In this apparatus two mutually rectangular components of the horizontal motion of the earth are recorded on a sheet of smoked paper wound round a drum, kept continuously in motion by clockwork, by means of two conical pendulum-seismographs. The vertical motion is recorded on the same sheet of paper by means of a compensated-spring seismograph. In details these instruments differ considerably from those described in the *Philosophical Magazine* for September, 1881, but the principle is the same. The time of occurrence of an earthquake is determined by causing the circuit of two electromagnets to be closed by the shaking. One of these magnets relieves a mechanism, forming part of a time-keeper, which causes the dial of the timepiece to come suddenly forward on the hands and then move back to its original position. The hands are provided with ink-pads, which mark their positions on the dial, thus indicating the hour, minute, and second when the circuit was closed. The second electromagnet causes a pointer to make a mark on the paper receiving the record of the motion. This mark indicates the part of the earthquake at which the circuit was closed. The duration of the earthquake is estimated from the length of the record on the smoked paper and the rate of motion of the drum. The nature and period of the different movements are obtained from the curves drawn on the paper.—Notes on some fossils, chiefly Mollusca, from the Inferior Oolite, by the Rev. G. F. Whidborne, M.A., F.G.S.—On some fossil sponges from the Inferior Oolite, by Prof. W. J. Sollas, M.A., F.G.S. Some fossil sponges have been described from the Inferior Oolite of the Continent, but hitherto none have appeared in the lists of fossils from this formation in British localities. The collection of sponges described by the author was made by the Rev. G. F. Whidborne. The author described eleven species (six of which he identified with those already described from Continental localities) belonging to nine genera, and concluded his paper with some general remarks. These sponges are calcareous, but are considered by the author to have been originally siliceous, replacement of the one mineral by the other having taken place as already noticed by him. The beds in which these sponges are found bear all the appearance of being comparatively shallow-water deposits.—On the Dinosaurs from the Maastricht beds, by Prof. H. G. Seeley, F.R.S., F.G.S.

EDINBURGH

Royal Society, March 5.—The Right Hon. Lord Moncrieff, president, in the chair.—Prof. Turner, in a paper on bicipital ribs, described two examples which he had recently come across in the human subject. In both of these cases, one of which closely resembled a specimen in the Anatomical Museum of the University which Knox had explained as due to the fusion of a cervical with a thoracic rib, the real cause was the union of the two first thoracic ribs. That the former explanation was the true one in certain instances was demonstrated by other specimens; and the distinctive peculiarities of each kind of fusion were pointed out.—Sir William Thomson read two papers on gyrostatics and on oscillations and waves in an adynamic gyrostatic system. The papers were in great part experimental illustrations of the theorems regarding gyrostatic stability which are laid down in Thomson and Tait's "Natural Philosophy" (second edition, vol. i. part i. § 345). It was thus demonstrated to the eye that a system when under gyrostatic domination is stable in positions for which, statically considered, the system is unstable as regards an *even* number of degrees of freedom; so that, to take a particular case, a gyrostat which is unstable, because statically unstable as regards one mode, is rendered stable by making it statically unstable as regards two modes. Hence also an ordinary spinning top is stable because it is statically unstable in two of its degrees of freedom. The curious behaviour of a gyrostat resting horizontally on gimbals with its axis of rotation vertical was also shown, viz. its instability as soon as the framework on which it rested was moved in the opposite rotational sense to the spin of the gyrostat. The author then proceeded to point out that all phenomena of elasticity which are ordinarily treated by assuming forces of attraction or repulsion between parts or stresses through connections can be as readily explained by the assumption of connecting links subject only to gyrostatic domination. The gyrostatic hypothesis led to other consequences which the ordinary dynamic assumption did not involve; but it had not been found as yet that elasticity had properties corresponding to these.—Sir William Thomson also communicated a paper on the dynamical theory of dispersion, which was virtually an application of the principle of forced vibrations to a molecular structure, each molecule forming the nucleus of a region whose density increases gradually from without inwards. As bearing upon the same kind of problem, a model was shown illustrating Prof. Stokes' dynamical theory of fluorescence, which is that, if the first of a connected chain of elements is disturbed by a periodic disturbance having no close relation to the free vibration periods of the chain, the disturbance does not pass along the chain, but has its energy stored up in the first few elements, to be given back again when occasion offers.

PARIS

Academy of Sciences, March 19.—President, M. E. Blanchard.—The following communications were read:—Summary description of a new system of equatorials and its installation at the Paris Observatory, by M. M. Lœwy.—Observations of the Swift-Brooks comet made at the Paris Observatory, by M. Périgaud.—Graphic proof of Euler's theorem on the partition of pentagonal numbers, by Prof. Sylvester.—Observations on blue milk (second part), by M. J. Reiset.—On the second edition of the "Pilot of Newfoundland," of Admiral Cloué, and on a question of atmospherical optics, by M. Faye.—Function of the lymphatic vessels in the production of certain pathological phenomena, by M. Alph. Guérin.—The following memoirs were presented:—On the possibility of increasing the irrigation waters of the Rhone, by means of reserves to be established in the lakes of Geneva, Bourget, and Annecy, by M. Ar. Dumont.—Determinations of longitudes effected at Chili, by the Transit of Venus Expedition, by M. de Bernardières.—On the number of the divisions of an entire number, by M. T. Q. Stieltjes.—On the equations to the partial derivatives, by M. G. Darboux.—On the application of the elliptic and ultra-elliptic intervals to the theory of unicursal curves, by M. Laguerre.—Table of reduced positive quaternary quadratic forms of which the determinant is equal or inferior to 20, by M. L. Charve.—Method of obtaining the formula giving the general integral of the differential equation—

$$x^n \frac{d^n y}{dx^n} + A_1 x^{n-1} \frac{d^{n-1} y}{dx^{n-1}} + A_2 x^{n-2} \frac{d^{n-2} y}{dx^{n-2}} + \dots + A_n y = f(x)$$

by means of a definite multiple integral, by Abbé Aoust.—New equations relative to the transmission of force, by M. Marcel Deprez.—The transmission of force by batteries of electrical apparatus, by M. James Moser.—On the maximum yield which a steam motor may attain, by M. P. Charpentier.—Influence of tempering on the electrical resistance of glass, by M. G. Foussereau.—On a modification into the bichromate of potassium pile to adapt it for lighting, by M. Trouvé.—On the calories of combination of the glycolates, by M. D. Tommasi.—On mononitrosoresorcine, by M. A. Févre.—Contributions to a study of the plastering of wires, by M. P. Picard.—Physiological effects of coffee, by M. J. A. Fort.—On salmon-breeding in California, by MM. Raveret-Wattel and Bartel.—On the solenoconchal molluscs of the deep sea, by M. P. Fischer.—Ovogenesis among the Ascidians, by M. Ad. Sabatier.—Influence of the wind on meteorological phenomena, by M. E. Allard.—On the hailstorm of March 9 at the Hyères Salines, by M. Le Goarant de Tremelin.—The Alfianello meteorite, by M. Denza.

CONTENTS

PAGE

Fire-Fountains 525
Our Book Shelf:—
  Macdonald's "Africana, or the Heart of Heathen Africa" . . 526
Letters to the Editor:—
  Natural Selection and Natural Theology.—Prof. Asa Gray; George J. Romanes, F.R.S. 527
  The High Springs of 1883.—P. L. Sclater, F.R.S. 529
  Scorpion Suicide.—C. Lloyd Morgan 530
  Nesting Habits of the Emu.—Alfred W. Bennett 530
  The Recent Cold Weather.—William Ingram 530
  Sap-Flow.—F. M. Burton 530
  Foamballs.—J. Rand Capron 531
  Meteor, the Transit; the Comet.—Consul E. L. Layard . . . 531
  Ticks.—W. E. L. 531
  Ignition by Sunlight.—Major W. J. Herschel; Edmund H. Verney 531
  Mimicry.—H. J. Morgan 531
  Braces or Waistband?—R. M. 531
Singing, Speaking, and Stammering, II. By W. H. Stone, M.B., F.R.C.P. 531
Professor Schiaparelli on the Great Comet of 1882. By Francis Porro 533
The Soaring of Birds. By Lord Rayleigh, F.R.S. 534
Philip Christoph Zeller. By R. McLachlan, F.R.S. . . . 535
The Great International Fisheries Exhibition (*With Illustration*) 536
Notes 538
Our Astronomical Column:—
  The Great Comet of 1882 540
  Variable Stars 540
  The Late Transit of Venus 541
Geographical Notes 541
Facts and Considerations Relating to the Practice of Scientific Experiments on Living Animals, commonly called Vivisection 542
The British Association and Canada 546
University and Educational Intelligence 547
Societies and Academies 547

THURSDAY, APRIL 12, 1883

THE VIVISECTION BILL

THE failure of Mr. Reid's Vivisection Abolition Bill on April 4 affords cause of congratulation to all who are interested in science, although it is perhaps to be regretted that the Bill did not come to a "division" instead of being "talked out." Scientific men must be pleased because one more attempt of ignorance to stop the pursuit of knowledge has been defeated. But, more than this, the failure of the Bill is a boon to all who care for their own health, for that of their families, and for the welfare of society at large. Had it passed it would not only have stopped all experiments in physiology, pathology, and pharmacology in this country, but it would have rendered impossible the detection of crime by the application of physiological tests. Had this Bill been law at the time of the trial of Lamson for poisoning by aconite, his conviction would have been impossible; for although chemical evidence pointed to aconite as the poison used, the tests for it were not sufficiently distinctive to have justified his conviction on chemical evidence alone, and it required to be corroborated by physiological evidence. This was afforded by the injection of the substance obtained from the stomach into some small animals. As these died presenting all the symptoms of aconitine poisoning, the chemical evidence was confirmed, and the poisoner was accordingly convicted.

Under the present law, considerable delay was caused before a certificate could be obtained to allow these experiments to be performed, but if Mr. Reid's Bill had been law, they could not have been performed at all; and secret poisoners secure of immunity might have become as common in this country as they were in the days of the Borgias.

To understand thoroughly the effect of the Bill upon medical science and practice, we must imagine to ourselves what would occur if experiments were stopped not only in this country but in others; for it is not alone in this country that the opponents of vivisection are active; they are endeavouring to stop it as far as they can in America and on the Continent also.

Last week we published some facts and considerations regarding vivisection and its relations to medicine, issued by the Association for the Advancement of Science by Research. The data there contained we should think were sufficient to convince any reasonable person of the advantages that medicine has derived from experiments on animals. But it is curious to notice the way in which they are regarded by anti-vivisectionists. Finding themselves in many cases unable to deny the advantages of the knowledge which has been obtained by experiments, they say this knowledge might have been got without experiments, and so it might, if man had been differently constituted. But being as he is, there is no royal road to knowledge, and he must take the only one which is available for him—that of experiment.

As Mr. Cartwright pointed out in his speech, if experiments on animals are prohibited, experiments must be made on human beings, and in their rudest form. The contrast between such rude popular experiments on man and scientific experiments on animals was illustrated in a speech of Dr. Lyon Playfair in reference to these two kinds of experiments on cholera. The first experiment was tried on 500,000 human beings in London, who were supplied with water contaminated by choleraic discharges with the result that 125 out of every 10,000 consumers died from the effects of the experiment. In two other experiments made by another water company, 180 died in the first experiment, and 130 in the second, out of every 10,000.

These popular experiments on a large scale involved the sacrifice of half-a-million human beings. In contrast with this may be taken the scientific experiments made upon animals by Thiersch and others. These experiments were made on 56 mice, 14 of which died from the choleraic discharges. These were not mixed with water accidentally or carelessly, as in the popular experiment, but were administered under definite conditions, and the effect watched. The results of these experiments showed that water contaminated with choleraic discharges was deadly; the water so contaminated was avoided, and an epidemic was escaped.

The common-sense conclusion on the whole matter was expressed by the Home Secretary, who said that he disliked as much as any man in the House the infliction of pain upon animals, but felt satisfied that under the administration of the law at present there was very little pain inflicted upon animals, and that pain was inflicted under such circumstances as to guarantee that it was not wantonly inflicted, but that it had occurred in the course of experiments that were abundantly justified for the benefit of humanity at large. As a guarantee that no experiments shall be performed that are not abundantly justified, Sir W. Harcourt has made the agreement with the Association for the Advancement of Science by Research, that, "if they will undertake the task of reporting to him upon the experiments, he will undertake that no certificate shall be granted except on a previous recommendation from them." This Association is a representative body of the whole medical profession, being composed of the Presidents of the Royal College of Physicians and Surgeons of London, Edinburgh, and Dublin, of the Royal Society of London, of the Medical Council, and of all the chief medical associations and societies, along with some others specially elected. It would be difficult to imagine a body better adapted for the purposes of maintaining the high character of the profession for humanity, by preventing any wanton infliction of pain upon animals by experiment, whilst at the same time preventing the serious consequences to human health and life which would ensue if properly devised experiments were prohibited by ill-judged and excessive care for animals.

THE BRITISH NAVY

The British Navy: its Strength, Resources, and Administration. By Sir Thomas Brassey, K.C.B., M.P. Vols. I., II., III. (London: Longmans, Green, and Co., 1882.)

THE three volumes of this work already given to the public by Sir Thomas Brassey are to be followed by three others; but as these are to contain reprints of speeches and publications on naval affairs it is preferable to notice separately the first half of the series, which is complete in itself. No better description of the scope and intention of the book can be given than that appearing in the Introduction, where it is described as "a comprehensive summary of all that has hitherto been published, whether in England or abroad, concerning the most important fighting vessels of modern times." It is avowedly a compilation rather than an original work, and Sir Thomas Brassey has rendered a most valuable service to all persons interested in naval affairs by undertaking the very laborious task now completed. He states that it has extended over twelve years, and it must often have seemed as if the end would never be reached in view of the rapid progress being made in naval armaments, and the large number of publications which have appeared in recent years dealing with war-ships, their armour, armament, and equipment. To keep abreast of this progress, and at the same time to retrace the history of war-fleets during the last quarter of a century, must have been a most arduous undertaking, and the author of these bulky volumes must be congratulated on his industry and perseverance. As the result he has produced an unrivalled book of reference, which should be in the hands of all naval officers, ship designers, shipowners, and administrators of naval affairs.

It is a singular fact that until this book appeared English readers had to turn to foreign publications for the best accounts not merely of foreign navies but of the British Navy. There was no English rival to the books produced by Dislére or Marchal in France; by Littrow, Brommy, or Kronenfels in Germany; by Von Tromp in Holland; and by King or Véry in the United States. Scattered notices in the press, meagre Parliamentary papers, the scanty facts respecting H.M. ships given in the Navy List, and the special information afforded by Reports of Commissions or Committees were the best sources of supply open not merely to the general reader but to most naval officers. Sir Edward Reed, in 1869, dealt with the general problems of armoured construction in "Our Ironclad Ships," but the character of that work excluded the detailed descriptions of individual ships and the statistics of various fleets which are most needed in discussions of the relative powers of maritime countries. This want in English literature Sir Thomas Brassey has admirably supplied. His book is better than all its foreign predecessors, and this may be said without offence, seeing that he has been able to draw freely from them, frankly acknowledging his indebtedness. Coming later into the field, he has also been able to add much valuable information not to be found in the earlier books; while in style of production, wealth and beauty of illustration, and moderate price, the "British Navy" stands alone. It is only proper to mention that Sir Thomas Brassey has evidently desired to secure a wide circulation for his book among naval officers, irrespective of the cost of production; and it is to be hoped that his wish will be realised, for it is clearly of the utmost importance that those who have to fight our ships should be well informed as to the characteristics of the ships with which they may be engaged.

Like all compilations this book requires very careful reading. The author gives, in every case, the fullest detail as to the authority from whom he is quoting; but he does not compare or correct various statements on the same subject, or attempt to appraise the relative value of the opinions of the writers from whom he quotes. This is left to the reader. A careless or hasty consultation of the book might therefore lead to wrong conclusions, and a word of warning on this point may not be out of place. For instance, one may find in close succession statements by Admiralty officials, or private shipbuilders who have designed and constructed foreign vessels, or officers of foreign governments—all of which are to be reckoned authoritative—and statements by anonymous or unofficial writers in various publications—some of which, at least, are of doubtful authority. The reader should turn, therefore, in all cases to the admirable "List of Authorities" in order to make sure whose opinions he is studying before adopting them.

Sir Thomas Brassey undoubtedly did wisely in not attempting to reconcile or correct the various statements which he has summarised. Had he done so before accepting office at the Admiralty, the task would have been beyond his power of accomplishment even for the Royal Navy, since it could only be performed by the freest use of official records; and for foreign navies the difficulties would have been obviously greater. As a matter of fact, before the publication of the book took place the author had accepted office as Civil Lord at the Admiralty, and thus had an additional reason for avoiding the difficult task. He is careful to explain that the publication is in no sense an official one, the work having been far advanced before he went to the Admiralty, and having been completed on the lines previously laid down.

This is only one of the many incidental illustrations of the magnitude of the work done, and the difficulty of bringing such a book up to date. For instance, in the second volume, issued in 1882, the author has to explain that the figures given for the naval strengths of various countries date from 1879. Again, the descriptions of progress and experiments in armour and guns, full as they are, necessarily leave unnoticed many important events of recent occurrence which must affect future war-ship construction. Even if a new edition could be produced speedily, and quite up to date, it too would soon need additions.

The author has had many reminders of the fact that although his book is announced as "unofficial," it may be used as an aid to criticism of the action of the Board of Admiralty, of which he is a member. Admiral of the Fleet Sir Thomas Symonds, and other advocates of a more energetic policy in naval affairs, have found many arguments in support of their views in these volumes. Into this controversy we have no intention to enter, but it may be observed that Sir Thomas Brassey, who must be as familiar with the facts as most persons, remarks that, "On a general and dispassionate review of our position, we are led to the conclusion that the naval power of England, in all the vital elements of strength, is greater now than in any former age." This may be true, but Sir Thomas Brassey would also be the first to admit that continued and strenuous efforts are required in order that this position may be maintained.

The first volume is chiefly devoted to armoured ships; a brief description of unarmoured ships being appended. Elaborate tables of the dimensions, speeds, cost, thicknesses of armour, weight of guns, &c., are given for the navies of the world; numberless diagrams and drawings also appear in illustration of distributions of armour, arrangements of armament, character of structural arrangements, design and position of propelling machinery, &c. Besides these there appear a large number of very beautiful woodcuts of typical ships, from designs by the eminent marine artist, the Chevalier de Martino, who was formerly an officer in the Italian Navy, and possesses a seaman's knowledge of ships in addition to his ability as a painter. These diagrams, drawings, and tables taken alone are of the greatest value, and if published separately in a handy form ought to command a large circulation. Sir Thomas Brassey would add to the debt of gratitude we already owe him if he undertook the issue of such a publication, rivalling the French "Carnet de l'Officier de la Marine," or the Austrian "Almanach für die Kriegs Marine."

The second volume deals with "miscellaneous subjects" of great interest and importance. Amongst these are a fuller discussion of unarmoured ships, of torpedoes and torpedo boats, harbour defence and coast defence ships, the employment of mercantile auxiliaries on war services, and many other topics. Amongst these none exceeds in importance the discussion of the possible employment of our merchant steamships in time of war. The means for securing the aid of these vessels when the necessity arises, and of best equipping them, require the gravest consideration. Already something has been done in this direction by the Admiralty, but much more yet remains to be done, if at the time of need the best of our unrivalled merchant ships are to be available for the defence of the mercantile marine or the many other services on which they might be employed.

The third volume is devoted to a summary of opinions on the shipbuilding policy of the Navy. It is in some respects a curious collection, but will well repay a careful study. The classification by the author of this mass of opinions greatly assists the reader. Unanimity on any point is scarcely to be hoped for, and is not to be found; but the reader will find ample suggestion and food for reflection. The advocates of small ships are fully represented; the designers of the *Italia* and *Lepanto* have their views set forth. Those who believe in armour-protection, and those who think it should be abandoned, obtain an equally fair audience. And in these, as in most other matters, the author gives little or no prominence to his own opinions.

Sir Thomas Brassey has given many proofs of his devotion to the naval interests of this country during his Parliamentary career; but by the publication of this work he has established a claim on the gratitude of all classes of English readers who take an interest in naval affairs.

W. H. WHITE

OUR BOOK SHELF

Camps in the Rockies. By W. A. Baillie-Grohman. Map and Illustrations. (London: Sampson Low and Co., 1882.)

MR. BAILLIE-GROHMAN has already made himself known as an intrepid hunter, a close observer of nature, and a charming *raconteur*. In the volume before us he shows no falling off in any of these points, and seems quite as much at home among the parks and peaks of the Rocky Mountains as he is among the chamois-haunted precipices of the Tyrol. The present volume is the result of more than one visit, mainly for sporting purposes, to the Far West, between the Yellowstone Park and Utah. Of the wild life of the ranchers and hunters of the region he has much to tell, and many exciting stories of his own hunting experiences. He adds, moreover, not a little to our knowledge of the topography, geology, and natural history of a region, of many parts of which we yet know little. On the Cañons of the Colorado region he has some interesting notes. We shall be pleased to have another such book from Mr. Grohman.

Physics in Pictures: the Principal Natural Phenomena and Appliances Described and Illustrated by Thirty Coloured Plates for Ocular Instruction in Schools and Families. With Explanatory Text Prepared by Theodore Eckardt, and Translated by A. H. Keane, M.A.I. (London: Stanford, 1882.)

THESE plates are somewhat rough and occasionally violent in colouring, but perfectly trustworthy, and well calculated to interest young people and convey to them a clear idea of the elementary scientific truths intended to be illustrated. The accompanying text gives all the explanation necessary. The plates embrace a wide field of subjects in mechanics, navigation, magnetism and electricity, sound, optics, photography, colours, spectroscopy, &c. We hope the collection will find its way into many schools and families.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Unprecedented Cold in the Riviera—Absence of Sunspots

IN the second week of March Cannes was visited by falls of snow and degrees of cold far exceeding any of which there is previous record. The preceding part of the winter was of average mildness; the minimum thermometer having fallen below freezing only three times, as follows: December 2, 32°; January 24, 29°; January 26, 31·8°. Not once did it fall so low during February; the average minimum being nearly 44°, and the maximum in shade 56°, and was apparently steadily rising with the approach of spring. The following notes are extracted from my diary:—

February 28.—Thermometer, minimum 46°·6, maximum 58°·3;[1] barometer 29·65. Day fine. Wind W., calm. No spot on the sun.

March 1.—Th. min. 43°·3, max. 58°·3; bar. 29·46. Day fine. Wind N.E., moderate.

March 2.—Th. min. 43°, max. 57°·5; bar. 29·42. Fine, with haze. Wind N.N.E., very strong in p.m.

March 3.—Th. min. 42°·8, max. 55°·3; bar. 29·70. Fine, but strong wind from N.E. Not a spot on the sun.

March 4.—Th. min. 35°, max. 54°·8; bar. 29·70. Wind very strong from N.E. Fine, with cumuli.

March 5.—Th. min. 40°, max. 54°·8; bar. 29·70. Cloudy, nimbostratus. Wind very high from N.E.

March 6.—Th. min. 40°, max. 51°·7; bar. 29·40. Fine, but some clouds. Wind N.E., very high and cold.

March 7.—Th. min. 36°·8, max. 53°; bar. 28·87. Snowed in night in large flakes, and till 10 a.m. to depth of 8 inches. Little wind, N.E. The weight of the snow bowed down shrubs and trees, breaking many. In a large shrubbery in my garden, *Erica arborea*, from 10 to 20 feet high, full of flowers, all prostrated. *Mimosæ* of various kinds, also flowering, and the more tender palms, were borne down and broken. Pelargoniums and other succulent shrubs destructively crushed. Partial thaw in the sunshine.

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[1] Thermometers by Casella. Minimum is placed every night outside an east window of the first floor of my villa, the bulb being protected from radiation. Maximum lies shaded inside the same window, open by day. Barometer, aneroid, by Pillischer.

March 8.—Th. min. 27°·7, max. 51°·3; bar. 28·83. Sunshine in morning began a thaw, but only to discover mischief done by the frost. Wind first from N.E.; in p.m. from S.W., increasing thaw.

March 9.—Th. min. 35°, max. 51°; bar. 29·67. Rain in night and most of day, but later turned to snow in large flakes. Wind S.E.

March 10.—Th. min. 27°, max. 44°; bar. 28·88. Fresh snow in night to depth of 4 or 5 inches. Whole country white, including Esterel Mountains, on which snow is hardly ever seen. Wind W., rising, threatening a mistral. Only two small spots on the sun.

March 11.—Th. min. 24°·1, max. 45°; bar. 28·84. Bright morning, but intense cold with mistral, at night destroyed almost all tender plants and shrubs in garden, in spite of covering. One fine young indiarubber-tree of 15 feet, with its rich green and bronze leaves, turned in the night to a spectre of limp black rags. Wind W., calm. Only one small spot on S E. border of sun.

March 12.—Th. min. 25°·7, max. 49°; bar. 28·90. Sun bright, but hard frost everywhere except in sheltered places. Wind W. strong. Four spots now visible on sun, one larger than the rest, and near it a large oval facula of brighter light.

March 13.—Th. min. 32°·1, max. 49°·6; bar. 29·30. Weather bright, wind W., moderate. Two of the four spots larger, with deeper umbræ; suspicion of a facula near one.

March 14.—Th. min. 29°, max. 54° (?); bar. 29·50. Sky bright, some haze, wind W. Four sunspots, less marked, varying from day to day; one, which was a penumbral streak, now hardly visible.

March 15.—Th. min. 32°, max. 50°·4; bar. 29·30. Weather feels much warmer, wind W.S.W.; one of the sunspots much larger, with a rent of dark umbra within.

March 16.—Th. min. 36°·7, max. 50°·3; bar. 29·19. Weather fine, a little haze, wind W.S.W. Now five spots; two large, with dark irregular centre and fringe of penumbra; two dark, without fringe; one a mere streak of penumbra.

March 17.—Th. min 41°·9, max. 52°·2; bar. 29·22. Fine in morning, but hazy; later, clouds from S.W. (showing rain-band) gathered, and brought first hail, then rain for two or three hours; later, the sun appeared with one of the new spots much enlarged, consisting of a penumbra with two distinct dark clefts within.

March 18.—Th. min. 35°·1, max. 53°·9; bar. 29·48. Bright morning, with haze, wind S.S.W. No change in sunspots.

March 19.—Th. min. 45°·9, max. 52°·5; bar. 29·20. Morning gloomy, with clouds and rain. The wave of cold seems to have passed, but not so the vast deposits of snow left on the mountains behind, and still less the unknown detriment inflicted on vegetable life in the olive and orange groves around us.

The foregoing observations are too few and too imperfect to warrant any decided conclusions, but they add to those already made in evidence of the connection between the absence of sunspots and the diminution of terrestrial heat; and I trust they may be followed by further and more exact investigations to determine the influence of our great luminary on the weather and climate of the world. How far this "cold wave" has extended to other countries and latitudes I am not informed; but it seems to me that their usually cloudless skies bring the shores of the Riviera into closer and more direct relationship with sun-power than other countries, and therefore render them more sensitive to its variations.

C. J. B. WILLIAMS

Cannes, March 19

Mr. Grant Allen's Article on "The Shapes of Leaves"

THE article by Mr. Grant Allen on "The Shapes of Leaves," published in NATURE (vol. xxvii. p. 439) as first of a series, calls for an emphatic protest on behalf of botanists, and especially of teachers of botany.

In his introductory paragraphs he at once cuts the Gordian knot of vegetable physiology in a most startling manner. He tells us that "from the free carbon thus obtained [*i.e.* by deoxidation of carbonic acid], together with the hydrogen liberated from the water in the sap, the plant manufactures the hydrocarbons which form the mass of its various tissues." If he had only substituted, by a slip of the pen, the term hydrocarbon for carbohydrate, it might have been regarded as a pardonable piece of negligence; but, since he speaks of "*free carbon*" and *hydrogen*, he shows that he really meant to write the word "hydrocarbons." Naturally he does not bring forward the results of any experiments which may have led him to make this extraordinary statement.

He goes on to say: "Vegetal life in the true or green plants consists merely in such deoxidation of carbonic acid and water, and arrangement of their atoms in new forms." Among other strange conclusions to be drawn from the above lines we see that, according to Mr. Grant Allen, either nitrogen does not enter into the composition of proteids, or that the latter have nothing to do with that "vegetal life" of which he speaks.

Articles containing blunders of such magnitude, but written with that assurance of style which naturally carries conviction to the mind of the unwary, and disseminated through the country in a widely read journal like NATURE, cannot but produce a rich crop of erroneous impressions. These it will be the arduous duty of teachers to eradicate.

Every one will agree that the popular writer must, before all things, be master at least of the first rudiments of the subject on which he writes: Mr. Grant Allen has in two consecutive sentences shown himself singularly deficient in this respect.

It would be premature here to enter upon a detailed criticism of these articles, since the series is not yet complete. But the two sentences I have quoted are so strangely heterodox that they could not be passed over without remark.

F. O. BOWER

As I do not think it necessary to preface four short papers on the shapes of leaves with a formal treatise on physiological botany, I am not careful to answer Mr. Bower in this matter. The word hydrocarbons was used deliberately, because the important point to notice is this—that the plant consists in the main of relatively deoxidised materials. From the point of view of energy, with which one has to deal mainly in treating of functions of leaves, that fact is of capital importance. I can conscientiously inform Mr. Bower that I was aware of the chemical constitution of proteids, and of the part which they bear in life generally; but I do not see what harm can be done to anybody by such a confessedly rough statement as that which he criticises. If we must always step aside to say all that we know about any subject whenever we have to deal with it, exposition of new matter becomes impossible. May I call Mr. Bower's attention to the further fact that in the same paper I spoke of the plant catching "fragments of carbon," meaning thereby not free carbon, but carbon in the form of carbonic acid, even though it be merely reduced from carbon dioxide to carbon oxide. It seems to me that such roughly accurate language is permissible in popular writing, where one's main object is to insist only on the general principle involved. It is the carbon that the leaf wants, not the oxygen; it is the carbon and the hydrogen that it deals with, not the nitrogen, which is but the instrument for dealing with them; and the two other elements may therefore be safely neglected. Or must we drag in sulphur, and potassium, and calcium, and all the rest as well?

GRANT ALLEN

Ticks

IF W. E. L. will acquaint himself with the somewhat scattered literature of this subject he will find that much useful information has already been placed on record by entomologists and others. The *Farm Journal* for July 10, 1880, contains a sensible and convincing article by Mr. James Elliot, showing the connection between ticks and louping-ill. A good article on the sheep-tick (falsely so called, since it is an insect and not one of the Ixodidæ) occurs in *The Field* for April 26, 1873. The scientific aspects of the subject are well treated of by Mégnin, especially in relation to classification in his "Monographie de la Tribu des Sarcoptides Psoriques," 1877. Mr. Hulme's edition of Moquin-Tandon's "Elements of Medical Zoology" has a useful chapter on ticks (p. 302). Some valuable hints are given in Prof. Verrill's Report on parasites to the Connecticut Board of Agriculture, 1870. An excellent article with good figures on *Melophagus ovinus* appeared in one of the volumes of the *Intellectual Observer*. The ticks of the sheep and stag are both figured in Van Beneden's "Animal Parasites" (English edition of International Series, p. 177). The sheep-tick is likewise figured and described in the "Micrographic Dictionary." References and figures are also given in the standard works of Westwood and Packard on insects. As W. E. L. is probably a practical man, he will do well to consider the proofs afforded by Mr. Elliot that the "ked," as they call it in Scotland, is anything but the harmless insect which some people imagine it to be.

T. SPENCER COBBOLD

I AM inclined to think your correspondent W. E. L., on the subject of "ticks" (p. 531), may have confounded two quite distinct animal forms under that name. The sheep-tick or louse, as shepherds call it, found at the roots of the wool on sheep, and which I have often formerly had brought to me under one of those names, is an aberrant form of *Hippobosca*, a genus of dipterous insects, the typical species being the well-known forest-fly. An excellent figure of the sheep tick will be found in Curtis's "British Entomology," Pl. 142, under the name of *Melophagus ovinus*.

Ixodes is a genus of the Acaridæ, a group easily distinguished from the true insects by their having eight legs in the adult state. Six British species of *Ixodes* are described by Dr. Leach in vol. xi. of the *Linnean Transactions*. There are probably others not as yet determined. The one best known is the common dog-tick, found in a free state in woods and plantations, and attaching itself not merely to dogs but to hares, &c., and especially to hedgehogs, which often abound with them, the ticks getting their hold as the animals pass through the close grass. After attachment they soon get gorged with blood, their abdomens swelling to an immense size compared with the insignificant appearance of them previous to attachment. But I can remember no instance of an Ixodes found on a sheep, though I would not undertake to say they never occur on that animal.

Bath L. BLOMEFIELD

Helix pomatia, L.

I AGREE with Mr. Gwyn Jeffreys (NATURE, p. 511) in considering *Helix pomatia* as indigenous in this country, and not introduced by the Romans. I never found or heard of a single specimen, either living or dead shell, being met with in the neighbourhood of Bath, which the Romans occupied for more than 400 years, though it is found in one or two localities in the adjoining county of Gloucestershire, from whence we have specimens in the museum of the Bath Literary Institution.

Bath L. BLOMEFIELD

Braces or Waistband?

HAVING worn a Spanish sash for some time many years ago while walking in the Pyrenees, I am decidedly of opinion that the weight of the trousers is supported much more easily and pleasantly by a sash than by braces; these last are narrow, about 2 inches wide, and though custom enables us to wear them without conscious inconvenience, I think any one using them for the first time would find them very unpleasant. The sash worn by the middle and lower class in Aragon is of wool 8 or 9 inches broad, and (if my recollection is correct) about 4½ feet long; when of such width and length it does not need to be drawn *tight*, but only closely wrapped round the waist and the end tucked in. I should certainly wear one constantly but that I do not wish to have an eccentric appearance. Medical men, I believe, attach great value to the wearing of sashes or bands round the stomach, especially in hot countries. A narrow silken sash which must be drawn tight is, I should suppose, far less pleasant to wear.

N.

SOLAR RADIATION AND GLACIER MOTION

IN the paper on the "Mechanics of Glaciers," which the author had the honour to read before the Geological Society of London in December last, it is stated that, after all allowance is made for work within the glacier due to the potential energy of the *weight* of the ice-mass, "there remains to be accounted for a *secondary* differential motion, which has, it appears, not yet received a satisfactory explanation . . . the movement is greater (*a*) by day than by night, (*b*) in summer than in winter." The present paper is intended as nothing more than a brief statement of the experimental evidence, upon the strength of which the explanation offered in the paper referred to has been put forward. I may say *en passant* that this investigation was suggested to me by a statement of Dr. Croll's ("Climate and Time," p. 519) that, "We find that the heat applied to one side of a piece of ice will affect the thermal pile on the opposite side." It occurred to me that the looseness of this statement was quite in keeping with the *un*physical notions upon which the writer has built up what he styles his "molecular theory" of glacier motion, and I set to work therefore to investigate its accuracy.

The principal apparatus used consisted of a delicate galvanometer, and a thermopile of a pretty high degree of sensitiveness, made up as it is of eighty-one couples of bismuth and antimony; the measurements were read off numerically by the light reflected on the scale as usual. Suspecting that the fallacy of the statement referred to lay in overlooking the effect of luminous energy, which of course is capable of passing through any *transparent* body, I made a few preliminary trials with glass and water, not having ice then at hand. A beam of solar radiation, having passed through two inches of distilled cold water + half an inch of glass, was allowed to fall upon a Crookes' radiometer; this made the vanes rotate too fast for their rotations to be counted, even when the instrument was enclosed in a wooden case on all sides except that open to the glass-water screen through which the sunshine passed. A beam of solar light, having been sifted of its dark heat-rays in the same manner as before, was received upon the absorbing face of the thermopile, producing a considerable deflection of the magnet in the galvanometer, even with the feeble sunshine of our recent December days.

The next step was a series of trials with *ice* itself. In the first instance, trials were made with the plates of *ice in contact with the metallic face of the pile*, the black (absorbing) face being placed at a distance of 3 inches opposite a large Bunsen flame in a room free from strong draughts: in this way a constant difference of 36° C. was obtained for the opposite faces of the pile, and maintained for more than half an hour, with the needle of the galvanometer quite stationary. An iron ball 3 inches in diameter, having been heated to dull redness (clearly perceptible in a dark room), was placed opposite the plate of ice (1 inch thick) in contact with the pile, and allowed to cool. It was again heated as before, and placed at a distance of *less than an inch* from the ice (now less than half an inch thick), and allowed to cool. In both cases the effect observed upon the galvanometer was *absolute nil*, even when, in the second trial, the ice had become so thin by melting as to break under the small force required to hold it against the pile.

In the next series of trials the arrangement was reversed, the *ice* being placed just in front of the condensing cone attached to the absorbing face of the pile at a distance of 4 inches; the metallic face of the instrument was maintained at a constant temperature by contact with a vessel of cold water, whose temperature was observed frequently, and found to be practically constant. On the distant side of the ice was placed a double board-screen, with air-space and a circular hole to allow the passage of a cylindrical beam of radiation of the same diameter as the condensing cone. The iron ball, heated to dull red heat as before, was placed opposite the hole of the screen, at a distance of 7½ inches from the face of the pile, the intervening ice-plate in this case being 1 inch thick, and the galvanometer having been stationary for half-an-hour before the experiment was made. Under the same conditions the experiment was repeated (1) with ¼-inch plate of ice; (2) with ¼-inch of pond-ice + wet half-melted snow; (3) with ⅝-inch of fresh-fallen snow. In all these cases the result of the obscure radiation from the ball upon the galvanometer was *absolute nil*, although, without the interposition of ice or snow, the maximum deflection at the end of 5 minutes was 460° on the scale (see accompanying table). This period of time was adopted for this reason, for the duration of each following experiment, though more than needed to produce maximum results. So far the evidence is conclusive that *dark heat* (*i.e.* heat capable of melting ice) applied to one side of a piece of ice does *not* affect the thermopile on the opposite side. So much for the negative results.

It seemed to me at this point worth while to investigate the effects produced by *luminous radiant energy* of various phases of quality after transmission through ice, which, it would appear, effectually barred the passage of all the obscure rays of the iron ball from even entering it, while the liquefaction of the ice at the surface was beyond all comparison greater than that which goes on at the surface of a glacier even with a full midsummer sun. The sources of *luminous energy* chosen are given in the first column of the following table. The feeble effect produced by the blue flame of a very large Bunsen lamp (giving no red, orange, or yellow when examined with the spectroscope) as compared with the effects produced by the more highly luminous gas-flames of far inferior thermal intensity (which gave, of course, a complete visible spectrum), is extremely interesting for the light it throws upon the subject in hand. The table of results explains itself at once to any student of physics. The lime-light used, it may be added, was a very powerful one; the sunshine, however, was not very bright or very constant, owing to the drifting of clouds. The latter fact explains the apparent slight anomaly in the results of the solar radiation given in Series II. and III. The observations were made however with the solar radiation (as estimated by a Crookes' Radiometer) approximately the same for them all.

Table to show the Sifting Power of Ice and Snow upon Radiation of different Phases of Quality

Sources of radiant energy.	*Series I.* Showing relative radiant energy	*Series II.* With an inch of very clear ice interposed.	*Ser. III.* With ¼ inch of clear ice (with many air bubbles) interposed.	*Ser. IV* With ¼ inch of pond ice (as in III) with much wet snow on one side.	*Series V* ⁴⁄₈ inch of fresh fallen snow interposed.
1. Red-hot iron ball, 3 inches diameter (at dull red heat)	460·00	0·00	0·00	0·00	0·00
2. Large Bunsen lamp flame (feeble luminosity) giving incomplete spectrum	135·00	2·00	2·00	0·00	0·00
3. Small Bunsen lamp flame, with air shut off below (giving complete spectrum)	77·00	6·00	4·00	2·00	0·00
4. Small fish-tail gas-burner	87·00	12·00	7·00	6·00	0·00
5. Lime-light... ...	192·00	51·20	38·40	20·48	0·00
6. The Sun	530·00	310·00	320·00[1]	—	13·00

The numbers in each series in the foregoing table do not give very simple relations among themselves, and each number must be regarded as only a near approximation to the exact truth. Still, when all those slight inaccuracies which arise from "errors of experiment" are allowed for, the general meaning and bearing of the facts remain, namely, that though heat (*quâ* heat capable as it is of melting ice) cannot enter ice, yet *luminous energy, which is readily absorbed and transformed into heat by opaque and semi-opaque bodies,* can enter and pass through the ice, until it meets with a non-transparent body. Substituting for our thermopile in the experiment, stones, dirt, organic germs, &c., within the glacier, we at once perceive how the luminous radiant energy of the sun can (by being transformed into dark heat) play its part in producing the movement of glaciers.

Further, this will be found, I believe, the *only* satisfactory explanation yet given of the remarkable facts (1) that *glaciers move faster* (in the Alps about twice as fast) *during the summer than during the winter; (2) that the motion during the day is greater than during the night.* This fact most people who have written on glaciers have found it difficult to explain, for when the "*Regelation Theory*" is fully accepted, and all that follows from it is recognised, and when due allowance is made for *internal friction*, we still must seek for a cause, independent of both of these, to account for the *variations* in the movements of glaciers, day and night, summer and winter. This cause has now, I think, no longer to be sought for.

The glacier may be compared to a large greenhouse; as luminous energy enters freely through the glass in the one case, so it enters freely through the transparent ice in the other; in both cases, heat available for work is produced by its transformation.

In the glacier this *work* is expended in diminishing the cohesion of the molecules of those parts of the ice which are in contact with the bodies which absorb the luminous energy. The beautiful silvery blue light of an ice-cavern seems to show that a part of a beam of luminous radiation is absorbed by clear ice.

The Series IV. and V. of the table illustrate the effect of (*a*) the more or less granular condition of the ice in many parts of a glacier, (*b*) the snow with which the glaciers are covered during the winter. The diffusive action of the latter upon luminous energy is seen by reference to Series V. to be very great; hence the necessity for the use of coloured spectacles on the higher glacier regions.

A. IRVING

DEDUCTIVE BIOLOGY

IT has probably occurred to a good many readers of NATURE that it would be well if some one were to utter a word of warning as to the mischief which may be done, and especially to students, by the present fashion of explaining all kinds of complicated morphological phenomena in a more or less purely deductive fashion. It is no doubt pleasant, even fascinating, to sit down at one's desk and, having formulated a few fundamental assumptions, to spin out from these explanations of what we see in the world about us. But I think when done it should be understood that the result is merely a literary performance, and though, viewed in that aspect, one may admire the skill and neatness with which it is accomplished, I nevertheless venture to think that the whole proceeding is harmful.

Now, as I shall attempt to illustrate my position by reference to papers which have appeared in NATURE in particular, I may as well say at once that I have no personal or merely controversial object in writing these lines. But though it is now no part of the business of my life to take part in teaching, I have had some experience of it, and a great deal too much of testing its results by the process of examination. I have derived then a tolerably definite idea—as I believe—of the difficulties that beset the imparting of scientific instruction, and a decided conviction as to what sort of discipline is wholesome, and what is mischievous.

Of course I do not deny—far from it—the inspiriting influence which large generalisations impart to teaching. But then I think the intellectual enjoyment of them must be earned. The first thing to do is to put before the student the facts, and then, when these are conscientiously mastered, to show what general conclusions may be drawn from them. The student will thus not merely appreciate the mastery which a comprehensive point of view gives of the subordinate facts, but he will get some insight into the value of the evidence upon which the induction rests, and be quite prepared to understand that in the face of a wider survey of observations it may have to be materially modified. This method of procedure seems to me to be not only the scientifically sound one, but to have an educational value of a very high order.

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[1] In this case a ¼-inch plate of *clear* ice was used.

The opposite method is to start with the general principles and derive the explanations from them. This no doubt affords play for ingenuity. But the intellectual discipline is immensely inferior. And when the elaborate structure is built up, it is impossible not to begin insensibly to resent with jealousy any criticism of its foundations, even when it has become difficult to resist the suspicion that they are decrepit. This state of things might be illustrated from the history of the biological sciences again and again. Generalisations which at first were justly hailed with enthusiasm have finally become mischievous obstructions in the way of their adherents arriving at a better knowledge.

I do not mean to say that I prophesy this fate for the evolution theory. But I confess I look with great dislike on the growing tendency, especially in writings intended for popular consumption, to explain everything by it deductively. We may think the probability of organic forms having been evolved is very great. But the *how* of the process is what in every case we have to prove. In this way the induction on which the theory of evolution rests perpetually widens its base, while at the same time our detailed knowledge of the subordinate laws through which it acts continually accumulates. But if, assuming the truth of the evolution theory, we proceed to spin out of our heads an explanation of how any particular phenomenon came about, I fail to see in what way we are the wiser. The theory of evolution runs a very good chance of being burlesqued; and at the best we find ourselves in possession not of a new knowledge, but merely of an ingenious literary exercise.

In several successive articles, a very able writer, Mr. Grant Allen, has discussed and given a deductive explanation of the shape of leaves. Now this is a matter on which a good many botanists have probably bestowed much thought, and it is well known to be beset with immense difficulties. I believe I am justified in saying that for the last ten years of his life it constantly engaged the attention of Mr. Darwin, and it cannot be doubted that if the problem had at all readily admitted of solution he would have at any rate made some attempt to do for leaves what he did for flowers. In work of this kind Mr. Darwin assumed nothing. His method was purely inductive. He made an immense number of observations drawn from the most widely severed types existing under the most varied conditions, and he gradually felt his way towards some general conclusions. But the fact is that the form of leaves, in common with a great deal of external morphology, is a product of a complex of conditions. Whatever general principles control it, we may be pretty sure that they do not lie on the surface. It is sufficient to mention a few of the obvious factors that must enter into the solution to see that this must be true. In the first place we have the conditions of development; a leaf which, like that of the wild hyacinth, has to be pushed up through compressed soil, must be shaped accordingly, and differently from one, such as that of a horse-chestnut, which languidly expands, like the wings of a butterfly newly escaped from its chrysalis, into the unresisting air. Then we have mechanical conditions; a leaf is a much greater feat of natural engineering than a stem; a fragile expanded structure has to be carried on a single support and supplied with a framework which must have the necessary rigidity not to collapse, and at the same time be carefully adjusted to withstand wind-strains. Then it must be adapted to meteoric conditions; it must be capable of withstanding solar radiation without being scorched, and its own reduction of temperature at night without being irremediably frozen. With this last circumstance is probably correlated the great variety of nyctitropic movements which leaves execute, and these again react on their form and construction. The enumeration might be very much prolonged; this is only a sample. But it will suggest to most people, as I imagine it did to Mr. Darwin, that, before asserting anything definite about the laws that govern the form of leaves in general, there is an enormous amount to be made out about their relation to particular circumstances of the environment.

But, as far as I can make out, all these considerations count as nothing with Mr. Grant Allen. "Two points," he says, "between them mainly govern the shapes of leaves." One of these is the relation of the leaf to sunlight: and the importance of this no one doubts. The other is the tendency of the plant "to have its whole absorbent surface disposed in the most advantageous position for drinking in such particles of carbonic acid as may pass its way." The importance of this, Mr. Grant Allen adds, "appears hitherto to have been too frequently overlooked."

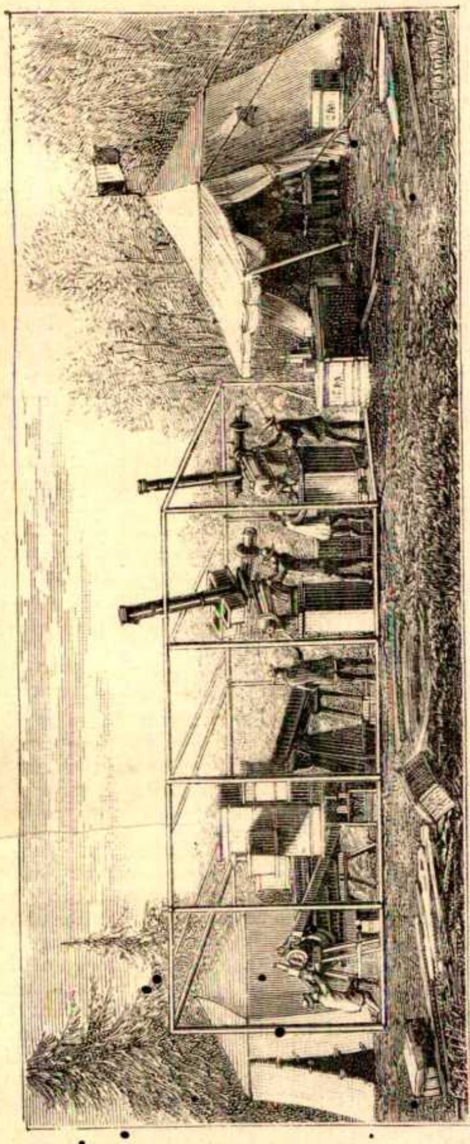
Now, as I have said, I think the deductive method is a bad way of solving morphological problems. It is still worse when the principle started from is more than doubtful. Mr. Grant Allen speaks of the competition of plants for carbonic acid as of the same kind as that of carnivorous and herbivorous animals for their respective foods. But it is surely nothing of the sort. Carbonic acid is an ingredient of the atmosphere to the extent of 1-2500th of its bulk. But only about one-quarter of the earth's surface is occupied by land, and from this a large deduction may be made on account of areas incapable of sustaining vegetation. There is therefore an enormous reserve of atmospheric carbonic acid which, as the atmosphere is rarely at rest, is constantly brought within the range of vegetation. Moreover, the carbon which plays its part in vegetation is continually being released from its organic trammels and the secular accumulation of carbon in the soil, though the work of vegetation is at most extremely slow. On what possible grounds then can Mr. Grant Allen talk of a competition for carbonic acid, which the wind that "bloweth where it listeth" perpetually and *impartially* supplies to the tissues capable of absorbing it? It cannot be doubted that, *per unit* of absorbent surface, one plant in a locality will get as much carbonic acid as another, no more and no less. And when I say per unit of absorbent surface, I do not mean external surface, which, as well as the shape, I apprehend has nothing to do with the matter. It is of no consequence how the chlorophyll-containing cells which bound the air-passages are massed into a leaf, provided that there is enough of them to do the carbon-fixing work of the plant. When, therefore, Mr. Grant Allen arrives at the conclusion that "the extent to which leaflets are subdivided depends upon the relative paucity of carbon in their environment," I confess that I should much like to see the experimental data, if any, on which this statement rests? As there are plants which at different periods of their lives produce much and little divided leaves, the point would possibly admit of being actually tested.

Now with regard to the submerged foliage of water-plants, I am free to admit that I think Mr. Grant Allen has made a point. These must absorb their carbonic acid superficially, being destitute of stomata and intercellular passages. But I do not see why he should say that the proportion of carbonic acid held in solution by water is very small. It is, I believe, never less than the proportion that occurs in the atmosphere, and may rise to nearly one per cent.

W. T. THISELTON DYER

THE APPROACHING ECLIPSE

THE accompanying illustration from *La Nature* shows the instruments to be used at the total eclipse of May 6, by M. Janssen, who has command of the French expedition. The illustration is after a photograph taken at M. Janssen's Observatory at Meudon. The French expedition, which has probably reached its destination, will be located on Sable Island, near Caroline Island, in the Marquesas Archipelago. Before quitting Paris, M. Janssen had all his instruments and tents erected in order to see that all worked well. The frame surrounding the apparatus is arranged to receive a large awning to protect them. The tent on the right is intended for the astronomers, the furniture consisting of a work-table, several camp-stools, and three beds. The little tent on the left is for photography. The instruments of the French expedition comprise—1. A telescope of short focus for spectroscopic work. 2. An equatorial on which will be arranged a photographic apparatus, containing five cameras which act together. The plates are $0^{m}\cdot40$ by $0^{m}\cdot50$; they will require an exposure of five minutes. This apparatus is intended for intra-Mercurial planets. 3. A telescope of 6 inches, with a lens of 3 inches, with photographic apparatus acting by means of three cameras at once. This apparatus is intended for the solar corona. 4. A fourth telescope, specially reserved for M. Trouvelot for drawings of the corona and search for intra-Mercurial planets.



Apparatus for French Eclipse Expedition.

DEATHS FROM SNAKE BITE IN BOMBAY

THE Report of the Sanitary Commissioner with the Government of Bombay shows that, among other causes of death in that Presidency in the year 1881, 1209 persons died from snake bite. The names of the snakes are not given, but it is probable that the cobra was the chief offender, the echis and bungarus accounting for those not slain by that snake. The monthly prevalence of deaths from this cause is interesting, as it shows at what period of the year efforts for destruction of snakes might be most effectively carried on; it also shows that there was an increase of thirty deaths on those of the preceding year; and it suggests that, however vigorous these efforts may have been, the result is not so satisfactory as could be wished, as a comparison of the deaths in 1881 with the mean of those of five preceding years shows that (in 1881 at least) the number had increased.

Months.	Deaths in 1881.	Mean of five years.
January	39	 30
February	34	 24
March...	55	 45
April	55	 49
May	95	 93
June	162	 135
July	191	 164
August	165	 159
September	161	 160
October	128	 144
November	80	 68
December	44	 39
	1209	1110

This (in 1881) proves that one person in 13,610 of the whole population of 16,450,414 for the twenty-four Presidency districts died from snake bite. June, July, August, September, and October are the months of greatest mortality, and it would be worth while inquiring if more vigorous efforts could not be made for the destruction of the snakes during these months, when it is presumed the creatures are more numerous and perhaps more active in their destructive work. The appearance and character of venomous as distinguished from harmless snakes ought now to be so well known in India that, whatever other difficulty may stand in the way of their destruction, absence of means of identification should not be one of the obstacles.

After all the mortality from snake bite is very small compared with that from other causes. The same able and most valuable Report shows that in the year 1881 there were 272,403 deaths from fever, of which no doubt a large proportion were due to miasmatic causes. The entire death-rate from all causes amounts to 381,450, or 23·18 per 1000 of the whole population. Against these death-rates and their preventable causes, whether from dirt, miasmata, foul water, or snake virus, the earnest endeavours of the sanitary authorities are now unremittingly directed, and it is impossible to read the Reports annually prepared by the Sanitary Commissioners without feeling impressed by their value and importance, or without a conviction that they must sooner or later have beneficial results on public health and the value of life in India.

JOSEPH FAYRER

ASTRONOMICAL PHOTOGRAPHY

THE important part that photography is likely to play in the future of astronomy renders it desirable that an opportunity should be afforded to astronomers to acquaint themselves with the improvements continually made in this branch of their science. This could best be done by the establishment at convenient places of collections designed to exhibit the progress of photography as applied to astronomical observations.

The Harvard College Observatory has some special advantages for forming such a collection, since it already possesses many of the early and historically important specimens which would naturally form part of the series. Among these may be mentioned four series of daguerreotypes and photographs of various celestial objects taken at this Observatory. These series were respectively undertaken in 1850, 1857, 1869, and 1882.

At present, the astronomers of the United States have no ready means of comparing their own photographic work with that done in Europe, or even with that of their own countrymen. The proposed collection of photographs, so far as it could be rendered complete, would greatly reduce the difficulty.

It is therefore desired to form, at the Harvard College Observatory, a collection of all photographs of the heavenly bodies and of their spectra which can be obtained for the purpose; and it is hoped that both European and American astronomers will contribute specimens to this collection. Original negatives would be particularly valuable. It may happen that some such negatives, having slight imperfections which would limit their value for purposes of engraving, could be spared for a collection, and would be as important (considered as astronomical observations) as others photographically more perfect. In some cases, astronomers may be willing to deposit negatives taken for a special purpose, and no longer required for study, in a collection where they would retain a permanent value as parts of an historical series. Where photography is regularly employed in a continuous series of observations, it is obvious that specimen negatives only can be spared for a collection. But in such cases it is hoped that some duplicates may be available, and that occasional negatives may hereafter be taken for the purpose of being added to the collection, to exhibit recent improvements or striking phenomena.

When negatives cannot be furnished, glass positives, taken if possible by direct printing, would be very useful. If these also are not procurable, photographic prints or engravings would be desirable.

In connection with the photographs themselves, copies of memoirs or communications relating to the specimens sent, or to the general subject of astronomical photography, would form an interesting supplement to the collection. A part of the contemplated scheme will involve the preparation of a complete bibliography of the subject, including a list of unpublished photographs not hitherto mentioned in works to which reference may be made.

The expense which may be incurred by contributors to the collection in the preparation and transmission of specimens will be gladly repaid by the Harvard College Observatory when desired.

EDWARD C. PICKERING,
Director of the Harvard College
Observatory
Cambridge, Mass., February 21

DARWIN AND COPERNICUS[1]

THE losses by death which natural science has sustained during the past year are unusually heavy. The fertile and ingenious mathematician who for more than a generation held a leading position among French men of science as the publisher of one of the best-known mathematical journals; the chemist who, by the first organic synthesis, helped to dispel the illusion of vital energy; the physiologist who solved one of the oldest problems of humanity—are men whose death leaves a void not easily filled up. But the lustre of even such names as Liouville, Wöhler, and Bischoff pales before that of the first on our list, Charles Darwin. Nearly every learned Society in existence has publicly deplored his loss. As this Academy has not hitherto found a fitting opportunity for doing so, it is necessary to add a few words to the formal mention of his decease, to show that we also appreciate the greatness of the man and of the loss science has sustained in him.

To say anything fresh concerning him will only be possible when the lapse of time and the progress of science have opened up new points of view; and the speaker, who has often had occasion to discuss Darwin before this Academy, finds it especially difficult not to repeat himself, the more so as opinions of his work are still somewhat apt to be influenced by personal feeling.

Darwin seems to me to be the Copernicus of the organic world. In the sixteenth century Copernicus put an end to the anthropocentric theory by doing away with the Ptolemaic spheres and bringing our earth down to the rank of an insignificant planet. At the same time he proved the non-existence of the so-called empyrean, the supposed abode of the heavenly hosts, beyond the seventh sphere, although Giordano Bruno was the first who actually drew the inference.

Man, however, still stood apart from the rest of animated beings—not at the top of the scale, his proper place, but quite away, as a being absolutely incommensurable with them. One hundred years later Descartes still held that man alone had a soul and that beasts were mere automata. Notwithstanding all the labour of naturalists since the days of Linnæus, notwithstanding the resurrection of vanished genera and species by Cuvier, the theory of the origin and interdependence of living things, which was almost universal five-and-twenty years ago, was only equalled in arbitrariness, artificiality, and absurdity by the celebrated theory of Epicycles, which caused Alfonso of Castile to exclaim, "If God had asked my advice when he created the world, I should have managed things much better."

"Afflavit Darwinius et dissipata est," would, alluding to the above-mentioned theory, be a fitting inscription for a medal in honour of the "Origin of Species." For now all things were seen to be due to the quiet development of a few simple germs; graduated days of creation gave place to one day on which matter in motion was created; and organic suitability was replaced by a mechanical process, for as such we may look on natural selection, and now for the first time man took his proper place at the head of his brethren.

We may compare Copernicus's student days at Bologna with Darwin's voyage in the *Beagle*, and his retired life at Frauenburg with Darwin's in his Kentish home, up to the time when the appearance of Mr. Wallace's work caused him to break his long silence. Here happily for Darwin the parallel ends. Many circumstances combined in Darwin's case to render his task easier and insure his ultimate triumph. Botany and zoology, morphology, the theory of evolution, and the study of the geographical distribution of plants and animals, had advanced far enough to allow of general conclusions being drawn from them; Lyell's sound sense had freed geology from the hypotheses which disfigured it, and introduced the idea of uniformity into science. The doctrine of the conservation of energy had been put on a new basis, and extended so that in combination with astronomical observation it gave rise to entirely new views of the history and duration of the universe. The doctrine of vital energy had been proved to be untenable on closer investigation. An unusually dry season had some years earlier led to the discovery of the so-called lake-dwellings in the bed of one of the

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[1] Address by Prof. E. Du Bois Reymond at the anniversary meeting of the Berlin Academy of Sciences.

Swiss lakes, whereby prehistoric research was quickly extended and developed. Though many links are still missing, we may fairly consider the knowledge of the existence of primeval man as the beginning of the long-looked for connection between him and the anthropoids on the one hand, and between them both and their common progenitors on the other. In a word the time had come for the publication of the "Descent of Man"; that is why an opinion on the nature of man, which differs from all former ones fully as much as the system of Copernicus, of which it is the complement, differs from that of Ptolemy, found such ready and general acceptance.

How different was the fate of Copernicus! "Copernicus," says Poggendorff, "is, and will ever remain, a brilliant star in the firmament of science; but he rose at a time when the horizon was almost entirely obscured by the mists of ignorance . . . The Ptolemaic system was too ancient and too much venerated to be easily displaced." Copernicus's teaching met with but scant appreciation for the first fifty years after its publication; even Tycho Brahe opposed it; it can therefore scarcely cause surprise that Luther rejected it, that Giordano Bruno died at the stake for his advocacy of it, while the less steadfast Galileo was forced to renounce it.

Notwithstanding the pessimism of our speculative philosophers, who deny all progress because they contribute nothing towards it, Darwin's lot was happier than that of the great reformer of astronomy. While Copernicus could only feast his eyes on the first printed copy of his work as he lay on his deathbed because he had not dared to publish it sooner, although he had completed it some years before, Darwin survived the appearance of his nearly a quarter of a century. He witnessed the fierce struggles its appearance at first gave rise to; its ever increasing acceptance and its final triumph, to which he, cheerful and active to the last, greatly contributed by a long series of admirable works.

While the Holy Inquisition persecuted the followers of Copernicus with fire and sword, Charles Darwin lies buried in Westminster Abbey among his peers, Newton and Faraday.

SINGING, SPEAKING, AND STAMMERING[1]

III.—STAMMERING

AFTER the emotional and intellectual sides of human utterance, what may be termed its pathological aspect was considered. Imperfections of speech, though serious hindrances to intercourse, are unfortunately not uncommon. It is not easy to realise how common they are. The statistics collected by Colombat point to the conclusion that about two persons in every thousand stammer, an estimate which is exactly borne out by official returns obtained in Prussia. This would make two and a half millions of stammerers in the world. But it is hardly fair to argue from the higher to the lower races of mankind, for stammering, like hysteria, is undoubtedly a disease of advanced civilisation. It was unknown among the North American Indians in Catlin's time; Livingstone says he never met with a case among the Negroes, and Cameron is stated to have confirmed the observation. It is uncommon in Spain and Italy, but reaches its maximum in highly-educated Prussia and in this country. "No nation in the civilised world," says Mr. Deacon, who has been already quoted, "speaks its language so abominably as the English."

Stammering appears to be commoner among males than females.

Laboured distinctions have been made between the two words, to stammer and to stutter, by which the infirmity is denoted. These seem to be wholly unnecessary, since they are practically synonymous. Both words contain an imitation of the defect itself. They probably reach us through the German language, but the ultimate root is the Greek Στείβω, and the fundamental meaning movement abruptly checked. There is indeed a whole series of allied old English words such as lag, dag, jog, shog, stag, and cognates are stab, stagger, stamp. In some parts of the country a horse is said to stammer when he trips in walking. Bacon, in his "Natural and Experimental History," says: "Many stutters are very cholerick, choler inducing dryness of the tongue." It was long ago noticed by Sir Charles Bell in his Bridgwater Treatise, that speech, like writing, walking, and other functions of life, is a coordinate muscular act involving many nerves as well as muscles, but which, having been learned early, has become so automatic that the directing of special attention to it rather hinders than assists in its easy performance. Indeed the act not only involves the mechanism of speech proper, but also that of thought and ideation, as well as that of hearing, by means of which the sounds emitted are discriminated. It thus may never be developed, as in idiocy, of which the failure to acquire it is often the first sign: or in congenital deafness, which is the precursor of dumbness. It may also disappear entirely or partially in conditions of cerebral lesion known to medical men under the titles of aphasia, aphemia, and amnesia, often accompanying hemiplegia of the right side of the body. Real stammering may be produced by mental strain or shock, and persist through life. Such cases are rare, but the lecturer has been allowed to refer in general terms to one which can easily be verified—that of a clergyman who, after being overtaxed physically and mentally during one of the earlier cholera epidemics, began to stammer, and though now an old man, has never since been able to officiate in the service of the Church. Mr. Plumptre, in his lectures on Elocution, quotes even a more remarkable case from Dr. Mariano Semmola, where the loss of articulation was accompanied by convulsive movements, and instantly restored by bleeding.

The failure of coordination requisite to accomplish so complex a function may occur anywhere in the apparatus involved. Hence there are many forms of the affection, which may be roughly classified into four: (1) at the glottis, (2) at the isthmus of the fauces, (3) between the tongue and palate, (4) at the lips and posterior nares. The late Charles Kingsley, in his article quaintly named "The Irrationale of Speech," published in *Fraser's Magazine* for July, 1859, calls these four variations abuses of breath, jaw, tongue, and lips. But these by no means exhaust the catalogue of physical infirmities affecting speech, though being the most completely functional they fall strictly within the definition of stammering. Idiocy, deafness, and paralysis have been named, and to them may be added spasm, as in some cases of St. Vitus's dance. There are also several malformations and acquired disorders, such as (1) large or unsymmetrical tongue or tonsils, (2) cleft palate, (3) obstructed nasal passages, (4) high roofed mouth, (5) prominent and everted incisor teeth, which interfere with distinct articulation; besides the kindred bad habits called lisping, burring, and thickness of speech. Even then the list is not completed; for we have to add (1) a sort of hyperæsthesia or nervousness which occurs in some persons when they are out of health, and which disappears under better hygienic conditions; (2) tricks and bad habits, of which a flagrant example occurred some years ago, when a mania for transposition of words seized the younger and more thoughtless of the generation. A mutton chop, for instance, became a chutton mop, and one heard of the Chishop of Bicester, who had a sit of fickness through eating acon and beggs. In many cases the habit became uncontrollable, and is handed down to fame by the lady aunt of "Happy Thoughts," in *Punch*, who corrected errors of speech by reference to "Dixon's Johnsonary." (3) Mimicry, which produces a sort of contagiousness in mispronunciation. An instance of this occurred within the lecturer's experience at Marlborough School not long ago: one stammering member of a certain form having communicated his defect to several of his schoolfellows. (4.) Bad teaching, and inattention to faults in their nascent condition. Many mothers think fit to accommodate their speech to favourite children by mutilating and defacing it; keeping two vocabularies, one for the drawing-room, another for the nursery. This is a fatal source of imperfections, the more so as it is to be remarked that stammering never comes on till about the age of five years or more.

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[1] Abstract by the Author of three Lectures at the Royal Institution, by W. H. Stone M.B., F.R.C.P. Concluded from p. 533.

Lastly come peculiarities of an unconscious character akin to stammering—clucking, coughing, the reiterated interpolation of otiose syllables such as "er er," "ta ta"; even of definite words or sentences such as "you know," or the coarse expletive adjectives of habitual swearers. The lecturer cited a case within his own remembrance where an estimable clergyman had acquired the singular trick of unconsciously interlarding all his remarks with the involuntary phrase, "What a pity! what a pity!" in defiance of all sense and context.

Methods of cure were then adverted to. Probably no human infirmity had been the object of such diverse or such blundering and unscientific treatment. Even so good a surgeon as Diefenbach cut wedges out of the tongue of the patient; Itard made them speak holding a gold fork in their mouth; Serres advised a waving motion of the arms during speech; Bertrand caused them to regulate the words to a rhythmical motion of the fingers, or to keep time to a stick as in the orchestra. He also placed substances in the mouth. This had been done centuries before by Demosthenes, according to that unveracious gossip, Plutarch. These might be termed mechanical attempts at cure.

Next to them came musical methods, and foremost among them singing; it being well known that many confirmed stammerers sing with perfect articulation. Secondly, a so-called secret method, which consisted in either whispering or speaking in a gruff unmelodious tone. Thirdly, the very opposite of this as recommended by Marshall Hall, namely, chanting or monotoning. Fourthly, preceding all abrupt and consonantal sounds by a vowel such as E, recommended by Arnott. Fifthly, the plan of running all the words of a long sentence into one, and thus acquiring as it were an articulatory momentum.

Intellectual or rational methods brought the lecture to a close. First among these is pausing and deliberateness. The stammerer may be compared mechanically to a steamship which overruns her screw, and treated similarly. Secondly, the imitation of good models, by reading in unison with an articulate speaker. Thirdly, and perhaps best of all, prefacing every sentence by a deep breath, which relaxes all the muscles of speech, and enables them to start fairly one against another. Fourthly, a plan was suggested which had succeeded admirably in the lecturer's experience, namely, that of learning a new language. For this purpose none was better than French. Its pronunciation is so thoroughly different from that of English, that it requires and establishes a totally new coordination of muscles. Moreover its mode of habitual acquirement is entirely different from that of English. Any one who will watch a French child just rising out of infancy must notice that whereas the character of an English child's incipient speech is "smudging" and confusion, the other's is slow, pompous, and deliberate. It is not till later in life that the French acquire that lightning-like rapidity of speech which is the terror of foreigners; while young they speak well and slowly. The third lecture ended with a few directions how to proceed in a case of stammering, and some suggestions as to the prospects of cure. As to the former, it is obviously desirable to examine carefully for the exact seat and the exciting cause of the defect; most of the systems in vogue having erred by exaggerating a particular treatment to the exclusion of others equally admissible. As to the latter, there is no doubt that stammering can be cured. This was proved by such instances as Demosthenes, Wilberforce, and Kingsley. But it was equally proved by the three names thus enumerated that to conquer the vicious habit required no usual amount of patience, ability, and determination.

DISTRIBUTION OF ENERGY IN THE SPECTRUM

In the reaction against the arbitrariness of prismatic spectra there seems to be danger that the claim to ascendency of the so-called diffraction spectrum may be overrated. On this system the rays are spaced so that equal intervals correspond to equal differences of wave-length, and the arrangement possesses indisputably the advantage that it is independent of the properties of any kind of matter. This advantage, however, would not be lost, if instead of the simple wave-length we substituted any function thereof; and the question presents itself whether there is any reason for preferring one form of the function to another.

On behalf of the simple wave-length, it may be said that this is the quantity with which measurements by a grating are immediately concerned, and that a spectrum drawn upon this plan represents the results of experiment in the simplest and most direct manner. But it does not follow that this arrangement is the most instructive.

Some years ago Mr. Stoney proposed that spectra should be drawn so that equal intervals correspond to equal differences in the *frequency of vibration*. On the supposition that the velocity of light in vacuum is the same for all rays, this is equivalent to taking as abscissa the *reciprocal* of the wave-length instead of the wave-length itself. A spectrum drawn upon this plan has as much (if not more) claim to the title of *normal*, as the usual diffraction spectrum.

The choice that we make in this matter has an important influence upon the curve which represents the distribution of energy in the spectrum. In all cases the intensity of the radiation belonging to a given range of the spectrum is represented by the area included between the ordinates which correspond to the limiting rays, but the form of the curve depends upon what function of the ray we elect to take as abscissa. Thus in the ordinary prismatic spectrum of the sun, the curve culminates in the ultra-red, but in the diffraction spectrum the maximum is in the yellow, or even in the green, according to the recent important observations of Prof. Langley. If we wish to change the function of the ray represented by the abscissa, we can of course deduce by calculation the transformed curve of energy without fresh experiments. To pass from the curve with abscissæ proportional to wave-length to one with abscissæ proportional to reciprocals of wave-length, we must magnify the ordinates of the former in the ratio of the square of the wave-length, and this will give us an energy curve more like that obtained with a prismatic spectrum.

There is another method of representation intermediate between these two, which is not without advantage. In the diffraction spectrum the space devoted to a lower octave (if we may borrow the language of acoustics) is greater than that devoted to a higher octave. In Mr. Stoney's map the opposite is the case. If we take the *logarithm* of the wave-length (or of the frequency) as abscissa, we shall obtain a map in which every octave occupies the same space, and this perhaps gives a fairer representation than either of the others. To deduce the curve of energy from that appropriate to the diffraction spectrum, we should have to magnify the ordinates in the ratio of the first power of the wave-length.

My object, however, is not so much to advocate any particular method of representation, as to point out that the curve of energy of the diffraction spectrum has no special claim to the title of "normal."

RAYLEIGH

THE ORNITHOLOGIST IN SIBERIA[1]

THE ornithologists are certainly among the most enterprising of the seekers after truth. John Gould, the Birdman, is dead, but the same spirit which led him over the seas fifty years ago to investigate the then unknown Ornis of Australia still animates his brother birdmen. Mr. Henry Seebohm—a distinguished Member of the British Ornithologists' Union—has recently made two journeys into Northern Siberia, solely with the object of observing new forms and habits of bird-life and of collecting specimens. The scientific results of these expeditions have been published in the *Ibis*—the organ of the British Ornithologists' Union—which is now entering upon the twenty-fifth year of its existence, whilst a most interesting and attractive general narrative of the two journeys is given in the volumes now before us.

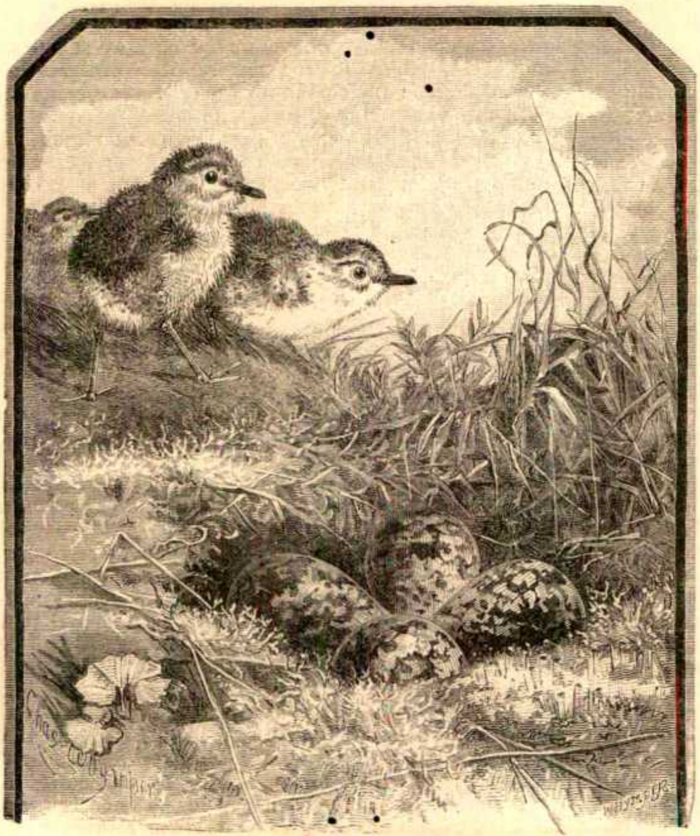

FIG. 1.—Grey Plover's nest and young.

The first of these two expeditions, to the lower valley of the Petchora, in North-Eastern Russia, was made by the author in 1875, in company with Mr. J. A. Harvie-Brown, a gentleman whose name is also known as that of an excellent field-naturalist. In order to be in time for the early spring migration, London was quitted on March 8, and the railway taken *viâ* St. Petersburg and Moscow to Vologda. Hence it was rather more than four days and nights continuous sledging to Archangel, which was reached on March 18 at noon. At Archangel, the last civilised city on the route, nineteen days were spent in completing preparations for the further journey and in collecting information of what was considered by the good citizens of that place to be a most formidable undertaking. From Archangel to Ust-Zylma, on the Petchora, a distance of from seven to eight hundred miles lay before the travellers, and as the frost showed some symptoms of breaking up, did not at first promise to be easily got over. Fortunately they were just in time. A fortnight later the thawing snow became impassable, the winter road was destroyed, and the valley of the Petchora became cut off from all communication with civilised Europe for two months! Ust-Zylma, a long, straggling village of wooden houses on the right bank of the Petchora, some 300 miles from its mouth, was the headquarters of the party until June 15. The waiting for the "coming of spring" was rather tedious. Their first week at Ust-Zylma was not very encouraging from an ornithological point of view. After eight days' work, the list of identified birds in the valley of the Petchora only amounted to nine species, mostly of the commonest description. Three weeks had passed, and the thaw still made no progress; the summer seemed as far off as ever. It was sometimes hot in the daytime, but always froze again at night. On April 28 the first bird's-nest was taken (that of the Siberian Jay), but snow-shoes were still required to get about. It was not until May 10, in fact, that any real summer weather came, and it thawed in the shade as well as in the sun; but two days later it actually rained. The migrants then arrived in quick succession: swallows, swans, geese, gulls, wagtails, redstarts, pipits, and shorelarks, all were hurrying up from the south along with the first blush of spring. On May 20, while the party were on a collecting expedition on the opposite bank of the Petchora, which they had crossed as usual on sledges, the grand crash came. The ice which had so long covered the river began to break up with a noise as of rumbling thunder, and cracks ran along it at the rate of a hundred miles in twenty-four hours. It was with great difficulty that the retreat was effected, and a few hours after home was reached the mighty river was in full flood, carrying its burden of pack-ice and ice-floes to the sea at the rate of six miles an hour. In a week's time the Petchora was entirely free from ice, and summer was upon them.

Collecting now began in earnest, and every day added to the number of interesting birds, and increased the variety of nests and eggs. On June 8, 143 eggs were taken and "blown" in the course of the day.

On June 10 the journey down the Petchora was commenced in a large, partly-covered boat hired for the purpose, so that the naturalists might stop when they pleased for the purpose of collecting. The voyage was delightful. Everywhere the Blue-throat, the Redwing, the Brambling, the Fieldfare, the Little Bunting, and the Willow-warbler were common, whilst Three-toed Woodpeckers, Terek Sandpipers, and other rarities were making their nests and laying their eggs for the benefit of the travellers. Here one of the great discoveries of the expedition was made, which cannot be described better than in Mr. Seebohm's own words:—

"We were now a little to the north of the Arctic circle, and at three in the morning moored our boat on the shores of an island, among whose willows grew an occasional birch or alder. I spent five hours upon it. Sedge-warblers were singing lustily, and sometimes so melodiously that we almost took them to be Blue-throats. Soon, however, my attention was arrested by a song with which I was not familiar. It came from a bird singing high in the air, like a lark. I spent an hour watching it. Once it remained up in the sky nearly half an hour. The first part of the song was like the trill of a Temmick's stint, or like the concluding notes of the Wood-warbler's song. This was succeeded by a low guttural warble resembling that which the Blue-throat sometimes makes. The bird sang while hovering; it afterwards alighted on a tree, and then descended to the ground, still continuing to sing. I shot one, and my companion an hour after shot another. Both birds proved to be males, and quite distinct from any species with which either of us was previously acquainted. The long hind-claw was like that of the Meadow-pipit, and the general character of the bird resembled a large and brilliantly-coloured Tree-pipit. It was very aquatic in its habits, frequenting the most marshy ground amongst the willows.

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[1] "Siberia in Europe: a Visit to the Valley of the Petchora, in North-East Russia; with Descriptions of the Natural History, Migration of Birds, &c." By Henry Seebohm, F.L.S., F.Z.S. 8vo. (London: Murray, 1880.)
"Siberia in Asia: a Visit to the Valley of the Yenesay, in East Siberia; with Descriptions of the Natural History, Migration of Birds, &c." By Henry Seebohm. 8vo. (London: Murray, 1882.)

"On our return home five skins of this bird were submitted to our friend Mr. Dresser, who pronounced it to be of a new species, and described and figured it in a work which he was then publishing on 'The Birds of Europe.' In honour of my having been the first to discover it, he named it after me, *Anthus Seebohmi*. But, alas for the vanity of human wishes! I afterwards discovered that the bird was not new, but had been described some years before from examples obtained on the coast of China. I had subsequently the pleasure of working out its geographical distribution. The honour of having added a new bird to the European list still remains to us, and is one of the discoveries made upon our journey on which we pride ourselves."

Ten days' voyage down the river occupied in this fashion brought the travellers in their boat to Alexievka, the shipping port of the Petchora, where the larch-timber felled on its banks is laden for Cronstadt and other ports. Here their headquarters were fixed until their departure for England on August 1. But the forty days passed here were by no means wasted. The "tundra" on the east bank of the great river, frozen hard and under snow during eight months in the year, becomes in summer a boggy moor covered with carices, mosses, and dwarf shrubs, and varied by abundance of lakes. Untrodden by ordinary man, it was splendid birds'-nesting ground for the ornithologists, who reaped there an abundant harvest. We cannot go separately into the discoveries here made, which are related by Mr. Seebohm in his usual sprightly and energetic style, but they are thus summed up at the conclusion of his volume:—

"Of the half-dozen British birds, the discovery of whose breeding-grounds had baffled the efforts of our ornithologists for so long, we succeeded in bringing home identified eggs of three—the grey plover (Fig. 1), the little stint (Fig. 2), and Bewick's swan. Of the remaining three, two, the sanderling and the knot, were found breeding by Capt. Fielden, in lat. 82°, during the Nares' Arctic expedition, but the breeding-grounds of the curlew sandpiper still remain a mystery. We added several birds to the European list, which had either never been found in Europe before, or only doubtfully so: such as the Siberian chiff-chaff, the Petchora pipit, the Siberian herring-gull, the Arctic forms of the marsh-tit, and the lesser-spotted woodpecker; the yellow-headed wagtail, and the Asiatic stonechat. We brought home careful records of the dates of arrival of the migratory birds which breed in these northern latitudes, besides numerous observations on the habits of little-known birds.

"Our list of skins brought home exceeded 1000, and the eggs were rather more than 600 in number."

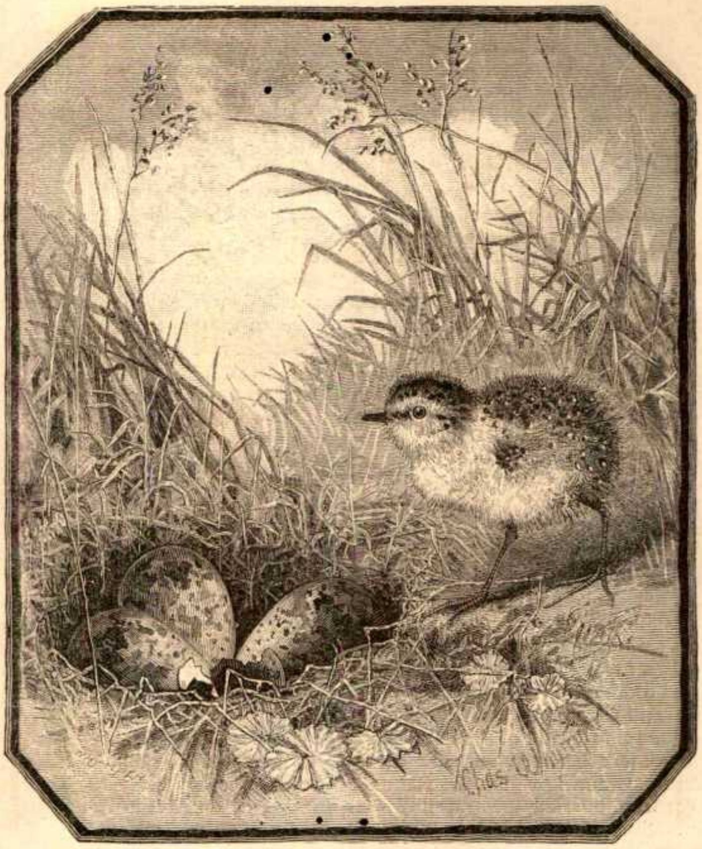

FIG. 2.—Little Stint's nest, eggs, and young.

The success of the Petchora expedition induced Mr. Seebohm to wish to extend his field of operations into districts yet further east, when it might be expected that some of the few remaining British birds, of which the breeding-haunts were still unknown, would be found nesting. The remotest eastern corner of Europe having been worked out, it was necessary to push on into Asia, and in 1877 an excellent opportunity of doing this presented itself Capt. Wiggins, of Sunderland, one of the pioneers of the recent attempts to reopen sea-communication with Northern Siberia, had succeeded in penetrating some 1200 miles up the Yenesay (Mr. Seebohm's phonetic spelling of Yenisei) in the previous autumn, and having left his vessel there to winter, and returned home overland, was preparing in February of that year to go back to the Yenesay. At a few days' notice Mr. Seebohm undertook to join him in his journey out, wisely thinking that in such an expedition it was as well to have the company of a gentleman who "knew the ropes," although he might have little sympathy with ornithological pursuits.

Mr. Seehohm and Capt. Wiggins accordingly left London on March 1, and travelled by rail to Nishni Novgorod, a distance of some 2400 miles. Thence was a sledge-journey of about 3200 miles to the winter quarters of the good ship *Thames*, on the Yenesay, or rather a little way up the Koorayika, an affluent of the Yenesay, on its right bank. The crew of the *Thames* who had passed a long and dreary winter, frozen up at this spot, were found on the travellers' arrival to be well and hearty, owing to the judicious precautions that had been taken by their Captain for the benefit of their health.

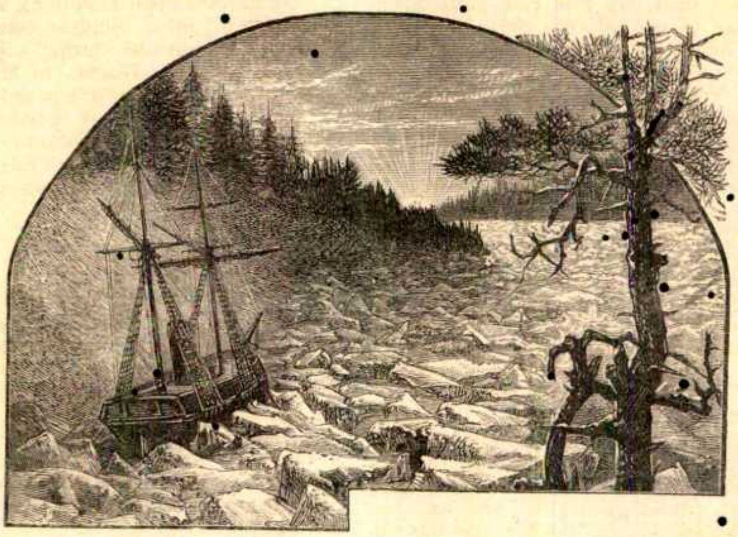
FIG. 3.—Driving with the ice on the Koorayika.

On April 23, when the travellers reached the ship, there were no signs of approaching summer on the Yenesay. On the frozen river the snow lay six feet deep, and was little less in the surrounding forests. Mr. Seebohm put on his snow-shoes and had a round with his gun. Birds were more plentiful than could have been expected. A pair of ravens were generally in sight, and flocks of snow-buntings flitted by. Nutcrackers came to the doors of the sailors' room, to pick up the cook's refuse, and Lapp-tits and Pine-grosbeaks were common in the woods. The excursions into the forest were continued every day, and a few additional birds observed, but on May 1 the list of identified species was only twelve in number, and summer seemed nowise nearer. It was not until May 15 that indications of a thaw appeared, and geese were seen travelling north, but the next day was as cold as ever. After that date, however, some slight progress was made: the water in the Koorayika began to rise, and the summer migrants appeared one by one.

The great battle of the Yenesay, as Mr. Seebohm calls the contest between summer and winter, lasted about a fortnight, during which thousands of acres of ice on the river were hurried up and down as the water rose and fell. Sometimes the floes were jammed so tightly together that it looked as though one might cross the river on them, at other times there was open water interspersed only with stray icebergs. At last the final "march-past" of the ice took place; "winter was vanquished for the year," and succeeded in a few days by "the triumphant music of thousands of song-birds, the waving of green boughs, and the illumination of gay flowers of every hue."

It was not until June 26 that the *Thames* was able to steam away down the river. By this date Mr. Seebohm and his collectors had made large collections of birds and eggs, and having exhausted the novelties of the surrounding district, were heartily glad to be off northwards to fresh fields of research. Unfortunately, after about a week's navigation, the *Thames* grounded on a shoal, and, as the water was falling rapidly, could not, in spite of every effort, be got off again. All that could be done was to move what was necessary into the *Ibis*—a small vessel built on the river—and to continue the voyage down the Yenesay, leaving the *Thames* to her fate.

On nearing the embrochure of the Yenesay, on July 12, a gale compelled the *Ibis* to cast anchor, and advantage was taken of the delay to explore the adjacent "tundra"—"a wild-looking country full of lakes, swamps, and rivers, a dead flat in some places, in others undulating, even hilly—brilliant with wild flowers, swarming with mosquitoes,

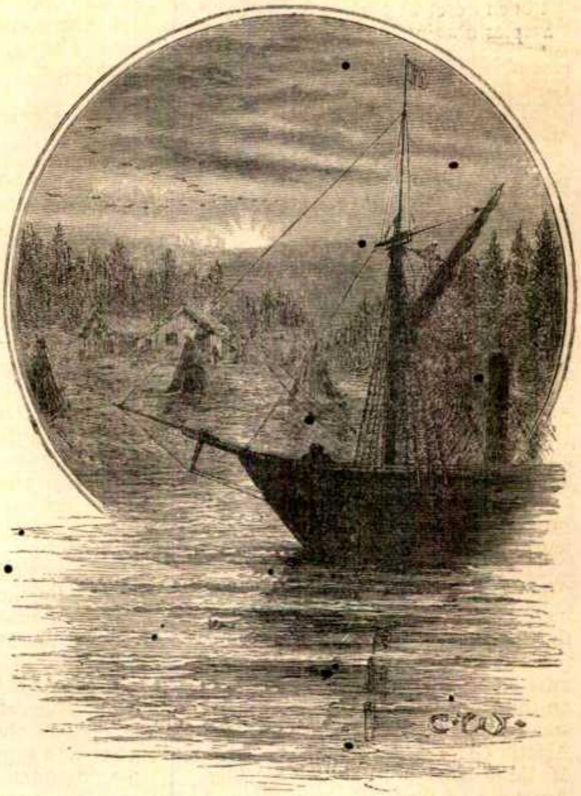
FIG. 4.—Summer quarters on the Koorayika.

and full of birds." Here one of the great discoveries of the second expedition was made, which is described by the author in his usual lively manner:—

"The gale continued next day with rain, until noon, when I took advantage of our enforced delay, and went on shore for a few hours. A climb of about 100 feet brought me on to the tundra. In some places the cliffs were very steep, and were naked mud or clay. In others the slope was more gradual, and covered with willow and alder bushes. In these trees Thrushes were breeding. I soon found the nest of a Dusky Ouzel, with five nearly fledged young. It was placed as before in the fork of a willow, level with the ground. On the top of the bank I found myself on the real Tundra. Not a trace of a pine tree was visible, and the birch trees rarely exceeded twelve inches in height. There was less grass, more moss and lichen, and the ground was covered with patches of yellow mud or clay, in which were a few small stones, that were apparently too barren for even moss or lichen to grow upon. The Tundra was hilly, with lakes, swamps, and bogs in the wide valleys and plains.

"As soon as I reached the flat bogs I heard the plaintive cry of a Plover, and presently caught sight of two birds. The male was very conspicuous, but all my attempts to follow the female with my glass, in order to trace her to the nest, proved ineffectual, she was too nearly the colour of the ground, and the herbage was too high. Feeling convinced that I was within thirty paces of the nest, I shot the male, and commenced a diligent search. The bird proved to be the Asiatic Golden Plover, with gray axillaries, and I determined to devote at least an hour looking for the nest. By a wonderful piece of good fortune I found it, with four eggs, in less than five minutes. It was merely a hollow in the ground upon a piece of turfy land, overgrown with moss and lichen, and was lined with broken stalks of reindeer moss. The eggs resembled more those of the Golden than those of the Grey Plover, but were smaller than either.

"These are the only authenticated eggs of this species known in collections."

Golcheeka, the port at the mouth of the Yenesay, was reached on July 18. As Mr. Seebohm did not think it prudent to attempt the sea-passage home in the little *Ibis*, and the last steamer of the season up the Yenesay was to leave six days afterwards, little could be done in this locality. But excursions were made over the adjoining tundra, where "birds were abundant." "Golden Plovers, Arctic Terns, Ruffs, Red-necked Phalaropes, Snow-buntings, Lapland Buntings, and Dunlins were continually in sight, and the Asiatic Golden Plover was breeding in numbers, though attempts to watch them on to their nests were made in vain." On July 24 Mr. Seebohm finally turned his face homewards, and reached Yenesaisk on August 14, after twenty-two days on the road, which was considered "a good passage." Thence post-horses, steamers, and railways brought him back to Sheffield on October 15, after a journey of some 15,000 miles.

The ornithological results of the second journey were "on the whole satisfactory." It was a great disappointment not to get to the coast, and still more so to miss the birds of the Kara Sea, and to arrive on the tundra too late for most of the eggs specially sought for. This misfortune was caused by the wreck of the *Thames*. But on the other hand "the delay in the pine-forests produced some very interesting results." Besides the eggs of the Asiatic Golden Plover already spoken of, nests and eggs of three species of Willow-warblers, of the Mountain-Accentor, of the Little Bunting, and of the Red-breasted Goose were obtained. All these were previously unknown to western collectors, and were for the most part never previously obtained. Besides this, a large number of other rare birds were found nesting, their eggs and young plumages obtained, and their habits and manners studied and recorded. Concerning particulars of their discoveries, and for much information on the native tribes of Northern Siberia (a subject to which our author appears to have devoted great attention), as likewise for observations on every other incident coming before the eyes of an intelligent traveller during a journey of 15,000 miles, we must refer our readers to Mr. Seebohm's volumes, which are full of interest not only to ornithologists, but to those who take pleasure in natural history in its widest extent. They may be placed on our shelves next to Bates's "Amazons" and Wallace's "Eastern Archipelago," and form no unworthy companions to the works of those great naturalists.

THE BACILLUS OF TUBERCLE

MR. WATSON CHEYNE'S Report on the Relation of Micro-organisms to Tuberculosis, published in the *Practitioner* for the present month, is one of the fruits of the Association for the Advancement of Medicine by Research, recently constituted for the protection of working physiologists and pathologists. On commission from the Association, Mr. Cheyne visited two of the chief workers on this subject, Toussaint and Koch. He was thus able to see their methods and obtained materials from them with which he has experimented on his return to England.

After some remarks on the method of staining the tubercle bacillus, Mr. Cheyne describes some experiments made with the view of testing the theory that tuberculosis in rodents can be induced by almost any irritant. The result of these experiments, made on a considerable number of animals, was to disprove this theory and to lead to the conclusion that in the former experiments, made before our present knowledge as to the precautions necessary for disinfection of instruments, &c., was gained, the channels for the introduction of specific micro-organisms had been left unguarded.

Experiments were next made to test Toussaint's statement that micrococci can be cultivated from the blood of tuberculous animals, and that the injection of these micrococci into other animals is often followed by tuberculosis. Mr. Cheyne failed to cultivate micrococci from the blood of tuberculous animals; he injected micrococci which M. Toussaint had liberally placed at his disposal, into a considerable number of animals without result, and he found tubercle-bacilli but no micrococci in the organs of several animals which had been injected by Toussaint himself with micrococcal fluid, and had become tuberculous. He therefore concludes that Toussaint's micrococci do not cause tuberculosis, and that an error has crept into his experiments probably because the means used to disinfect his syringes, although amply sufficient to destroy some other kinds of bacilli, did not destroy the tubercle-bacilli.

Cultivations of bacilli were also obtained from Dr. Koch, and the results of their inoculation was in all cases rapid development of tuberculosis. The examination of a large quantity of tuberculous material showed the constant presence of tubercle-bacilli, but of no other micro-organisms. The rapidity and certainty of action of tuberculous material when inoculated into animals was in direct ratio to the number of bacilli introduced, and the most certain and rapid means of inducing tuberculosis in animals is the inoculation of the tubercle-bacillus cultivated on solidified blood-serum. These facts lead Mr. Cheyne to the conclusion that we have before us in these bacilli the virus of the acute tuberculosis caused in animals by the inoculation of tuberculous material.

Pursuing the inquiry from this point, to which it had been brought by the researches of Koch, Mr. Cheyne proceeds to discuss the relation of these bacilli to tuberculous processes in man and to tubercle generally. In all tubercles there are present epithelioid cells, to which, however, only a few authors have attached any importance. On investigation Mr. Cheyne found that the tubercle-bacilli were, unless when present in large numbers, only found in or among these epithelioid cells, and that the tuberculous nodules first begin by the entrance of bacilli into these cells and the subsequent development of the epithelioid elements. Surrounding these epithelioid cells a slight amount of inflammation occurs, giving rise to the small-celled growth around the tubercle, which is generally regarded as the growing part of the tubercle. This Mr. Cheyne denies, asserting that it is merely inflammatory tissue, and that the essential elements of the tubercle are the epithelioid elements in its centre. In the lungs these cells seem to be derived from the alveolar epithelium, in the liver often apparently from the liver cells, but in other organs and also sometimes in these from the endothelium of the lymphatics and blood-vessels.

In phthisis the bacilli were found at the margin of cavities and in the epithelioid cells surrounding the cheesy matter. Mr. Cheyne concludes that in phthisis the bacilli, inhaled into the alveoli, develop in the alveolar epithelium, cause accumulation of epithelial cells in the alveolus, and inflammatory hypertrophy of its walls. Thus the bacilli are practically shut off from the circulation and acute general tuberculosis cannot occur. The two extremes of phthisis are considered—the very rapid form or caseous pneumonia, and the slow form or fibroid phthisis. In the former the bacilli grow rapidly, are fairly numerous, and the lung rapidly breaks down; in the latter the bacilli grow slowly and with difficulty, and hence extensive fibrous formation occurs.

There are many other points of interest in this research to which we cannot allude, but which will be found at length in the Report. The Association is to be congratulated on having chosen such a fertile subject for their first report, and we hope that they will continue to encourage similar work.

PROFESSOR H. J. S. SMITH AND THE REPRESENTATION OF A NUMBER AS A SUM OF SQUARES

THE award of the great Mathematical Prize of the French Academy to the late Prof. H. J. S. Smith may have the effect of drawing the attention of mathematicians to the wonderful extent and value of his researches on the Theory of Numbers. Probably no more important or remarkable mathematical investigations have ever appeared in this country than his memoirs on systems of linear indeterminate equations and congruences and on the orders and genera of ternary quadratic forms and of quadratic forms containing more than three indeterminates, which were published in the *Philosophical Transactions* for 1861 and 1867 and the *Proceedings of the Royal Society* for 1864 and 1867. The results contained in these papers are, by far the greatest additions that have been made to the Theory of Numbers since it was placed on its present foundation by Gauss in the "Disquisitiones Arithmeticæ." The subject for which the prize was awarded to Prof. Smith was that of the theory of the representation of a number as a sum of five squares, and of this question as well as that of the corresponding one for seven squares he had given the complete solution in the *Proceedings of the Royal Society* for 1867 (vol. xvi. p. 207). The words with which Prof. Smith introduced his statement of the solution of these important questions are as follows:—

"The theorems which have been given by Jacobi, Eisenstein, and recently in great profusion by M. Liouville, relating to the representation of numbers by four squares and other simple quadratic forms, appear to be deducible by a uniform method from the principles indicated in this paper. So also are the theorems relating to the representation of numbers by six and eight squares, which are implicitly contained in the developments given by Jacobi in the 'Fundamenta Nova.' As the series of theorems relating to the representation of numbers by sums of squares ceases, for the reason assigned by Eisenstein, when the number of squares surpasses eight, it is of some importance to complete it The only cases which have not been fully considered are those of five and seven squares. The principal theorems relating to the case of five squares have indeed been given by Eisenstein (*Crelle's Journal*, vol. xxxv. p. 368); but he has considered only those numbers which are not divisible by any square. We shall here complete his enunciation of those theorems, and shall add the corresponding theorems for the case of seven squares."

In the announcement of the subject for the prize in the *Comptes Rendus* in February of last year, reference was made to the work of Eisenstein, but the fact that his solution had fifteen years before been completed by Prof. Smith—who had also solved the problem in the case of seven squares, the whole being only a corollary from the general principles contained in his memoirs—seems to have escaped the attention of the proposers of the subject. In the paper in the *Proceedings of the Royal Society* the results only for the case of five squares and seven squares are given, the demonstrations being omitted; and accordingly, when the subject for the prize was announced, Prof. Smith followed the only course open to him, and communicated to the Academy his demonstrations for the case of five squares.

All who knew Prof. Smith will understand how uncongenial to him was the idea of becoming a competitor for the prize, but under the circumstances he had no choice. It is a singular tribute to Prof. Smith's mathematical powers, as well as a curious episode in the history of mathematics, that the French Academy should have chosen as the subject of the "Grand Prix"—thereby indicating their opinion of its importance in the advance of the science[1]—a question that had been solved already fifteen years before as a corollary from more general principles

The state of the question of the number of ways in which a number can be expressed as a sum of squares therefore stands as follows:—For two squares the solution was given by Gauss in the "Disquisitiones"; the cases of four, six, and eight squares are due to Jacobi, Eisenstein, and Prof. Smith (see *Report of the British Association for* 1865, p. 366). In these cases in which the number of squares is even, the problem can be solved by means of elliptic functions, and it is not necessary to have recourse to the special methods of the Theory of Numbers; but it is not so in the case when the number of squares is uneven, and the question is then essentially "arithmetical" as regards its method of treatment and expression. The case of three squares was given by Lejeune-Dirichlet, and is included in Prof. Smith's general treatment of ternary quadratic forms in the *Philosophical Transactions* for 1867: the enunciations for the cases of five squares and seven squares were given, as has been stated, in the *Proceedings of the Royal Society for* 1867. The demonstrations for the case of five squares have been communicated to the French Academy, but those for seven squares still remain unpublished in Prof. Smith's note-book. This class of questions ceases to admit of the same kind of solution when the number of squares exceeds eight, so that with the publication of the demonstrations for seven squares the solution of the whole problem will be complete. It will be seen that Prof. Smith has had a large share in this great mathematical victory.

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[1] "L'Académie était donc fondée à espérer que ce voyage de découvertes imposé aux concurrents à travers une des régions les plus intéressantes et les moins explorées de l'arithmétique produirait des résultats féconds pour la science Cette attente n'a pas été trompée" Report on the award of the prize, *Comptes Rendus*, April 2, 1883 In this report however no mention is made of the fact that these "résultats féconds" had been published in 1867

NOTES

THE Queen has signified her intention of conferring the honour of Knighthood upon Prof. Frederick Augustus Abel, C.B., F.R.S., in recognition of the valuable services rendered by him to the War Department and to other departments of the Government in his capacity of War Department Chemist.

HER MAJESTY has also been pleased to confer the honour of Knight Commandership of the Bath on the Right Hon. Lyon Playfair, C.B., M.P., F.R.S.

WE are glad to learn that the Hong Kong Observatory scheme, to which we have frequently adverted, has at last become so far a *fait accompli* that Dr. Doberck of the Dunsink Observatory has been appointed astronomer to the new institution by the Secretary for the Colonies. The opportunities afforded for independent and original work in Hong Kong are very great, and we are sure the head of the new Observatory will make the most of them. Dr. Doberck is at present attached to the Kew Observatory, and expects to leave England with his first assistant early in June. Lord Derby is taking a marked interest in the new Observatory, and we are glad to learn is making Dr. Doberck a very liberal allowance for the purchase of instruments.

THE Davis Lectures for 1883 will be given in the lecture room in the Zoological Society's Gardens, in the Regent's Park, on Thursdays, at 5 p.m., commencing June 7, as follows:—June 7, Ungulate Mammals, by Prof. Flower, LL.D., F.R.S.; June 14, Our Snakes and Lizards, by Prof. Mivart, F.R.S.; June 21, The Lamprey and its Kindred, by Prof. Parker, F.R.S.; June 28, Birds and Lighthouses, by J. E. Harting; July 5, The Niger and its Animals, by W. A. Forbes; July 12, South American Birds, by P. L. Sclater, F.R.S.; July 19, The Siberian Tundra, by Henry Seebohm. These lectures will be free to Fellows of the Society and their friends, and to other visitors to the Gardens.

OUR readers will doubtless be surprised to learn that the masterly address on Darwin and Copernicus, of which we publish a translation in another column, has called forth much hostile criticism in Germany. It was read before the members of the Berlin Academy of Sciences, of which Prof. Du Bois Reymond is Secretary, at their last annual meeting. Shortly afterwards one of the Clerico-Conservative newspapers of the German capital called attention to what it was pleased to call the public laudation of one of the worst and most dangerous atheists by a member of a public body supported by the State. Many other papers of the same views immediately followed suit; while the notorious Court Chaplain, Stöcker, whose exploits as a Jew-baiter furnished the Berlin correspondents of the daily papers with a good deal of matter about twelve months ago, preached a long sermon against Prof. Du Bois Reymond and his views. His example was followed by other members of the so-called "Orthodox" clergy in Berlin and the provinces. But the Court Chaplain is also a member of the Prussian Parliament; so not content with crushing "atheism" from the pulpit, he put a question in the House on the subject, supported by Herr Windthorst, one of the leaders of the Ultramontane party. They were answered by Prof. Virchow and the Prussian Minister of Public Instruction, thus causing a whole sitting of the Prussian Landtag to be taken up by a debate on the graceful tribute to the memory of Darwin. That such things should take place in Germany, which has always been considered the home of philosophic freedom, really seems to justify the remark of the author of "Darwin and Copernicus," that freedom of thought, which, after taking its rise in England in the middle of the eighteenth century, passed through France to Germany, where it attained a fuller and more systematic development, seems now to be passing away from the latter country again! Let us hope that it is coming to our shores once more, as the Professor says it is.

THE Swedish subscription to the Darwin Memorial is now closed. The number of subscribers is 2294, and the amount subscribed 400*l*.

THE *Times* Paris correspondent telegraphs as follows under date April 10:—A shameful trick has been played on the Academy of Sciences. The Königsberg student, Hermann Minkowsky, who with the late Prof. Henry J. S. Smith was declared to have gained the great mathematical prize of 3000 francs, had simply pirated Prof. Smith's communication to the Royal Society in 1868, on the representation of a number as the sum of five squares. He had even copied a slight error in it. The Academy, therefore, at a secret session yesterday annulled its original decision and declared that the whole prize had been gained by the distinguished English professor, who unfortunately has not lived long enough to expose the hoax.

WE would again draw the attention of local scientific societies to the circular which has been issued by the Committee of the British Association appointed to consider certain matters in connection with such societies. These societies will be doing themselves as well as the Committee service by forwarding the information desired without further delay.

THE Scotch Universities Bill, which has been introduced by the Lord Advocate, establishes an Executive Commission, and gives them extensive powers for reorganising the Universities, including the power of revising existing foundations and endowments, and of founding new Professorships. They will also have authority to affiliate Colleges in other parts of the country with the University of St. Andrews; and, if satisfied that that University is no longer able to perform its functions, to dissolve it, and create a new Corporation. The Bill also proposes that a grant of forty thousand pounds a year shall be given to the Scotch Universities from the Consolidated Fund.

THE committee for the organisation of the Congress of Orientalists in Holland has issued a circular letter explaining the reasons for the alteration of the time of meeting of the Congress at Leyden from 1884 to the present year. The last congress, which met at Berlin in 1881, decided that the next should take place at Leyden in 1884; but, the committee say, since then, as it has been arranged that an international colonial exhibition was to be held in Amsterdam this year, it was thought better, after consultation with the previous committee, and after having obtained the sanction of the Netherlands Government, to hold the Oriental Congress at the same time. It is accordingly notified that the Congress will assemble at Leyden from September 10 to 15 of the present year. A small exhibition of literary curiosities, manuscripts, rare books, &c., will be held at the same time. Oriental scholars desirous of being present, or of reading papers, are invited to communicate with Mr. W. Pleyte of Leyden before the end of July, in order that the necessary accommodation may be prepared.

THE *Japan Mail* in announcing recently the death of a student of the Imperial College of Engineering, Mr. Yamada, from overstudy, refers to his docility, untiring assiduity, and very remarkable ability. The writer, who appears to possess intimate knowledge of the subject, speaks thus of Japanese students in general:—"It is hard for those to think ill of Japan who have watched these gentle, earnest-hearted lads, set themselves, almost before they have ceased to be children, with unflagging resolution to accomplish the task their fathers bequeathed to them unattempted, the task of winning for their country the place they hope to see her one day occupy. 'Very fine, forsooth!' we can hear your professional maligner exclaim, 'but after all what have they done?' Ay, indeed, what have they done! Doubtless they never ask themselves that question. Doubtless they never have to struggle against the paralysing consciousness that the most they can hope to do is to lay a foundation for others to build on, to play the brave part of those silent workers who sow that their successors may reap. That is not much, to be sure, so far at least as visible results are concerned, but it is a work incomparably higher than anything within reach of those cowardly cynics who toil for nothing but to make the world forget that the noblest of English attributes is generosity."

Dr. G. W. Leitner, the explorer and orientalist, is now on his way to England.

A company has been formed for the construction and working of an electric railway from Charing Cross to Waterloo, a Bill for which was recently obtained. The line will pass under the Thames through iron caissons. The work of construction will commence near the northern end of Northumberland Avenue, opposite the Grand Hotel, and be continued through an arch under that avenue and the Victoria Embankment. Of that arch sixty feet under the Embankment have already been constructed. The railway will pass under the Thames, and again through an arch under College Street and Vine Street, and terminate at Waterloo Station, where it will be directly connected with the platforms of the London and South-Western Railway, with a separate approach from the York Road. The line will be double, and worked by means of a stationary engine at Waterloo, transmitting the power to the carriages, which will run separately, start as filled, and occupy about three and a half minutes in the journey. A tender has been accepted for the construction of the railway, to be ready for opening within eighteen months from the commencement of the work. A contract has also been made with Messrs. Siemens Bros. and Company to provide and erect all requisite electrical machinery, rolling stock, and apparatus not included in the before-mentioned tender.

In connection with the meeting of the Civil Engineers on Saturday the *Times* makes some very definite statements on the position and function of science in our time, which are worth placing on record as the deliberate opinion of a leading organ of public opinion:—"Meetings such as that of Saturday evening remind us not merely of the services of a particular branch of science to mankind, but of the remarkable determination of human activity to scientific pursuits which is characteristic of the present age. Literature no longer holds the place it once did in the minds of men; nor does it command, as it once did, the services of the most powerful intelligences. The protest against an education wholly or chiefly consisting of the study of the classics is the result of a profound change in the conditions of life. Men have not deliberately and as a result of abstract reasoning discarded one set of studies in favour of another. On the contrary they have discovered, often to their great chagrin, that a complete intellectual displacement has taken place. That which was taken up under protest as a thing too closely connected with utilitarian pursuits to be quite worthy of a man of intellect has now pressed into its service the chief intellectual power of the country. The tide of intellectual effort sets strongly in the direction of science, just as at an earlier period it set in the direction of letters. The teachers and leaders of the day, the real dominant forces of the age, are the men of science, the investigators of natural phenomena, not the thinkers, philosophers, or metaphysicians who formerly gave their names to sects, and made all the world their partisans. Nothing is more remarkable than the profound respect of the scientific conception associated with the name of Darwin, not on science only, but on literature, art, morals, and, in short, upon life. Some will tell us that all this is a lamentable result of the materialism of the age, but we naturally ask how it happens that some centuries of a non-scientific or literary culture left us a prey to the materialism it is supposed to antidote? It is untrue, moreover, that material interest has been the great impelling force. The great discoveries of science have usually been made by men seeking no material reward, and, as a matter of fact, receiving very little. Science pursues her own way for the most part, and her discoveries are afterwards utilised by men eagerly seeking for the means of material enrichment. Even when it is a question of so practical a thing as a new dye, it will be found that the chemist searching into the properties and combinations of matter, comes upon the secret unawares, while the manufacturer and the dyer reap the profits. It is indeed, only upon these terms that nature yields up her secrets."

The death is announced at Basle of Dr. Ziegler, who has been long and honourably known for his numerous and remarkable works in cartography. Born at Winterthur in 1801, he began his studies under the direction of Carl Ritter, the creator of modern geography. At a later period of his life he established in his native town the cartographic establishment which is now conducted by Messrs. Wurster and Randegger. From Winterthur he proceeded to Basle, and a few years ago, in testimony of his gratitude for the kindness with which he was received there, he presented to the city of his adoption his magnificent collection of ancient and modern maps. For the conservation and augmentation of this collection a special society has been formed. Dr. Ziegler's most important works are his great map of Switzerland, maps of Glarus, of St. Gall, and of the Engadine, and a hypsometric map of the world. His last work, completed shortly before his death, and now in the press, was a geological atlas and an explanatory description of the geological map of Switzerland.

Under the title of "Cacao: How to Grow and how to Cure it," Mr. D. Morris, the Director of the Public Gardens and Plantations in Jamaica, has issued a pamphlet of some 45 pages. It is divided into chapters, the first of which is of an introductory character, and treats of the character of climate and soil of Jamaica, the abolition of slavery and its consequent effects upon the cultivation of the sugar-cane, and the necessity at the present time to plant new economic plants, and a consideration of the prospects of cocoa planting. On this point Mr. Morris says: "I am glad to say that the largest number of the best Trinidad varieties distributed from the Public Gardens during the last five or six years have been intelligently and carefully cultivated on portions of sugar estates which, although unsuitable for canes, are admirably adapted for cacao." Mr. Morris's remaining chapters are devoted to the following considerations: Historical description; cultivation of cocoa; how to start a cacao plantation; planting, pruning, gathering, sweating, curing; yield of cocoa-trees; cost of establishing estates, &c. Under these several heads much interesting and useful information is given, as, for instance, on the original home of the cacao plant, the introduction of cacao or chocolate into England, its consumption in Europe and Great Britain. As a guide to planters or those intending to introduce cacao as a crop, the succeeding chapters will be of much value. The little book is both readable and useful, and can be obtained in this country of Messrs. S. W. Silver and Co.

Although the Chinese Educational Mission has been recalled from the United States before its work was done, through some fancy, we believe, that the young men composing it were becoming too republican in their ideas, yet the results have been in many respects gratifying to those who desire to see Western knowledge spread in China. The youths have been drafted to telegraph stations, arsenals, and elsewhere, and we observe that the secretary and interpreter, Mr. Kwong ki Chin, who recently published a bulky volume of English phrases, is now preparing a series of schoolbooks for use in Chinese government schools. An English reading-book for beginners, an elementary geography, a series of conversation books, and a manual of English correspondence have either been already published, or will shortly appear. Among many other indications of the steady, though slow, advance of the Chinese in this direction, the Peking correspondent of the *North China Herald* refers with regret to the retirement from business of Mr. Yang, a well-known pawnbroker of the metropolis. In addition to the ordinary duties of his calling this individual appears to have studied chemistry, mechanical science, French, mineralogy, medicine, and other subjects of a similar kind. He owned gasworks, steam-engines, a complete pharmacopeia of drugs, photographic apparatus, and a geological cabinet. It is to be hoped that Mr. Yang has prospered in his business, because he has retired to his native province, Shansi, where he intends prosecuting enterprises for coal and iron mining, and other appliances of foreign machinery. When tastes of this kind extend to the shrewd and enterprising Chinese traders, we need not despair of the outlook for science in China.

SOME time since we alluded to the work done in China by an American female physician, Miss Dr. Howard. She has attended the mother of Li Hung Chang, the great Viceroy, and now we read she is treating the wife of the same high official. The fame of the lady doctor appears to have spread far and wide over North China, and she is now flooded with applications for assistance and advice from the women of wealthy families, who would die rather than be treated by a foreign male physician. It looks as if the various countries of the East offered an almost inexhaustible field for women possessing medical knowledge and skill.

THE Annual Report of the Glasgow Museum is as favourable as can be expected, considering the totally inadequate space allotted for the purpose in one of the wealthiest cities of the world.

PROF. H. CARRINGTON BOLTON has issued in a separate form his address on Chemical Literature, delivered before the American Association at Montreal last year.

FOR Baron Nordenskjöld's coming expedition to Greenland a flying-machine is now being constructed in Gothenburg. The apparatus, a kind of flying or air-sailing machine, is the invention of a Swedish engineer, Herr A. Montén, who is now constructing the same at the expense of Dr. Oscar Dickson.

ON the night of April 3, frequent and violent shocks of earthquake were felt at Pedara in Sicily.

THE additions to the Zoological Society's Gardens during the past week include a Leonine Monkey (*Macacus leoninus* ♂) from Arracan, presented by Mr. A. G. Henry; a Mule Deer (*Cervus macrotis* ♀) from North America, presented by Judge Caton, C.M.Z.S.; a Common Squirrel (*Sciurus vulgaris* ♀), British, presented by Miss A. M. Frost; a Common Pintail (*Dafila acuta* ♂), British, presented by Mr. Frank Seago; a Grey-lag Goose (*Anser ferus*), British, presented by Mr. Vincent W. Corbett; four Palmated Newts (*Triton palmipes*), British, presented by Mr. J. E. Kendall; a Radiated Tortoise (*Testudo radiata*) from Madagascar, deposited; a Black Saki (*Pithecia satanas* ♀), a White-bellied Parrot (*Caica leucogastra*) from the Amazons, a Talapoin Monkey (*Cercopithecus talapoin* ♂), four Harlequin Quails (*Coturnix histrionica* ♂ ♂ ♀ ♀) from West Africa, a Brazilian Blue Grosbeak (*Guiraca cærulea*), four Saffron Finches (*Sycalis flaveola* ♂ ♂ ♂ ♀) from Brazil, purchased.

OUR ASTRONOMICAL COLUMN

D'ARREST'S COMET.—On April 4 a.m. this comet was re-observed by Dr. Hartwig with the 20-inch refractor of the Observatory of Strasburg, near the position indicated by the elements of M. Leveau of Paris. The observation is a notable one, having been made at the great interval of 285 days from the date of perihelion passage; no other comet of short period has been hitherto observed under such circumstances, indeed there is only one instance upon record where a comet has been observed further from perihelion passage, and this was in the case of the celebrated comet of 1811, which was in perihelion on Sept. 12 in that year, and was followed by Wisniewsky till Aug. 17, 1812, or 309 days after its nearest approach to the sun. The great comet of 1861 was observed at Pulkowa 284 days after perihelion.

The comet in question was discovered by the late Prof. D'Arrest at Leipsic on June 27, 1851, and was observed at Berlin till October 6; its periodicity was pointed out by the same astronomer in the first week in August. MM. Oudemanns and Schulze specially occupied themselves with the investigation of its orbit in this year. At the next return in 1857 its position did not allow of observations in this hemisphere, but it was observed at the Royal Observatory, Cape of Good Hope, on December 5, and followed until January 18, 1858. The ensuing perihelion passage took place at the end of February, 1864, but from the unfavourable track of the comet in the heavens no observations were procured. During this revolution the comet had approached the planet Jupiter within about 0·36 of the earth's mean distance from the sun, and large perturbations of the elements were thereby produced; the nearest approach occurred in April, 1861. At the returns in 1870 and 1877 observations sufficient for the correction of the elements were obtained; the later investigation of the comet's motion has been ably conducted by M. Leveau.

In 1851 at perihelion the comet was distant from the earth's orbit only 0·162; at the present time this distance has been increased by perturbation to 0·316. There is a very close approach to the orbit of Jupiter, in heliocentric longitude 154°, or at an angular distance of about 165° before perihelion. In the orbit of 1870 the distance was 0·0845, in that of 1884 it is 0·1232; the presumption will therefore be that the attraction of this planet has fixed the comet in the system.

The following positions are calculated from M. Leveau's predicted elements; the perihelion passage occurs 1884, January 13 5765 G.M.T.:—

At Greenwich Midnight

	R.A. h.	m.	s.		Decl. °	'		Log. distance from Earth.		Sun
April 23,	13	38	14	...	+11	13·7	...	0·2951	...	0·4649
25,	,,	36	25	...	11	27·6				
27,	,,	34	37	...	11	40·8	...	0·2927	...	0·4609
29,	,,	32	50	...	11	53·2				
May 1,	,,	31	3	...	12	4·7	...	0·2912	. .	0·4569
3,	,,	29	18	...	12	15·3				
5,	,,	27	35	..	12	25·1	...	0·2906	...	0·4528
7,	,,	25	55	. .	12	33·9				
9,	,,	24	18	. .	+12	41·7	...	0·2908	...	0·4486

THE SOLAR ECLIPSE IN MAY.—On May 7, on the eastern coast of Australia, the sun will rise in a sea-horizon about the time of greatest eclipse. With favourable weather the observation will be a very interesting and unusual one, more particularly about Sydney, where the magnitude of the eclipse is greatest. It will be seen from the maps in our ephemerides that totality does not reach Australia, but at Sydney the sun will rise at 6h. 38m., within a quarter of an hour after the middle of the phenomenon, when the magnitude will be 0·95. In Queensland the magnitude diminishes to 0·75, and the sun will be in the horizon at greatest phase. At the former place, therefore, a narrow crescent emerging from the sea-horizon will constitute apparent sunrise.

PHYSICS IN RUSSIA DURING THE LAST TEN YEARS[1]

THE Russian Physical Society was founded only ten years ago, and since its foundation it has become the centre of all researches in physics carried on in Russia, which were limited before to a few dissertations written by Russian Professors of Physics in German Universities, and to a few memoirs communicated to the Academy of Sciences. At present the

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[1] Historical sketch of the work done by the Physical Society at the University of St. Petersburg during the last ten years by N. Hesehus in the *Journal of the Russian Chemical and Physical Society*, vol. xiv. fasc. ix.

Society has 120 members, a capital of 1638*l.*, a library, and a physical laboratory, mostly of instruments presented by M. Bazilevsky. As to the scientific communications made to the Society, they are of great value, as will be seen from the following brief summary.

The first rank among them belongs to the researches of Prof. Mendeléeff, which are nearly all connected with his extensive work on the elasticity of gases, these last leading him to a great number of collateral researches, and to the invention of new methods and instruments. Such are, for instance, his communications:—1. On a differential naphtha-barometer intended to show small changes of pressure. 2. On a levelling instrument, being a modification of the former, and easily showing changes of level of one metre; it might be applied also to the measurement of the changes of density of air; an entire memoir was written by M. Mendeléeff to describe this apparatus, which is susceptible of so many applications. 3. On a means of boiling mercury in barometers. 4. On a new siphon-barometer, which is, so to say, a combination of two siphon barometers connected together in their upper parts, one of the two tubes being capillary, and serving to exhaust the air which may penetrate Torricelli's vacuum, and for filling the instrument with mercury. 5. On a mercury pump which eliminates the disadvantages of friction. 6. On a very sensitive differential thermometer. 7. On a formula of expansion of mercury from temperature: the volume at a temperature t being $= 100,000 + 17 \cdot 99\, t + 0 \cdot 002112\, t^2$, where 100,000 represents the volume at zero. 8. On the coefficient of expansion of air; the experiments were made with great accuracy, and the volumes measured by the weight of mercury; the coefficient was found to be $\alpha = 0 \cdot 0036843$. 9. On the temperature of the upper strata of the atmosphere; according to the measurements of Mr. Glaisher, Prof. Mendeléeff found that the increase of temperature (t) is equal to the increase of pressure (H); that is, $\frac{d\,t}{d\,H} =$ *Const.*, or $t = C + H\frac{t_0 - C}{H_0}$. Taking, then, into account the influence of moisture, Prof. Mendeléeff deduced, from the laws of the mechanical theory of heat, a formula which better agrees with observations than the formula of Poisson, deduced for dry air. An accurate knowledge of the law of changes of temperature in the upper parts of the atmosphere having an immense importance for meteorology, astronomy, and cosmography, Prof. Mendeléeff elaborated a thorough scheme of aerostatic observations in Russia. 10. On a general formula for gases; instead of the well-known formulæ of Clapeyron, he proposes the following, which embodies the laws of Marriott, Gay-Lussac, and Avogadro:—$A\,P\,V = K\,M\,(C + T)$, where M is the weight of the gas in kilogrammes, and A – its molecular weight, the atomic weight of hydrogen being taken as unity; K is a constant for all gases, whilst the R of Clapeyron varies with the nature and mass of the gas. 11. On the compressibility of air under pressures less than that of the atmosphere; the chief results for pressures from 650 millimetres to 0·5 millimetre are: the law of Marriott not only is not true for low pressures, but the disagreement increases as the pressure decreases; the produce $P\,V$ (pressure multiplied by the volume), at pressures from 0·5 to 650 millimetres, *increases* for the air approximately from 100 to 150, instead of decreasing, as resulted from Regnault's measurements under higher pressures. This result was so unexpected and so contrary to current opinion that the measurements were repeated many times and by different methods, but the result was always the same. So it must be inferred (to use Prof. Mendeléeff's own words) "that as the rarefaction of gases goes on, a maximum volume, or limit volume, is reached, like the minimum or limit volume reached at compression; therefore it cannot be said that a gas, when rarefied, merges into luminous ether, and that the atmosphere of the earth has no limits." The rarefied gas becomes, so to speak, like a solid body. If the pressure on a solid is diminished its volume increases, but at a pressure equal to zero it still attains a limit volume. There are many other communications of less importance which were made also by Prof. Mendeléeff.

Some communications by M. Kraevich were also connected with the same subject. He made investigations into the degree of rarefaction reached by mercury-pumps; into the luminous phenomena in Geissler tubes; into the dissociation of sulphuric acid and glycerine in vacuum, and so on. A special interest is attached to his preliminary experiments on rarefied air by a new method, which experiments lead to the conclusion that "after a certain limit of rarefaction the elasticity decreases much more rapidly than the density, and at a very great degree of rarefaction the air loses its elasticity." These experiments would thus confirm the researches of Prof. Mendeléeff.—M. Kraevich has described an improved barometrograph, a portable barometer, and a mercury-pump of his own invention.

Several improvements of the barometer were proposed, too, by MM. Shpakovsky, Gulkovsky, Reinbot, and others. M. Lachinoff has proposed a mercury-pump without cocks. To the same department belong also the researches by M. Rykacheff into the resistance of the air; by M. Eleneff, on the coefficients of compressibility of several hydrocarbons; by M. Sreznevsky, on the evaporation of water-solutions of the chlorate of zinc; and by M. Schiff, on the compression of indiarubber cylinders.

In mechanics and mechanical physics M. Heselius notices the works, by M. Bobyleff, on the weighing methods of Borda and Gauss; on the length of the seconds-pendulum at Kharkoff, by M. Osiroff, and several other communications by MM. Bobyleff, Schiller, Lapunoff, and Gagarin.

Calorific phenomena were the subject of many communications, we notice these: On the calibration of thermometers, by MM. Mendeléeff and Lermontoff; on the expansion of mercury and gases, by M. Mendeléeff; a formula of expansion of mercury and water, by M. Rosenberg; on the expansion of indiarubber, by M. Lebedeff; on a new method of determining the caloric conductibility of bodies by heating them at one end, by Prof. Petrushevsky; and several communications on the critical temperature, by MM. Avenarius, Joule, and Strauss.

The communications on optics were numerous, and we notice among them the descriptions of an optical micrometer based on Newton's rings; and of a spectrophotometer, by Prof. Petrushevsky; the very interesting researches of M. Ewald on the phenomena of vision; the researches into the chemical action of light, by M. Lermontoff, who has tried to prove that light produces a dissociation of molecules and a new distribution of atoms whose return to their former distribution produces the phenomena of phosphorescence; several communications dealing with reflexion in mirrors; several papers on spectrum analysis; and researches dealing with photography.

The communications on electricity were as numerous as all the others taken together, the chief of them being: On the distribution of electricity on spheres under different conditions, and two other papers on electrostatics, of less importance, by M. Bobyleff; on the magnetisation of fine steel cylinders, by M. Khivolson, who has proposed a theory of residual magnetism, explaining these phenomena by the influence of molecules of carbon, which prevent to some extent the rotation of the molecules of iron; researches by M. Van der Flith on the mechanism of the interior and exterior phenomena of the current, which are explained by the molecular rotation in the circuit and by the breaking of equilibrium in the surrounding ether; the papers on thermoelectricity by Prof. Petrushevsky and M. Borgman, and several other papers by M. Borgman, Prof. Lenz, and Prof. Umoff; the microscopical researches into the crystallisation of the metal of electrodes, by M. Shidlovsky; and many others which it would be impossible to enumerate in this note. It will be sufficient to mention that the number of proposed electrical apparatus, as well as of papers on electro-technics, was very great, and some of them were of great value.

Cosmical physics was represented by most valuable papers on the resisting medium in space, by M. Asten; on the transits of Venus and Mercury, on variable and double stars, and on the parallax of refraction, by M. Glasenap; on the tails of the comets *b* and *c*, 1881, by Prof. Bredikhin; and by several interesting communications of MM. Woeikoff, Mendeléeff, Rykacheff, Schwedoff, and many others.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 1.—"Contributions to the Chemistry of Storage Batteries," by E. Frankland, D.C.L., F.R.S.

1. *Chemical Reactions.*—The chemical changes occurring during the charging and discharging of storage batteries have been the subject of considerable difference of opinion amongst chemists and physicists. Some writers believe that much of the storage effect depends upon the occlusion of oxygen and hydrogen gases by the positive and negative plates or by the active material thereon; some contend that lead sulphate plays an important

part; whilst others assert that no chemical change of this sulphate occurs either in the charging or discharging of the plates.

To test the first of these opinions, I made two plates of strips of thin lead twisted into corkscrew form, and after filling the gutter of the screw with minium, so as to form a cylinder that could be afterwards introduced into a piece of combustion-tubing, these plates were immersed in dilute sulphuric acid and charged by the dynamo-current in the usual manner. The charging was continued until the whole of the minium on the + and − plates respectively was converted into lead peroxide and spongy lead, and until gas bubbles streamed from the pores of the two cylinders.

After removal from the acid the plates were superficially dried by filter-paper, and immediately introduced into separate pieces of combustion-tubing previously drawn out at one end, so as to form gas delivery tubes. The wide ends of these tubes were then sealed before the blowpipe, care being taken not to allow the heat to reach the inclosed cylinders. The tube containing the cylinder of reduced lead was now gradually heated until the lead melted, the drawn-out end of the tube meanwhile dipping into a pneumatic trough. The gas expelled from the tube consisted almost exclusively of the expanded air of the tube and contained mere traces of hydrogen.

The tube containing the cylinder of lead peroxide was similarly treated, except that the heat was not carried high enough to decompose the peroxide. Mere traces, if any, of occluded oxygen were evolved.

These results justify the conclusion that occluded gases play practically no part in the phenomena of the storage cell.

With regard to the function of lead sulphate in storage batteries, I have observed that during the so-called "formation" of a storage cell a very large amount of sulphuric acid disappears from the liquid contents of the cell · indeed, sometimes the whole of it is withdrawn. The acid so removed must be employed in the formation of insoluble lead sulphate upon the plates which, in fact, soon become coated with a white deposit of the salt, formed equally upon both positive and negative surfaces. This visible deposit is, however, very superficial, and does not account for more than a very small fraction of the acid which actually disappears from solution. The great bulk of the lead sulphate cannot be discovered by the eye, owing to its admixture with chocolate-coloured lead peroxide.

Unless the coated plates have been previously immersed for several days in dilute sulphuric acid, this disappearance of acid during their "formation" continues for ten or twelve days. At length, however, as the charging goes on the strength of the acid ceases to diminish and soon afterwards begins to augment. The increase continues until the maximum charge has been reached and abundance of oxygen and hydrogen gases begin to be discharged from the plates; that is to say, until the current is occupied exclusively, or nearly so, in the electrolysis of hexabasic sulphuric acid · expressed by Burgoin in the following equation ·—

$$\underset{\text{Sulphuric acid}}{SO_6H_6} = \overbrace{\underset{\substack{\text{Sulphuric}\\\text{anhydride.}}}{SO_3} + 3O}^{\substack{\text{Eliminated on}\\+\text{ plate.}}} + \overset{\substack{\text{Eliminated on}\\-\text{ plate}}}{3H_2}.$$

Of course the sulphuric anhydride immediately combines with water and regenerates hexabasic sulphuric acid :—

$$SO_3 + 3OH_2 = SO_6H_6.$$

On discharging the cell the specific gravity of the acid continually decreases until the discharge is finished, when it is found to have sunk to about the same point from which it began to increase during the charging. Hence it is evident that during the discharge the lead sulphate, which was continuously decomposed in charging, was continuously reformed in discharging.

The chief if not the only chemical changes occurring during the charging of a storage battery, therefore, appear to be the following :—

1st. The electrolysis of hexabasic sulphuric acid according to the equation already given.

2nd. The reconversion of sulphuric anhydride into sulphuric acid.

3rd. The chemical action on the coating of the + plate.

$$\underset{\text{Lead sulphate.}}{SO_4Pb} + O + 3OH_2 = \underset{\text{Lead peroxide}}{PbO_2} + \underset{\substack{\text{Hexabasic}\\\text{sulphuric acid}}}{SO_6H_6}.$$

4th. The chemical action on the coating of the negative plate :—

$$\underset{\text{Lead sulphate.}}{SO_4Pb} + H_2 + 2OH_2 = Pb + \underset{\substack{\text{Hexabasic}\\\text{sulphuric acid.}}}{SO_6H_6}.$$

If I have correctly described these changes, the initial action in the charging of a storage cell is the electrolysis of hexabasic sulphuric acid, each molecule of which throws upon the positive plate three atoms of oxygen, and upon the negative plate six atoms or three molecules of hydrogen. Each atom of oxygen decomposes one molecule of lead sulphate on the positive plate, producing one molecule of lead peroxide, and one of sulphuric anhydride, the latter instantly uniting with three molecules of water to form hexabasic sulphuric acid.

The following are the chemical changes which I conceive to occur during the discharge of a storage cell :—

1st. The electrolysis of hexabasic sulphuric acid as in charging.

2nd. The reconversion of sulphuric anhydride into hexabasic sulphuric acid as already described.

3rd. The chemical action upon the coating of what was before the positive plate or electrode, but which now becomes the negative plate of the cell, that is to say, the plate from which the positive current issues to the external circuit :—

$$\underset{\text{Lead peroxide.}}{PbO_2} + H_2 = \underset{\text{Lead oxide.}}{PbO} + \underset{\text{Water.}}{OH_2}.$$

The lead oxide thus formed is immediately converted into lead sulphate :—

$$PbO + SO_6H_6 = SO_4Pb + 3OH_2.$$

4th. The chemical action upon the coating of what has now become the positive plate of the cell :—

$$Pb + O + SO_6H_6 = SO_4Pb + 3OH_2.$$

Thus in discharging, as in charging, a storage cell, the initial action is the electrolysis of hexabasic sulphuric acid. The oxygen eliminated on the positive plate reconverts the reduced metal of that plate into lead oxide, whilst the hydrogen transforms the lead peroxide on the negative plate into the same oxide, which in both cases is immediately converted into lead sulphate by the surrounding sulphuric acid, thus restoring both plates to their original condition before the charging began.

The real "formation" of the cell consists, I conceive, in the more or less thorough decomposition of those portions of the lead sulphate which are comparatively remote from the conducting metallic nucleus of the plate. Lead sulphate itself has a very low conductivity, whilst lead peroxide, and especially spongy lead, offers comparatively little resistance to the current, which is thus enabled to bring the outlying portions of the coating under its influence. It may be objected that, during the discharge, the work of formation would be undone; but probably, in the ordinary use of a storage battery, the discharge is never completed. Thus I have found that, in a small cell containing two plates 6″ × 2″, short circuiting with a thick copper wire for twelve hours was far from producing complete discharge, for on breaking this short circuit the cell *instantly* rang violently an electric bell with which it was previously connected. In ordinary discharges of "formed" cells, therefore, the lead sulphate on the positive and negative plates still remains mixed with sufficient lead oxide and spongy lead respectively to give it a higher conducting power than the sulphate alone possesses.

2. *Chemical Estimation of the Charge in a Storage Cell.*—No method has hitherto been known by which the charge in a storage cell could be ascertained without discharging the cell; but the results of the foregoing experiments indicate a very simple means of ascertaining the amount of stored energy without any interference with the charge itself. The specific gravity and consequent strength of the dilute sulphuric acid of a "formed" cell being known in its uncharged and also in its fully charged condition, it is only necessary to take the specific gravity of the acid at any time in order to ascertain the proportion of its full charge which the cell contains at that moment; and if the duty of the cell is known, the amount of energy stored will also be thereby indicated. In the case of the cell with which I have experimented, containing about seven quarts of dilute sulphuric acid, each increase of ·005 in the specific gravity of the dilute acid means a storage of energy equal to 20 amperes of current for one hour, obtainable on discharge.

I hope shortly to be able to express, in terms of current from the cell, the definite relation between the amount of energy stored and the weight of sulphuric acid liberated.

Chemical Society, March 30.—Anniversary Meeting.—Dr. Gilbert, president, in the chair.—The President presented his annual report, in which he gives a review of the progress of the Society from the commencement of its existence in 1841 up to the present time. The Society numbers 1247 Fellows, with an income of about 3000*l*. During the past year 70 papers have been read, and a discourse delivered by Prof. Dewar. Grants in aid of research have been made of 220*l*. 1775 copies of the *Journal* were printed during the past year. The library contains 6800 volumes, and a new catalogue will shortly be issued to the Fellows. In his address the President gives a most interesting *résumé* of the arrangements for chemical education and research on the American Continent. After the usual votes of thanks the following Officers, &c., were balloted for and declared duly elected :—President, W. H. Perkin, Ph.D., F.R.S. Vice-presidents: F. A. Abel, Warren De La Rue, E. Frankland, J. H. Gilbert, J. H. Gladstone, A. W. Hofmann, W. Odling, Lyon Playfair, H. E. Roscoe, A. W. Williamson, A. Crum Brown, P. Griess, G. D. Liveing, J. E. Reynolds, E. Schunck, A. Voelcker. Secretaries: H. E. Armstrong, J. Millar Thomson. Foreign Secretary, Hugo Muller. Treasurer, W. J. Russell. Council: E. Atkinson, Capt. Abney, H. T. Brown, W. R. E. Hodgkinson, D. Howard, F. R. Japp, H. McLeod, G. H. Makins, R. Meldola, E. J. Mills, C. O'Sullivan, C. Schorlemmer.

Meteorological Society, March 21.—Mr. J. K. Laughton, F.R.G.S., president, in the chair.—The following gentlemen were elected Fellows of the Society: viz. Mr. G. T. Hawley, Dr. C. W. Siemens, F.R.S., Mr. C. Walford, F.S.S., and Col. H. G. Young. Dr. W. Köppen was elected an Honorary Member.—The paper read was notes on a march to the hills of Beloochistan, in North-West India, in the months of May to August, 1859, with remarks on the simoom and on dust storms, by Dr. H. Cook, F.R.G.S., F.M.S. These months may be considered as the summer of the hill-country of Beloochistan, though the natives expect the weather to change soon after the fall of rain, which takes place about the end of July and beginning of August. Compared with that of the plains, the climate is delightful. The actual heat is greater than in England, especially the intensity of the sun's rays, but the weather is less variable. Fruits and crops, as a rule, ripen earlier, and are not exposed to the vicissitudes of the English climate. The atmosphere is clear and pure, the air dry and bracing. Dr. Cook describes different kinds of dust-storms, and considers that they are due to an excess of atmospheric electricity. With regard to the simoom, which occurs usually during the hot months of June and July, it is sudden in its attack, and is sometimes preceded by a cold current of air. It takes place at night, as well as by day, its course being straight and defined, and it burns up or destroys the vitality of animals and vegetable existence. It is attended by a well marked sulphurous odour, and is described as being like the blast of a furnace, and the current of air in which it passes is evidently greatly heated. Dr. Cook believes it to be a very concentrated form of ozone, generated in the atmosphere by some intensely marked electrical condition.—After the reading of this paper the Fellows inspected the exhibition of meteorological instruments for travellers, and of such new instruments as had been constructed since the last exhibition. In addition to the ordinary instruments designed for travellers, viz. barometers, thermometers, hypsometrical apparatus, compasses, artificial horizons, &c., some very interesting historical instruments used by celebrated travellers and explorers were exhibited, including those used by Dr. Livingstone in his last journey; by Commander Cameron during his journey across Africa; by Sir J. C. Ross in his Antarctic Expedition; by Sir E. Sabine, in his Arctic voyage, &c.

Zoological Society, March 20.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. Sclater called attention to the fact that a living specimen of *Macropus erubescens* (a species originally described from a single specimen living in the Society's Gardens) was in the Gardens of the Zoological and Acclimatisation Society of Melbourne.—Mr. Sclater laid before the meeting a set of the sheets of a new List of British Birds which had been prepared by a Committee of the "British Ornithologists' Union," and would shortly be published, and explained the principles upon which it had been constructed.—Prof. Huxley read a paper on the oviduct of the Common Smelt (*Osmerus eperlanus*), and took occasion to remark on the relations of the Teleostean with the Ganoid fishes. Prof. Huxley came to the conclusion that the proposal to separate the Elasmobranchs, Ganoids, and Dipnoans into a group apart from and equivalent to the Teleosteans was inconsistent with the plainest anatomical relations of these fishes.—Mr. G. A. Boulenger read a paper containing the description of a new species of Batrachian of the genus *Bufo* obtained at Yokohama, Japan, during the expedition of H.M.S. *Challenger*. The author proposed to describe it as *Bufo formosus*.—A communication was read from Mr. W. N. Parker containing some notes on the respiratory organs of *Rhea macrorhyncha*, and comparing these organs with those of the Apteryx and Duck.

Royal Horticultural Society, March 27.—Sir J. D. Hooker, K.C.S.I., in the chair.—*Sclerotia of Peronospora infestans:* Mr. W. G. Smith called attention to the fact that the so-called "sclerotia," described in a paper by Mr. A. Stephen Wilson, read at the last meeting, were observed and figured by Von Martius so long ago as in 1842 ("Die Kartoffel Epidermie") as Protomyces and by Berkeley as Tubercinia in his paper on the Potato Murrain, in the first volume of the *Hort. Soc. Journal*, 1846. They were subsequently figured by Broome in 1875, and by Prof. Buckman. Mr. G. Murray said that from his examination they often seemed to consist of the discoloured and disorganised contents of the cells, which they completely filled, though in Martius's drawing two or three were in one cell; Dr. Masters, however, noticed that they are often *outside* the cells and of an angular character, as if they had not assumed the form of the interior of a cell. The question was raised whether they might not have been expressed by the covering glass. Martius figured them with conidiferous threads proceeding apparently in abundance from them. Further investigation of their true nature was thought desirable.—*Abutilon* and *Hibiscus* "bigener": Dr. Masters described a very dark-flowered Abutilon which was said to be due to an original cross between *H. Rosa-sinensis* and *A. striatum*. The original plant was a dark-flowered seedling which was fertilised by Mr. George of Putney for two or three generations with the pollen of the Hibiscus, and though the character of the flower is that of an Abutilon, it has the truncated column and foliage of the Hibiscus, thus showing distinctly intermediate characters. In one plant the leaves were marked with a dark crimson spot. Hence it appears to be a true bigener, or cross between two distinct genera.—*Ivy-leaved Pelargonium Cross:* Mr. George sent some foliage of a cross between the ivy-leaved and a rough-leaved Pelargonium. Several showed a reversion to the peltate type, some assuming a funnel-shape or other irregular form, thus betraying its origin from *P. peltatum*.—*Orange-trees attacked by Mytilaspis citricola*, one of the Coccidæ: Mr. Maclachlan exhibited leaves and branches of oranges much injured by this insect from the Bahamas. He read a communication by Messrs. Dunlop and Roker communicated by the Governor to the British Government, requesting information. The insect was therein named *Aspidites Gloverii*. He made some remarks on the method of attack of the insect, and suggestions as to remedies to suppress it, such as washes and syringing with petroleum and the use of whale-oil soap.—*Solanum species:* Sir J. D. Hooker read a communication from Mr. Lemmon, of Oakland, California, upon the discovery of three species or varieties of Solanum bearing tubers, from the borderland of Arizona and Mexico:—"We found them first," writes the author, "on the cool northern slopes of the high peaks [of the Huachuca range]; then afterwards, where least expected, invading the few rudely cultivated gardens of the lower foothills. One kind is called *S. Jamesii*, Tor., in the "Survey of the Mexican Boundary." This has white flowers and tubers. Another was *S. Fendleri*, Gr. It has smaller purple flowers and flesh-coloured tubers. This Dr. Gray lately concludes to be but a variety of the old Peruvian potato, and he calls it *S. tuberosum*, var. *boreale*. The third form or species found at 10,000 feet altitude has mostly single orbicular leaves, one or two berries only to the umbel, and small pink tubers on long stolons, growing in loose leaf-mould of the cool, northern forested slopes. . . . I have great faith in the successful raising of one of these species (or varieties) to a useful size, for the following reasons:—1. While the *S. tuberosum*, var. *boreale*, bears long stolons and but a few tubers, the other kind, *S. Jamesii*, makes many short stolons terminated by four to eight large, round white tubers. 2. While the first kind has been partially tried and then given up, the latter species is known to have become enlarged to the size of domestic hens' eggs during the accidental cultivation of three years in the embanking of a rude fish-pond."

EDINBURGH

Royal Society, March 19.—Prof. Maclagan, vice-president, in the chair.—Mr. Sang read a paper on the impossibility of inverted images in the air, in which he discussed the conditions as to density necessary for such an effect, concluding that these atmospheric conditions were so unstable as to make it physically impossible for clear images to be formed. The famous observation by Vince of the erect and inverted images high up in air was, he maintained, simply the case of a vessel and its reflection in the sea, which was so calm as to be indistinguishable from the sky—the *apparent* horizon being the margin of a ruffled portion of the surface between the true horizon and the observer.—Prof. Tait communicated a note on the thermoelectric positions of pure rhodium and iridium, specimens of which had been supplied him by Messrs. Johnson and Matthey. The lines of these metals on the thermoelectric diagram were found to be parallel to the lead line, that is, according to Le Roux, the Thomson effect is *nil* in them. Unfortunately the lines are too close to be of any practical use as a thermoelectric thermometer.—Dr. Christison gave the results of the observations on the growth of wood in deciduous and evergreen trees, which had been begun by the late Sir Robert Christison in 1878, and continued by himself since Sir Robert's death. It appeared that the evergreen trees began their rapid growth much earlier in the year than the deciduous trees, and stopped sooner. Hence the reason why the variations in growth in successive years did not follow the same law in these two classes—an early winter affecting the deciduous trees, a late winter the evergreen. The effect of wet seasons was also indicated, the deciduous trees being apparently more influenced.—Mr. Buchan read a paper on the variation of temperature with sunspots. The comparison was not a direct one, but was based upon the well-known phenomenon of the diurnal barometric oscillation viewed in relation to the amount of water vapour in the air. From the observations of the *Challenger* Expedition, Mr. Buchan had concluded that this diurnal variation over the open sea was not the result of changes of surface temperature (for these were very small), but was to be referred to the direct heating effect of the sun upon the air, or more strictly upon the water vapour in the air. This view was supported by the fact that over the sea the diurnal variation of pressure was greatest where most vapour was; whereas the contrary held over the land, the temperature of which varied greatly during the day, and the more so when the air above was drier, as more heat then reached the earth. In other words, the increase of moisture in the air increases the barometric oscillation over the sea and diminishes it over the land; and hence it seemed probable that the discussion of these daily oscillations in sun-spot cycles might lead to some definite result. The long-continued observations at Calcutta, Madras, and Bombay were combined in this way, and yielded a remarkable result—there being a well-marked maximum of barometric diurnal oscillation half way between the minimum and maximum sun-spot years, and a minimum half way between the maximum and minimum years. The averages were taken for the five dry winter months, and the effects were explained as due to the accumulated water vapour in the upper southerly winds that exist over India during these months. When the rainfall on the southern slopes of the Himalayas was similarly treated—which rainfall is of course due to the arresting of these upper moist currents—the analogous fact was brought out, viz. minimum rainfall at times of maximum barometric oscillation and *vice versâ*.

DUBLIN

Experimental Science Association, March 13.—On Ayrton and Perry's voltmeter, by Prof. Fitzgerald.—On an experiment on the resonance of flames, by H. Maxwell. A vibrating tuning fork when held in a gas or candle flame, or in the heated current of air above, was shown to have its note greatly strengthened. A current of unignited gas produced no perceptible strengthening of the note.—A thermal galvanoscope, by C. D. Wray. A method of showing to an audience the expansion of a wire under the heating influence of a current of electricity.—On a thermometer that can be read by telegraph, by J. Joly. An arrangement whereby the level of the mercury in a thermometer can be read by reckoning the number of contacts made with a battery in the home station. Suitable mechanism on the thermometer causes a wire to advance down the open tube of the thermometer, by a known minute distance, at each passage of the current. On reaching the mercury, a current passes to a galvanometer in the home station.

SYDNEY

Linnean Society of New South Wales, January 31.—C. S. Wilkinson, F.G.S., president, in the chair.—The following papers were read:—On a new form of mullet from New Guinea, by William Macleay, F.L.S., &c. This is a description of a very remarkable freshwater fish from the interior of New Guinea, allied to Mugil, but constituting a new genus to which the author gives the name of *Æschrichthys*.—By J. J. Fletcher, M.A., B.Sc. The second part of his paper upon the anatomy of the urogenital organs in females of certain species of Kangaroos.—On remains of an extinct Marsupial, by Chas. W. De Vis, B.A. This is a very careful description of a number of bones found together and evidently of the same individual, by Mr. Henry Tryon, in Gowrie Creek, Darling Downs. The bones and teeth point to some bilophodont form, showing affinity with *Macropus* and *Palorchestes* on the one hand, and with *Nototherium* and *Diprotodon* on the other.—Contributions to the ornithology of New Guinea, by E. P. Ramsay, F.L.S., &c. This contained a complete list of the birds recently brought by Mr. Goldie and others from the south-east part of the island.—On a new species of Tree Kangaroo from New Guinea, by the same author. This differs from *Dendrolagus venustus* in some particulars, and is named after the Marquis Doria. A new Rat (*Hapalotis Papuanus*) was also described.—On some habits of *Pelopæus lætus* and a species of *Larrada*, by Mr. H. R. Whittell.—Mr. Whittell also read a short paper on the voracity of a species of *Heterostoma*. He had observed one of these centipedes in the act of eating a live lizard. The aggressor, evidently finding his victim too powerful for his unassisted strength, had ingeniously taken a double turn with the posterior portion of his body around a small stem which was found conveniently at hand, and so was enabled to continue his meal without interruption.

BERLIN

Physical Society, March 2.—Prof. Kirchhoff in the chair.—Dr. König reported on two optico-physiological researches, which he had carried out in consequence of his optical studies with the leucoscope. In the first he has, with the aid of a special apparatus, examined a number of colour-blind persons as to the position in the spectrum of their so-called "neutral" point. According to the Young-Helmholtz theory, it is known, there are three primary colours (red, green, and violet), each of which produces its special colour-sensation, while all combined give the impression of white. The sensibility for the three primary colours is so distributed over the spectrum that their curves in great part coincide on the abscissa of wave-lengths, and therefore mixed colour-sensations occur everywhere, while the maxima of the separate curves occur at the places of brightest red, green, and violet respectively. In the case of the colour-blind one curve is wanting, and the two remaining ones have therefore a point of section where their ordinates are the same. Hence the eye must at this part have the impression of white or grey. For finding this neutral point in the spectrum, an apparatus served, in which the telescope of a spectroscope was so arranged with regard to the non-refringent angle of the prism that the spectrum took up only half of the field of vision, while the other half was occupied with the image of the white-painted ground-surface of the prism. Instead of the eyepiece there was another slit in the telescope, in which one saw only a small section of the spectrum; by micrometric displacement of the collimator of the spectral apparatus any part of the spectrum could be brought on the slit. Now at a particular part of the spectrum the colour-blind person saw both halves of the field of vision white, while the person with normal vision saw the part of the spectrum in question in its normal colour, and so could determine the wave-length at which the neutral point of the colour-blind person occurred. Changes of light-intensity displaced the neutral point; hence in comparative measurements care must be taken to have the same intensity in the source of light. Such measurements were made by Dr. König with great precision on nine colour-blind persons, and it appeared that the neutral points are situated between about 491 and 500 millionths of a millimetre, and (what is of special interest theoretically) that the mean values of the separate observations with different colour-blind persons were not equal, but varied in a pretty regular series between the two terminal values. According to the common view that colour-blindness depends on the disappearance of one of the normal three curves of colour-perception, the position of the neutral point as point of section of the two curves present must be always the same, and for the red and the green blind must be at two quite determinate points of the spectrum. As the experiments have yielded a different result in persons, two of whom were red-blind, and seven green-blind, Dr. König believes that the essence of colour-blindness consists not in the absence of one curve, but in the displacement of two curves on one another, which may be more or less complete, and so produces the different degrees of colour-blindness observed. In the second investigation Dr. Konig sought to determine the two remarkable points of section of the three curves that occur, according to the Young-Helmholtz theory, in normal colour-perception. From the researches of Prof. von Helmholtz on the wave-lengths of the complementary colours, and from those of Clerk Maxwell on colour-mixtures, appear values for these points of section which agree pretty well. The same values, approximately, are reached by the researches of several ophthalmologists on the places of quickest change of colour in the spectrum. Dr. König tried to determine the first section point by making the violet curve disappear through the taking of santonin, and when he had thus made himself temporarily violet-blind, he determined his neutral point, the point of section of the red and the green curve. All these determinations and theoretical considerations led to pretty much the same values for the points of section, and the first point is situated not, as is often supposed, in the yellow, but in the blue, between the Fraunhofer lines E and b_1, and nearer the latter.

PARIS

Academy of Sciences, March 26.—M. Blanchard in the chair.—The following papers were read:—On an objection of M. Tacchini relative to the theory of the sun in the *Memorie dei Spettroscopisti Italiani*, by M. Faye. Having observed the eruptions accompanying a spot to be intermittent and of brief duration, M. Tacchini thinks this fact against the theory of the spots being due to cyclonic movements. M. Faye says this is as if, on seeing the water-jet of a force-pump go down, one maintained that the pump did not exist.—Contribution to the study of stamping and of the "prows" it produces, by M. Tresca.—On the motion and deformation of a liquid bubble which rises in a liquid mass of greater density, by M. Resal.—Note on the preparation of oxide of cerium, by M. Debray.—On the reading of a report by M. d'Abbadie on his transit expedition to the island of Haiti, the president expressed the felicitations of the Academy (concern had been felt on account of the prevalence of yellow fever).—Addition to preceding communications on continuous periodic fractions, by M. de Jonquières.—Character by which one may perceive if the operation indicated by

$$\sqrt[2m+1]{a\sqrt{v} \pm b\sqrt{wi}}, \text{ or by } \sqrt[2m]{a \pm b\sqrt{vwi}},$$

may be effected under the form $\alpha\sqrt{v} \pm \beta\sqrt{wi}$, m designating a positive whole number, v and w positive rational numbers, and a and b, α and β any rational numbers; method of effecting this operation, by M. Weichold.—On a spectroscope with inclined slit, by M. Garbe. He described to the French Physical Society on March 2 an arrangement similar to M. Thollon's.—Observation on the figures of consumption of zinc given by M. G. Trouvé for his bichromate of potash batteries, by M. Regnier. He points out a difference between the effective and the theoretical expenditure.—Heat of formation of glycolates, by M. de Forcrand.—Action of sulphur on oxides, by MM. Filhol and Senderens. Sulphur acts on alkalies in the dissolved state less and less easily the greater the dilution.—On the action of different varieties of silica on lime water, by M. Landrin. Hydraulic silica, gelatinous silica, and soluble silica absorb lime water more or less rapidly, but in all cases the absorption finally varies, for one equivalent of silica, between the limits 36 and 38. The formula $3SiO_2,4CaO$, requiring for 30 of silica 37·3 of lime, thus fairly expresses the limit towards which those phenomena tend.—On the hydrate type of neutral sulphate of alumina, by M. Marguerite-Delacharlonny.—On the production of bromised apatites and Wagnerites, by M. Ditte.—Researches on crystalline phosphates, by MM. Hautefeuille and Margottet.—On various effects of air on beer yeast, by M. Cochin. It is only some time after the glucose solution has penetrated, by endosmose, the membranous envelope of the yeast cells, that fermentation commences. Sometimes (yeast aërated) the sugary liquid simply penetrates into the yeast, the proportion of sugar outside continuing undiminished; sometimes (yeast deprived of air) the sugar is absorbed in larger quantity by the yeast and the liquid outside impoverished. It is within the cells that the sugar is transformed. Probably air attenuates ferments as it does virus.—Determination of extractive matters and of reducing power of urine, by MM. Etard and Richet. This determination is made with bromine; which in acid solution attacks the uric acid and the extractive matters. The reducing power of urine varies much in different individuals, but little in one individual.—The perception of colour and the perception of form, by M. Charpentier. Luminous rays have two distinct actions on the visual apparatus—one gives rise to the rudimentary perception of light and is distributed pretty equally over all points of the retina; the other is more efficacious on the centre of the retina, giving rise, on the one hand, to the sensation of colour, on the other to the distinction of multiple luminous points.—Note on the adherence of a frontal tumour with the yolk, observed in a cassowary which died in the egg at the moment of hatching, by M. Dareste.—New observations on the dimorphism of Foraminifera, by MM. Munier-Chalmas and Schlumberger.—Attempt at application of M. Faye's cyclonic theory to the history of primitive meteorites, by M. Meunier. He considers that chondrites are to rocks of gaseous precipitation what iron grains, &c., are to rocks of aqueous precipitation. They testify to eddies in the generating medium, to photospheric cyclones.—On shocks of earthquake observed in the department of La Mayenne, by M. Faucon. These were felt about 3 p.m. on March 8. Three considerable trepidations occurred in a few seconds.—M. Decharme described a method of preserving and reproducing crystalline forms of water. A horizontal glass plate at a low temperature is covered with a thin layer of water mixed with minium; particles of the minium are involved in the formation of ice. Ulterior fusion and evaporation leave the minium in position.

VIENNA

Imperial Academy of Sciences, February 15.—C. v. Ettingshausen, contributions to the knowledge of the Tertiary flora of Australia.—F. Brauer, to nearer knowledge of the Odonatæ, genera Orchithemis, Lyriothemis, and Agrionoptera.—On the systematic position of the genus Lobogaster, Pil., by the same.—S. Tolver Preston, a dynamic explanation of gravitation.—On the possibility of explaining past changes in the universe by the action of natural laws now active, by the same.—E. Heinricher, contributions to the teratology of plants and morphology of flowers.—P. Pastrovich, on Reichenbach's picamar; on cœrulignol, Reichenbach's oxidating principle.—A. Tarolimek, on the relation between tension and temperature of saturated vapour.

March 1.—W. Biedermann, contributions to general nerve and muscle physiology (eleventh communication); on rhythmic contractions of striped muscles under the influence of constant currents.—V. Graber, fundamental experiments on the light and colour sensibility of eyeless and blinded animals.—P. R. Handmann, on a very useful filling of the zinc-carbon battery.

CONTENTS

PAGE
THE VIVISECTION BILL 549
THE BRITISH NAVY. By W. H. WHITE 549
OUR BOOK SHELF:—
  Baillie-Grohman's "Camps in the Rockies" 551
  Eckardt's "Physics in Pictures" 551
LETTERS TO THE EDITOR:—
  Unprecedented Cold in the Riviera—Absence of Sunspots.—C. J. B. WILLIAMS 551
  Mr. Grant Allen's Article on "The Shapes of Leaves"—F. O. BOWER; GRANT ALLEN 552
  Ticks.—Dr. T. SPENCER COBBOLD, F.R.S.; Rev. L. BLOMEFIELD 552
  Helix pomatia, L.—Rev L. BLOMEFIELD 553
  Braces or Waistband?—N. 553
SOLAR RADIATION AND GLACIER MOTION. By Rev. A. IRVING . . 553
DEDUCTIVE BIOLOGY. By W. T. THISELTON DYER, C.M.G., F.R.S. 554
THE APPROACHING ECLIPSE (*With Illustration*) 556
DEATHS FROM SNAKE BITE IN BOMBAY. By Sir JOSEPH FAYRER, K.C.S.I., F.R.S. 556
ASTRONOMICAL PHOTOGRAPHY. By EDWARD C. PICKERING, Director of the Harvard College Observatory 556
DARWIN AND COPERNICUS. By Prof. E. DU BOIS REYMOND . . . 557
SINGING, SPEAKING, AND STAMMERING, III. By W. H. STONE, M.B., F.R.C.P. 558
DISTRIBUTION OF ENERGY IN THE SPECTRUM. By Lord RAYLEIGH, F.R.S. 559
THE ORNITHOLOGIST IN SIBERIA (*With Illustrations*) 560
THE BACILLUS OF TUBERCLE 563
PROFESSOR H. J. S SMITH AND THE REPRESENTATION OF A NUMBER AS A SUM OF SQUARES 564
NOTES 565
OUR ASTRONOMICAL COLUMN:—
  D'Arrest's Comet 567
  The Solar Eclipse in May 567
PHYSICS IN RUSSIA DURING THE LAST TEN YEARS 567
SOCIETIES AND ACADEMIES 568

THURSDAY, APRIL 19, 1883

THE SCOTCH UNIVERSITIES BILL

THE long expected Scotch Universities Bill has at last made its appearance. As no explanation of its provisions has yet been offered in Parliament, and the Scotch newspapers have shown the caution characteristic of their country in declining to commit themselves to an opinion about it till they learn what its authors have to say in its favour, it may be interesting to our readers to know what the Bill proposes to do and how it proposes to do it. So much at least can be stated in a few sentences. The Scotch Universities derive a considerable portion of their revenues from Parliamentary grants. The Bill proposes to give them a sum which is estimated at about 8000*l.* a year, or 25 per cent. more than they now get; to remove the whole of their payment from public moneys from the annual estimates to the Consolidated Fund; to settle this sum of 40,000*l.* on them "in full discharge of all claims past, present, and future," and to cut them adrift. They now get really about 28,000*l.* annually, the other 4000*l.* going to two institutions—the Royal Observatory in Edinburgh, and the Botanic Garden there, which are in future to be handed over to the University of Edinburgh and to be maintained by it out of the portion of the 40,000*l.* to be allocated to it. The allocation of this sum as between the Universities is to be made once and for ever by a new Executive Commission, with whose judgment, except in the form of a somewhat complicated and expensive appeal to Her Majesty in Council and the usual formal laying of their ordinances on the table of Parliament, the State will not farther concern itself.

The second main provision of the Bill is that these Commissioners are directed to make ordinances, subject only to the same appeal, regulating everything in or concerning these Universities, and in particular fixing anew the *constitution* and functions of all the various University bodies and officers, such as the University Court, the University Council, the Senatus Academicus, the chancellor, the rector, the assessors, and all other University officers. They are directed in only two particulars. They are to institute a first examination which is to be compulsory on all persons who intend to graduate in Arts or in any other Faculty, and to institute if they think fit, in any or all of the Universities, a new Faculty of Science, subject to these particular directions: they are to "regulate the manner and conditions in and under which students shall be admitted, the course of study and manner of teaching, the amount and exaction of fees, the length of the academical session or sessions, and the manner of examination."

The next important duty imposed on the Commissioners is to report within twelve months whether in their opinion it is no longer possible for the University of St. Andrews, which is the oldest and by far the least numerously attended of the four, "in consequence of the want of sufficient endowments," to "continue to perform its functions with advantage," and in the event of their so reporting they are to make "suggestions for dissolving that University and its Colleges, and creating a new corporation to which the funds and property of the University and Colleges shall be transferred."

There is another curious provision, which we mention only from the interest which will generally attach to it, not because we should venture in this place to express any opinion about it, in one way or another. Like all the Universities in the kingdom, except London and the new Victoria University, the Scotch Universities have a Faculty of Theology. This has been hitherto in direct connection with the Scotch Established Church, and the Professorships can only be held by clergymen of that Church. It is well known that the Nonconformist denominations in Scotland prescribe a professional course of their own for students preparing for their ministry, and the two great Presbyterian nonconforming bodies have each of them Colleges and Professors, whose lectures their students must attend. The Bill provides that from this time forward no test of any kind shall be applicable to the University Chairs of Theology, which may therefore either be held by clergymen of any persuasion or by laymen. Should this provision become law, it will be most interesting to watch what may be the tendencies and character of the new scientific theology which will develop itself in Scotland after it has been freed from the trammels of any creed. It is to be feared, indeed, that the first effect may be that the students who now attend the University Chairs of Theology may be directed elsewhere to new Colleges or Halls of Presbyterian theology taught from the point of view of the Established Church, and that the rising clergymen of the nation, who are generally of opinion that they do enough when they do all that their licensing bodies require of them, may not sit in great numbers at the feet of the occupants of the new scientific Chairs. There is another provision which illustrates in a singular way the jealousy with which a lay State can scarcely help regarding theology, even after it has become scientific, and "in the abstract." Whatever happens, whoever may benefit by the 25 per cent. of increased emolument to be made over to the Scotch Universities, it is expressly provided that the scientific theologians are never to get any of it.

The most interesting question to our readers is how the new Bill will influence the progress of science in the Scotch Universities. The obvious and only answer is that nobody can tell. The Commissioners may make provision for a Faculty of Science, and in the three younger and more numerously attended Universities they will probably do so. In Edinburgh they could certainly do so without requiring to create new Chairs. In Glasgow there is not at present a Chair of Geology, though that subject is taught in an old-fashioned alliance with zoology, by the single Professor of Natural History. There is no Professor of Geology or of Astronomy or of Engineering in Aberdeen. The foundation of new Chairs on these subjects may possibly be thought necessary before a Faculty of Science is instituted; and there are medical Chairs, like that for Pathological Anatomy, which are not established in Glasgow. A great deal will depend, in fact, on the extent to which the free balance of 7500*l.* or thereabouts may be found sufficient to meet the more urgent and immediate demands which will be made on it from all quarters. Glasgow and Aberdeen have no Chair of Modern History. In Aberdeen one Professor teaches English Literature and Logic, and there is no Chair of Political Economy. In the University of Adam Smith

Political Economy is only taught by the incumbent of the Chair of Moral Philosophy. The recommendations of the Inquiry Commissioners stated urgent wants of the Universities five years ago which would amount to much more than the added 25 per cent. now to be given to the Commissioners to settle upon the Universities for ever. It is true that Scotland is now both a rich and a liberal country, and that much may be expected in future from the direct contributions of her people. But experience has abundantly shown that private benevolence is never organised benevolence, and that sums which might in the aggregate be sufficient to meet all the most urgent wants of the Universities are not to be expected to be provided by voluntary contributions where or when they are most wanted. To reorganise the Scotch Universities, a liberal provision of public money is probably necessary, and it seems strange to throw the burden of such a provision on a fixed and moderate sum, which is declared to be for ever incapable of increase. It is not for us of course to consider whether Parliament would act wisely in placing the grants for the Scotch Universities on the annual estimates, where they are always open to comparison and challenge, or on the Consolidated Fund, where they are practically liable neither to increase nor to diminution. But it seems a strange policy to declare beforehand that the grants for objects which are admitted to be of national importance shall never exceed a severely limited sum. The demands of science alone are continually increasing in pecuniary severity, and we say no more than every one will admit when we add, that it is not for the public advantage that the natural teaching of science should be hindered in any of the three kingdoms by a too rigid or mechanical economy. It is not placing her in her true position to compel her to an undignified struggle with a host of other claimants for her fair share of a moderate allowance which cannot be increased. If the Scotch Universities had great College estates and ample revenues like Oxford and Cambridge, her claims might be met from time to time as they have been in England. Until of late years they have been very moderately provided institutions, and there are no available funds for the extensions of the future but the freewill offerings of her people. The State is, in our opinion, reasonably expected from time to time to organise, or to help to reorganise them in the interests of the nation. We should like to see it ready to do something more in that way than to offer to cut them adrift with a little extra money, and to provide Commissioners to whose absolute discretion is to be intrusted the reconstructive duties which naturally devolve on Parliament. The Oxford and Cambridge Act of 1877 gave the Commissioners then appointed very extensive powers, but it was in marked contrast, in the precision and fulness of its enacting clauses, and in the checks under which the Commissioners were to exercise their functions, to the Scotch Universities Bill of 1883.

THE SCHEME OF THE GROCERS' COMPANY FOR THE ENCOURAGEMENT OF ORIGINAL RESEARCH IN SANITARY SCIENCE

THE relation of man, whether savage or civilised, to his surroundings is one of constant exposure to influences which are hostile to his bodily well-being. Some of these are dangerous chiefly by reason of their insidiousness; others, although not concealed, are in their nature unavoidable; others, though both known and avoidable, are yet for various reasons not avoided. All of them, of whichever class, are subjects of earnest study to the pathologist, one part of whose science is for this reason called etiology as relating to the *causes of disease*, the other being concerned with the disturbances which these causes induce inside the living human organism. "Sanitary Science" in so far as it is a science, is identical with etiology, and is therefore a branch of pathology. In this sense it is the science on which the art of preventing disease, or "Preventive Medicine," as it is commonly called, is founded, and it will be admitted that, whatever doubt may exist as to the utility of exact knowledge of the nature of disease for its cure, there can be none as to its direct applicability to prevention.

The Grocers' Company, one of the oldest and most distinguished of the City Guilds and second to none in the liberality with which it has always bestowed its funds for the general good, has thought fit to create an endowment, or rather a system of endowments, for the encouragement of "Original Research in Sanitary Science." This it defines as relating to the "causes of important diseases and the means by which the respective causes may be prevented or obviated." The endowments which the Company have created are of two kinds. The one is intended "as maintenance for work in progress in fields of research to be chosen by the worker," the other as reward for actual discovery; the former intention being carried into effect by the establishment of three "Research Scholarships," each of 250*l.* a year, the latter by the appointment of a "Discovery Prize" of 1000*l.*, to be given once in every four years. With a special view to the promotion of pathological study in the United Kingdom and its dependencies, the Scholarships are limited to British subjects who must be under thirty-five years of age; but in all other respects they are entirely open to persons *cujuscunque ordinis sive professionis*. Candidates are expected to state precisely the researches they propose to undertake, and are invited to refer, in support of their applications, to any work they may have in progress or may have published in the same or in any kindred field of study. In the appointment to Scholarships preference will be given to those candidates whose researches are judged likely to result in increase of knowledge of the "*Causation or Preventability of some important Disease or Diseases.*" It is further provided that towards the close of his year of scholarship each scholar shall publish the result of his research or researches either in print, or, if desired, in a lecture to be delivered at Grocers' Hall or elsewhere.

The Quadrennial Discovery Prize is intended to reward original investigations, irrespectively of the country in which they may have been made, which shall have resulted in important additions to exact knowledge in particular (previously defined) subjects. The subject for the first Discovery Prize will be announced in May next, and the award will be made in May, 1887, when a further subject for investigation will be proposed. Any treatise which the candidate may have published, whether in England or in any other country, at any time during the period allowed, will be accepted as a competition-treatise, provided that the author has duly declared himself a candidate. Every treatise must be in print and in the English language, and must bear the name of its author. It seems to be contemplated that some definite problem will always be involved in the subject announced, the solution of which will be considered as the essential condition of success. But if it shall appear that although this has not been accomplished any candidate has made valuable progress towards its accomplishment, or has even incidentally made some discovery of practical importance, the merits of such candidate will be recognised by the award of a part of the sum offered.

Such is the scheme; we think it will be generally regarded as well adapted for the accomplishment of the end proposed. The objections to which it is liable are exclusively those which are applicable to all similar schemes for the encouragement of research by pecuniary endowment.

To the Discovery Prize we attach less value than to the other. In the natural sciences discoveries are usually made by men to whom the prospect of a reward, however munificent, would not be a sufficiently strong reason to induce them to change the course or purpose of their investigations. It is the consideration of this fact, no doubt, which has led the Company, we think very wisely, to determine to accept published researches in competition; but here the difficulty at once arises, for the discovery must relate to a particular question previously announced. How will the selection of this question be made?

It is clearly desirable that on each occasion the problem selected should be one which will certainly meet with its solution during the next four years—and therefore one as to which investigation is already in progress. To anticipate what will and must soon be discovered is half way towards discovery, and consequently demands, on the part of the individuals intrusted with the selection, powers at least equal to those which it is proposed to recognise in the bestowal of the prize. Nothing could be better than that Prof. Tyndall, Mr. Simon and the other scientific advisers of the Company should have the opportunity given them, or rather the duty imposed upon them, of publishing these forecasts of the probable progress of knowledge in relation to the causes of disease, for, even if their prognostications serve no other purpose, they will at least be of use in directing inquiry into the most promising channels.

The more important division of the scheme—that which relates to the scholarships—is open to no objections of the kind referred to above. Its purpose is simple, and the way in which it is proposed to carry it out effectual. It is of course quite as impossible to make a worker of a man by giving him a scholarship as to make a discoverer of him by offering him a prize, but there is this difference between the two cases, that the endowment *enables*, the prize only *rewards*. The scholarships are limited to candidates under thirty-five. Among men of this age who are now working at pathology in this country we may be sure that there are some who are doing so, if one may so express it, at the cost of life, for they are devoting o investigations which certainly will not pay, time which ould otherwise be spent with direct advantage to them-elves; and that there are among such men some at least who are fitted by nature to undertake the work of investigation, and have the additional qualifications afforded by training in scientific methods. Their number is no doubt very inconsiderable, for pathology as a science is of very recent birth. It is the offspring of physiology, and has only just arrived at such a stage of development as to claim an independent position. By reason of its being in this evolutionary condition it happens in pathology, as in all other sciences during the initial stage of their growth, that the more work is done the more is required—the completion of each bit of research only preparing the way for fresh investigations. New methods, new applications of physical, chemical, or physiological knowledge to the problems which relate to the causes of disease, are being brought within reach of the pathological worker every year, but all of these require *work* to make them fruitful. There is therefore not the least reason for apprehending that there will be any difficulty in finding subjects for future inquiries. It is far more doubtful whether the men possessing the qualifications which have been already indicated will be forthcoming. At first, if we are not mistaken, the choice will be very restricted, but each year will bring an accession of strength to the ranks of the competitors, so that if in the first instance the Company should be advised for want of suitable applicants to allow one or more of their scholarships to remain vacant, they will act wisely in delaying the appointment.

We do not think that the difficulty will arise, for the tide has already turned. Practical medicine, which has hitherto been strangely indifferent to the science on which it professes to be founded, is awakening to the importance of scientific investigation of the cause and nature of diseases. Among indications of the change may be mentioned the origin and successful progress of the new "Association for the Advancement of Medicine by Research," which has begun its function by devoting its funds to an inquiry into the etiology of tuberculosis. Another fact of equal moment as indicating the recognition of pathology as a special subject of study, is the intended establishment of a Professorship of the science in the University of Cambridge—an example which will no doubt soon be followed by the sister University. When this shall have been accomplished it may be hoped that the great educational institutions which are attached to Guy's, Bartholomew's, and St. Thomas's Hospitals may be also induced to follow the example of the Worshipful Company of Grocers, by doing something more than they have done hitherto to encourage and provide for "the making of exact researches into the causes of important diseases and the means whereby these causes may be prevented or obviated."

ELEMENTARY METEOROLOGY

Elementary Meteorology. By R. H. Scott, M.A., F.R.S., Secretary to the Meteorological Council. (London: Kegan Paul, Trench, and Co., 1883.)

MR. SCOTT'S aim in this text-book of meteorology is to explain the conditions required for the successful prosecution of the science, and to show in some detail the more prominent of the results which have already been arrived at. The various instruments are figured and described, and the methods of observing detailed at length; and emphasis is laid on the necessity of securing accurate observations, and of paying attention, in making arrangements for observing, to the few simple and obvious principles which underlie the science. An account is then given of the geographical distribution of temperature, pressure, and the other phenomena of meteorology, particularly those which are usually comprised under the heads of climate and weather. The book is a highly successful one, and evinces a full and ready knowledge of the work which has been done by the meteorologists of this and other countries down to the present time, and we must not omit to add that there is an earnest endeavour manifested throughout to give the fullest credit to the first discoverers of the more important facts and principles.

The following extracts, in explanation of hill and valley winds and the distribution of rain and weather on the two sides of a mountain-chain, show the general style of the book: –

"The day wind brings up moisture to the upper strata of the atmosphere, and this is condensed, forming caps on the mountain-tops, and often giving rise to thunderstorms. The night wind, a descending current, carries the moisture with it, and so the highest peaks are oftenest clear in the early morning. The reasons of this rhythmical change in air-motion are to be sought for in the action of heat. In the daytime the air in the valleys and on the lower slopes of the mountains becomes heated and expanded. The isobaric surfaces over such districts rise, and the air so raised has a tendency to flow towards the mountains and up the upper valleys as long as the heat action over the lowlands is maintained. At night the temperature in the valleys falls, and the air lying in them contracts, producing a partial vacuum. This causes the air above to descend, so that a downward current is generated, which lasts all through the night. . . .

"When wind coming in from the sea, and therefore charged with moisture, meets a mountain-chain, it is forced to rise; it is cooled by rising, and made to give up much of the vapour it brings with it in copious rains. The result is that the air is rendered dry and cold. If now the average height of the cols of the chain above the plain country beyond be 4000 feet, the air in its descent may receive an increment of temperature of over 20°·0, and as at the same time its capacity for containing moisture will be increased, it will be felt as a dry hot wind. This is the explanation of the characteristics of the Föhn of Switzerland."

This gives the true explanation of the increased humidity observed during the hottest hours of the day on the Faulhorn and similar elevated situations.

A long extract is given (pp. 269-275) from Laughton's "Physical Geography," summarising the broad features of atmospherical circulation as exemplified by the trades and anti-trades, in which it is stated that in both hemispheres to the north or south of the parallel of 30° or 40° a strong westerly wind blows with great constancy all round the world;—and that, alike in the Atlantic and Pacific; in North America, west of the Rocky Mountains; in the Eastern States; in European Russia and Germany, and in Northern Asia, there is found the same predominance of westerly winds. A more decided objection might have been made to the above view than by stating that the winds of the temperate zone and of the higher latitudes seem to be regulated by the distribution of pressure. Laughton's statement might possibly be accepted if we had before us little more than Horsburgh and the other directories of the navigator. In the north-west of Iceland observations show on the mean of the year 212 days when the wind blows from some easterly point, and only 71 days when it blows from any westerly point, and these prevailing winds of Iceland are essentially typical of the winds of an extensive region of the north. The cyclonic and anticyclonic systems of winds observed on the surface of the earth in connection with the well-known seasonal areas of low and high pressure are not merely surface winds, but extend to a considerable height in the atmosphere. This is evident from the consideration that in winter, pressure is 1·115 inch higher in Siberia than in Iceland, and in summer 0·860 inch higher in the Atlantic than in the south-west of the Punjaub in the same latitudes, and that great disturbances of the equilibrium of the atmosphere must necessarily obtain to very great heights. It therefore follows that over large portions of the northern hemisphere gradients for prevailing westerly winds cannot be formed within many thousand feet of the earth's surface.

Mohn's happy classification of thunderstorms into heat thunderstorms and cyclonic thunderstorms is adopted and illustrated. The former is the type which predominates in summer and in hot climates, while the latter are characteristic of our Atlantic coasts, Iceland, and Norway, and are a not infrequent accompaniment of cyclonic disturbances. Cyclonic thunderstorms have their maximum period in winter, and though they occur at all hours of the day, yet long-continued observations show a distinct diurnal period having the maximum during the night; and in some regions so strongly marked is this phase that, of the twenty-three cyclonic thunderstorms which occurred in Iceland in fourteen years, only three took place at an hour of the day when the sun was above the horizon. On the other hand, heat thunderstorms are most frequent during the hottest period of the day, or during the early afternoon.

Sheet lightning and the so-called summer or heat lightning are stated to be nothing else than the reflection of, or the illumination produced by, distant electrical discharges. This opinion, so long and generally entertained, is not supported by observation. At Oxford, during the twenty-four years ending 1876, the following are the number of times which thunder, with or without lightning, and lightning unaccompanied with thunder, have been recorded during May, June, July, and August, from 3 p.m. to 4 a.m.:—

	Thunder.	Lightning.		Thunder	Lightning.
3–4 p.m.	17	0	10–11 p.m.	12	13
4–5 ,,	27	1	11–mid.	8	30
5–6 ,,	29	0	mid.–1 a.m.	7	27
6–7 ,,	21	0	1–2 ,,	11	27
7–8 ,,	21	2	2–3 ,,	12	10
8–9 ,,	8	1	3–4 ,,	3	3
9–10 ,,	7	7			

Thus at Oxford the hours of maximum occurrence of thunder is from 4 to 6 of the afternoon; but the hour of maximum occurrence of sheet lightning or heat lightning is delayed till about midnight. These different times, but above all the larger number of cases of heat lightning over thunder about midnight, which are nearly as 7 to 2, proves that a large proportion of these cases of heat lightning at Oxford were not the reflection of distant electrical discharges or thunderstorms, and this conclusion is amply confirmed by similar observations made in other parts of the globe. A very large number of these cases of sheet lightning at Oxford are, as suggested by Prof. Loomis in 1868, due to the escape of the electricity of the clouds in flashes so feeble that they produce no audible sound, and they occur when the air being very moist offers just sufficient resistance to the passage of the electricity to develop a feeble light.

SALVADORI'S PAPUAN ORNITHOLOGY

Ornitologia della Papuasia e delle Molucche, di Tommaso Salvadori. Parte terza. 4to. pp. 597. (Torino, 1882.)

THE completion of the third and concluding portion of Count Salvadori's great work upon the Birds of New Guinea and the adjoining Islands is an event that should be duly chronicled. We have already spoken of the plan of this great undertaking, and of the excellent way in which it has been carried out, in our notices of the preceding volumes (see NATURE, vol. xxiii. p. 240, and vol. xxiv. p. 603). We will now say a few words upon the general results arrived at.

The ground covered by the present work embraces, it must be recollected, the whole of the northern portion of the great Australian region. The mainland of this district is New Guinea, but it also contains the islands of the Moluccan Archipelago up to "Wallace's Line," besides various groups situated to the east and south-east of New Guinea, and extending as far as the Solomon Islands. In the "Papuan Sub-region," as it is generally called, thus constituted, it will be evident that variation must necessarily play a much more important part than in the solid Continent of Australia. Not only do the species isolated in the different islands obtain a better chance for the exaggeration of their peculiarities (as has been so well shown by Mr. Wallace in his "Island Life"), but in the mainland of New Guinea we find mountains reaching to such an altitude as to cause the presence of a very different fauna from that of the adjoining lowlands. From these two causes it would be naturally expected that the ornithology of the Papuan Sub-region would be more rich in species than that of Australia proper. And such, indeed, is shown to be the case by the completion of Count Salvadori's work, whereby the first summary has been effected of the Papuan Ornis, since recent researches have revealed to us its luxuriance. In Mr. Gould's great work upon the Birds of Australia little more than 700 species of birds are given as inhabitants of the whole of that great continent. By Count Salvadori's volumes, we find that 1028 species are already known to us from the Papuan Sub-region, and, as we all know, a very large portion of New Guinea and many of the adjacent islands are still *terra incognita*. Much therefore remains to be added to the Papuan Avi-fauna, whilst in Australia the subject is comparatively exhausted.

Taking a general survey of the forms of Papuan bird life, we see at once how nearly akin it is to that of Australia. Recent researches especially have shown that nearly all the peculiar forms of the Australian Ornis have their representatives in the Papuan Sub-region. Some of these forms, however (for example, the Paradise-Birds and the Cassowaries), are much better represented in the Papuan Islands than on the Australian Continent, and the Papuan Islands must be regarded as their original home, whence they have sent forth stragglers into the Southern Continent.

Such general facts as regards the distribution of bird life in the Australian Region may be easily gathered from an inspection of the contents of the present work. But our author, we are glad to see, promises us to put them forward in his own shape, in an "Introduction to the Ornithology of Papuasia and the Moluccas," which he is now preparing. In this supplementary volume will be likewise given chapters on the history and bibliography of the subject, and a chart to illustrate its somewhat complicated geography. Count Salvadori is evidently determined to spare no trouble in order to render complete the results of his eight years' hard labour on the Birds of Papuasia and the Moluccas.

OUR BOOK SHELF

Cutting Tools Worked by Hand and Machine. By Robert H. Smith, M.I.M.E. (London: Cassell, Petter, Galpin, and Co., 1882.)

STUDENTS of mechanical engineering, and more especially those who study machine tool construction, have up to the present time found it very hard to obtain a suitable text-book relating to the theoretical part of the subject; hitherto almost the only books relating to it have been published in Germany.

This work comes to hand at a time when the want of such a work is much felt, and students attending mechanical engineering classes will find that it will help them considerably in understanding the construction, theoretically and practically, of the machines dealt with. The author in his preface states distinctly that he does not intend the book to be a descriptive treatise on tools, nor does he refer to all the different cutting tools in use, but he has happily chosen the more important machines, and gives a very full description and illustration of each. The subject of driving power is dealt with and fully explained, and results of experiments carried out by the author on the subject are carefully arranged in tables.

In the first chapters cutting tools for wood are discussed, the wedge action of any cutting tool being clearly described and illustrated; also the method of grinding and setting edge tools, frequently a very difficult task for beginners to accomplish. He also gives the results of experiments carried out by himself on the power required to be exerted through certain tools when doing a fixed amount of work, an interesting subject from a theoretical point of view.

The chapter on chipping-chisels and hand-planes fully explains the action and construction of the several tools, the different angles of the cutting-edge of cold chisels are shown, and the author points out the reasons for varying the angle according to the quality of the metal. The whole chapter goes into the subject practically, the explanations being clear and to the point. The next chapters deal more especially with wood-working machinery. The variety of teeth used in the different kinds of saws, including inserted teeth, are amply illustrated, the important matter of setting the teeth being fully explained, with experiments showing the power absorbed in driving the different saws, this also being usefully arranged in tables; after which the author goes on to explain the different machines used in working the metals, milling machinery having its full share of the text. The cutting speed and rate of feed for milling-cutters is gone into, and in the latter part of the chapter the milling-cutters themselves are dealt with.

Chapter V. relates to the various methods of planing metals, and concludes by dealing with the behaviour and action of the larger machine tools, including planing, shaping, and slotting machines, all of which are illustrated.

The following chapters go fully and minutely into the construction and working of the lathe, perhaps the most important of all the machines in a workshop; describe its various uses, both for hand-turning and wood, and the mechanical slide-rest for metals. A screw-cutting and surfacing lathe is illustrated, and all its different motions explained. It is impossible here to do justice to these chapters on the lathe and turning in general. The student will find the time well employed if he studies them carefully, the author evidently being well acquainted with the practical working of this all-important machine.

The remaining chapters are occupied with drilling, boring, and the necessary machines for carrying out the same; a variety of drills are shown, and their different uses explained. The drilling machine, its construction, and various arrangements of feed gear are illustrated and concisely shown.

The book concludes with shearing and punching machines. Various illustrations are given, including Whitworth's driving gear for the same, and an illustration of Tweddle's hydraulic shearing and punching machine.

As a treatise on cutting-tools, for wood and iron, this work will be found extremely useful to engineers generally; moreover, there is a good deal of original information that will be found interesting to experienced toolmakers. The classification of the machinery is decidedly good, and the descriptions are so simple as to be easily understood by the uninitiated. It is impossible to study the book without at once finding that the author is completely master of his subject. Of course there is no doubt that practical working is essential to perfection in any branch of engineering; yet the student who is unable to attain such practical knowledge will obtain a good insight into the construction and the uses of the various machines and tools in their connection.

The author certainly seems to have omitted a matter of great importance in tool-making, namely, that of tempering and hardening the cutting-tools. There is little doubt that most of the failures arising in wood and iron-working machinery are due to tools not being properly hardened. A chapter or two devoted to the subject would have been of great service.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Metamorphic Origin of Granite.—Prehistoric "Giants"

I HAVE for some time intended to send you a few notes on two matters, both connected with geology, though very different in kind. In NATURE, vol. xxvii. p. 121, there was an interesting paper by Mr. Geikie on the metamorphic origin of granite and on the crystalline schists. Last autumn I became satisfied of a conclusion which I had long suspected—that the large granitic district in the Ross of Mull, adjacent to the Island of Iona, is a great mass of granite formed by the metamorphism of an old stratified deposit belonging to crystalline schists. It is well known to geologists that the Isle of Mull consists almost entirely of the series of (Tertiary) volcanic rocks which have been admirably described by Prof. Judd. These traps, tuffs, and lavas rest in some places on chalk, and where the chalk had been previously denuded they rest on Oolitic and Liassic beds. Older rocks, belonging, I think, to the Cambrian series, appear at one place subjacent to the traps. But the limit of all these volcanic rocks to the south-west is sharply defined by the deep bay and harbour of Bunessan, called Loch Laigh. As we enter that loch in a boat, we have on our left the trap headland of Ardtun, where I found the Tertiary leaf-bed many years ago, and on the right a headland of massive red granite. But the shores at the end or head of the bay, including all the hills above the village of Bunessan, are neither trap nor granite, but are composed of the regular crystalline mica schists which constitute the great bulk of the county of Argyll. This is the only part of Mull, so far as I know, where these rocks appear. They stretch right across the long promontory of the Ross to the southern shore. At the head of Loch Laigh they are more highly crystalline than in most parts of the mainland of the county. Very fine crystals of tourmaline have been found above Bunessan, and the schists near the new pier are highly micaceous and in some places full of coarse garnets. These schists dip at a high angle, and indeed are in some places nearly perpendicular. On the southern coast of the promontory (which is here very narrow) they occupy a considerable space between the traps which terminate on the farm of Scoor, and the granite which begins on the farm of Ardalanish. The point of contact between these schists and the granite is obscured at the head of Loch Laigh, and I have not visited it on the southern shore. But the point of most interest will be found in the granitic headland which forms the south-western shore of Loch Laigh. Along part of this shore the granitic masses at the top of the hill have all the appearance of standing upon legs. These legs at a little distance seem granitic, and although they have a suspicious appearance of tilted strata, I had passed them over and over again under a general impression that they were nothing but granite divided by unusually narrow lines of cleavage. On examining them, however, carefully, in August, 1882, I found that they are (in my judgment) beyond all doubt crystalline bedded schists exhibiting the phenomena of metamorphism in the most curious and instructive form. The metamorphic action has often segregated the mineral constituents of the old sedimentary rock in bands transverse to the line of bedding, so that in one stratum we have bands of pure quartzite and of hornblende gneiss, between bands of granitoid and of pure granitic composition. These beds pass up without a break into the amorphous granite of the great bulk of the hill; and how pure and typical that granite will be acknowledged when I add that the columns of the memorial to the Prince Consort in Hyde Park are made of it. Since discovering this passage I have found some other spots on the coast where the relation of the two rocks to each other is well seen. The best is on the deeply indented shore of the farm of *Knockvoligan*, behind the Island of "Gilan Giraid," on which the Northern Light Commissioners have placed their establishment in the Sound of Iona. Boats can be hired at Iona, and at high tide there is a beautiful passage behind Gilan Giraid to the shore I refer to. There a dark hornblendic gneiss will be seen underlying, involved in, and passing into granite in every form of complication and variety. An interesting question arises as to the horizon to which this hornblendic rock belongs. As Iona belongs unquestionably, as I believe, to the Laurentian series, and the Bunessan schists to the metamorphosed Silurian, the sub-granite gneiss which intervenes may be assigned to either the one or the other. My impression is that it represents some of those gneissose beds of the Silurian series which are highly developed in Sutherland, and lie high above the "fundamental" or Laurentian gneiss, so well known in that county. I should be very glad if some competent geologist could investigate this district of the Ross of Mull, and could confirm or check my observations.

Turning now to the other subject. I have been surprised to see in the English scientific journals no notice taken of the very remarkable discovery reported from the Californian Academy of Science in a paper communicated to that body by Charles Drayton Gibbs, C.E., on the discovery of a great number of (apparently) human footprints of a gigantic size in the State of Nevada. It appears that in building the State Prison, near Carson City, the capital of that State, there was occasion to cut into a rock composed of alternate layers of sandstone and clay.

On several of the clay floors exposed in this operation great numbers of tracks of all sorts of animals have been exposed. These tracks include footprints of the mammoth or of some animal like it, of some smaller quadrupeds apparently canine and feline, and of numerous birds. Associated with these are repeated tracks of footsteps, which all who have seen are agreed can be the footsteps of no other animal than man, and the engravings and photographs which accompany the paper leave no doubt on the mind of any one who sees them. The most remarkable circumstance characterising them is their great size. In one case there are thirteen footprints measuring 19 inches in length by 8 inches wide at the ball, and 6 inches at the heel. In another case the footprints are 21 inches long by 7 inches wide. There are others of a smaller size, possibly those of women. One track has fourteen footprints 18 inches long. The distance between the footprints constituting a "step" varies from 3 feet 3 inches to 2 feet 3 inches and 2 feet 8 inches, whilst the distance between the consecutive prints of the *same foot* constituting a "pace" varies from 6 feet 6 inches to 4 feet 6 inches. In none of the footprints of the deposit are the toes or claws of animals marked. As regards the beasts, this is probably due to the "slushy" state of the mud when the tracks were made. But in the case of the human footprints it is probably due to the use of some kind of shoe or mocassin.

I need not say that so far as the geological horizon is concerned this discovery does not carry the existence of man beyond the Quaternary Mammalia, with which it has long been pretty clear that he was associated in prehistoric times. Nevertheless it is, if confirmed, a highly remarkable discovery, especially as connected with the curious intimation so concisely made in the Jewish Scriptures, "And there were giants in those days." Hitherto, so far as I know, the remains of prehistoric man, so far as hitherto discovered, have not revealed anything abnormal in point of size. It is just possible that the slippery and yielding nature of the muddy lacustrine shore on which the tracks were made may have partly occasioned the apparent size. But the photographs and engravings exhibit them as very sharp and "clean cut." Professional Indian trackers have been employed to examine the tracks, and none of them seem to have the smallest doubt as to the footprints being human. ARGYLL

Cannes, April 14

P.S.—The paper was sent to me by my son, the Governor-General of Canada, a few weeks ago.

"The Ether and its Functions"

IN NATURE, vol. xxvii. pp. 304, 328, is a reprint of a lecture delivered by Dr. Oliver Lodge in December 28, 1882, at the London Institution, on "The Ether and its Functions." As this happens to be a subject to which I have devoted special attention, I would beg to offer a few remarks, also as my name is alluded to in the article.

The repudiation of the assumption of "action at a distance" in the first part of the lecture, coupled with the ingenious arguments by which its baselessness is exhibited, will no doubt be encouraging to all those who favour the advance of knowledge. But that portion of the lecture dealing with the constitution of the ether (and which assumes it to be non-molecular) is to my mind disappointing, as it looks like a step backwards to suppose the ether to be something essentially different from ordinary matter, while on all sides the simple opinion of the "unity of matter" has been making progress. I will quote the passage more especially relating to this point, viz. :—

"As far as we know, it (the ether) appears to be a perfectly homogeneous incompressible continuous body, incapable of being resolved into simple elements or atoms; it is in fact continuous, not molecular. There is no other body of which we can say this, and hence the properties of ether must be somewhat different from those of ordinary matter" (p. 305).

It will be admitted that clearness is a first desideratum in a theory. It appears difficult to see how an "*incompressible*" body is to transmit waves.[1] A remark of Maxwell's in his paper "On the Dynamical Theory of Gases" has some bearing on this point, viz. : "The properties of a body supposed to be a uniform plenum [*i.e.* not molecular] may be affirmed dogmatically, but cannot be explained mathematically" (*Phil. Trans.*, 1867, p. 49). Moreover, it seems hard to reconcile the fact that Sir W. Thomson's theory of the constitution of matter is apparently adopted or favoured in the lecture, and yet at the same time the molecular or atomic nature of the ether is repudiated. But is not Sir W. Thomson's theory of matter essentially an atomic theory? The incompressible fluid outside the vortex atoms cannot serve as the ether, or this seems an impossibility. Maxwell, for example, remarks in relation to this point, viz. : "The primitive fluid [*i.e.* the fluid exterior to the atoms] entirely eludes our perceptions"[1] (see "Encyc. Brit.," article "Atom," p. 45). The ether, however, does not entirely elude our perceptions, but is very distinctly felt in the beating of the waves of light upon the eye. It appears, therefore, that if the ether is to affect the senses at all it must consist of atoms or molecules (doubtless very much smaller than those of gross matter, a difference in *degree* but not a difference in *kind*). It may be noted in passing here how often notoriously has the error of mistaking a mere difference in *degree* for a difference in *kind or essence* been made in the history of science, and the correction of this error with the correlation and simplication of views attendant on its removal marks one of the chief stages of our progress. The theory of evolution abolished this error in regard to the animal world; its abolition in regard to the universe of matter is equally demanded. One satisfaction that Sir W. Thomson's theory of matter brings, consists perhaps in the fact that it does not overthrow our old conceptions as to the atomic constitution of matter so firmly built up by the able reasoners of the past, including Lucretius and Newton—and which has produced such great results for science. The Thomsonian view goes rather to confirm the atomic theory and to establish its truth by explaining in addition how an atom can be *elastic* and yet indestructible. Let us not deviate from the well-tried ground of the atomic constitution of matter, already won with so much labour, unless we are forced to do so, and let us work towards the great generalisation of the Unity of Matter and of Energy.

London, April S. TOLVER PRESTON

P.S.—My views regarding the Matter of Space (the result of many years of thought and study) are contained in various scattered papers, references to the chief of which may be conveniently given here, viz. *Philosophical Magazine*, September and November, 1877, February, 1878; NATURE, January 15,[2] 1880; March 17, 1881; March 20, 1879; *Philosophical Magazine*, August, 1879, November, 1880; April and May, 1880, &c., &c. Also a little book, "Physics of the Ether" (E. and F. N. Spon), was published in 1875 as a first imperfect essay on the subject. The above papers include an atomic theory of the ether, capable of affording a simple and natural explanation of gravitation without the aid of "ultramundane corpuscules" [*i.e.* without the supply of any energy or matter at all from outside the bounds of the visible universe]. Dr. Lodge seems to admit that his premises cannot explain gravitation. But is not the elucidation of gravitation (which may be called the primary physical effect in the universe) one of the first requirements of any theory of the constitution of the Matter of Space? A more concise summary of my views (with additions and developments, the work of recent years) regarding the relations of the Matter of Space to ordinary matter and to the local fluctuating changes taking place in the universe, may be found in the forthcoming volume of the *Transactions of the Vienna Academy of Sciences*, to which they have been communicated by Prof. Ludwig Boltzmann of Graz.

"Krao"

SOME two months ago there appeared in NATURE (vol. xxvii. p. 245) certain statements about "Krao," the Siamese hairy child, which with your leave I would venture to correct. Krao's parents are both Siamese, not Laos; they are both still living in this city; neither of them presents any special peculiarity; they have other children still living, and also showing no special peculiarities; Krao, it is true, was not born in Bangkok, but in a village between this and the sea, her parents having a little time before her birth run away from their master, but coming back after the event. Siamese is of course Krao's native language;

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[1] For it seems apparent that an incompressible, non-molecular, "frictionless" liquid could not have wave-energy imparted to it at all, or a hot substance could not emit light or heat in such a medium. Moreover let us (in a spirit of fair argument) take a representation of one of the most commonplace effects in physics, say an explosion of gunpowder. Then the assumption of "action at a distance" being rejected, there is (admittedly) no more playful building of castles in the air out of "force," or no store of phantom energy to get the motion ("explosion") from. The motion therefore must inevitably come from the matter of space, or from a set of particles or atoms already in motion in space in their normal state. How is a non-molecular, *frictionless* liquid to lay hold (as it were) or act upon the molecules of gunpowder and put them in motion? Is not the objection conclusive though elementary?

[1] A clear and able exposition of the relation of Sir W. Thomson's theory of matter to the old-established atomic theory may be found in a paper on "The Atomic Theory of Lucretius," by an anonymous author in the *North British Review* for March, 1868.

[2] This paper includes a corpuscular theory of light consistent with the main principles of the undulatory theory—*not* therefore an *emission* theory.

in her short journey up country with Mr. Bock, she, being an intelligent child, picked up a few words of Laos; the joints of her arms and fingers possess, it is true, according to European ideas great flexibility, but really they have it to no greater degree than those of ordinary Siamese; it is also true that she is able to use her toes, grasping things between the big toe and the next one in a way that is surprising and amusing to Europeans, but this is a faculty which all Siamese, being a barefooted people, possess to a greater or less degree; the child was looked upon here as even a greater natural curiosity than she is considered to be in England, her parents being in the habit of taking her about and showing her for a small reward, and the price they obtained for her (in native currency equal to 60*l.*) being twice that of an ordinary child of the same age. A strange mistake has been made about the child's name, "Krao" being merely the Siamese name for whiskers, a very natural nickname for the child to obtain. As far as I can ascertain from those who knew the child well, she is endowed with the average intelligence of Siamese children of her age and class, and beyond her abnormal hairiness presents no peculiarity.

To sum up, "she is," as you rightly remarked, "merely a *lusus naturæ*, or a sport, possessed rather of a pathological than an anthropological interest. I may add that I have carefully verified all the foregoing statements. A RESIDENT

Bangkok, Siam, March 3

[From information that has since reached me I am able fully to confirm the particulars here supplied by "A Resident."—A. H. K.]

Singing, Speaking, and Stammering

IN NATURE, vol. xxvii. p. 532, in the report of Dr. Stone's lecture on "Singing, Speaking, and Stammering," there appears a *Classification of Vowels*, which is described as an abstract of Mr. Melville Bell's scheme. I should like, however, to point out that the system which Mr. Bell has advocated for the last fifteen years is hardly represented in the *Classification* referred to. On turning to p. 63 of Mr. Bell's "Sounds and their Relations," which is a new exposition of "Visible Speech," it will be seen that the vowels *Ir* and *Ah* are not described as labio-lingual, and that the threefold arrangement of the vowels as lingual, labio-lingual, and labial is abandoned as incorrect. The lecturer does not appear to have mentioned the phonetic researches of Mr. A. J. Ellis and Mr. Henry Sweet. In many important points, however, they supplement the system of Mr. Bell, and their works cannot be overlooked in the scientific study either of etymology or pronunciation. The student of language can hardly do better than begin with Mr. Ellis's "Speech in Song," and Mr. Sweet's "Handbook of Phonetics." JAMES LECKY

5, Alexandra Road, Wimbledon, S.W., April 10

THE classification of vowels to which Mr. Lecky refers is taken from Mr. Melville Bell's "Principles of Elocution," which I obtained with much difficulty from a publisher in Salem, Massachusetts. It is dated 1878, and may, I suppose, be held to represent the author's system at that date. I am well acquainted with the other works named by Mr. Lecky. W. H. STONE

AS an illustrative instance of the peculiarities akin to stammering, referred to in Mr. Stone's lecture in last week's NATURE (p. 559), I may mention the case of an old Scotch lady whom I knew some years ago, and who was in the habit of interpolating at frequent intervals in her talk the wholly irrelevant words "This that here there ye ken." She herself evidently made use of the words with perfect unconsciousness of their irrelevancy; indeed I doubt whether, if challenged, she would have admitted using them at all. K.

A Curious Case of Ignition

"A CURIOUS case of ignition," quoted in NATURE, vol. xxvii. p. 509, reminds me of a similar circumstance that came under my own observation when serving in H.M. despatch vessel *Psyche*, 1862–66. We were moored "head and stern" in Port Napoleon, Marseilles, on a bright summer day. A strong smell of burning was traced to the saloon skylight. On bursting open the door of the saloon it was found that a scuttle glass (a plano-convex lens) through which the solar rays were admitted and focused on a rep curtain (which was smouldering) had been substituted for a broken one, but through an oversight had not been ground on the plane surface (as is usual). The case was reported by letter, and an order issued to insure all scuttle glasses used in men-of-war for the purpose being ground. BERTRAM GWYNNE

Fibreballs

I HAVE seen balls of vegetable fibre, such as those referred to by Mr. G. H. Darwin in his letter of March 23 (NATURE, vol. xxvii. p. 507), in great abundance on the sea-beach at Cannes; there however they are not spherical like those described by Sir A. Musgrave, but cylindrical, two or three inches in length, finely and closely matted, and all wonderfully similar in appearance. In one place they had been collected and employed, if I remember rightly, to form a kind of wall. Some balls of a similar kind, but more nearly spherical and much coarser in texture, were found, on draining a pond, by Dr. Fitton, and sent by him to Sir J. Herschel, these were three or four inches across, and looked almost like small hedgehogs rolled up. J. H.

Benevolence in Animals

MR. GEO. J. ROMANES, in a lecture delivered in Manchester, March 12, 1879, on "Animal Intelligence," points out the following emotions which resemble human intelligence as occurring in animals below the human species, namely: fear, affection, passionateness, pugnacity, jealousy, sympathy, pride, reverence, emulation, shame, hate, curiosity, revenge, cruelty, emotion of the ludicrous, and emotion of the beautiful, and gives some remarkable instances in support of his statement. To this I can add benevolence on the part of our household cat, who was observed to take out some fish bones from the house to the garden, and, being followed, was seen to have placed them in front of a miserably thin and evidently hungry stranger cat, who was devouring them; not satisfied with that, our cat returned, procured a fresh supply, and repeated its charitable offer, which was apparently as gratefully accepted. This act of benevolence over, our cat returned to its customary dining-place, the scullery, and ate its own dinner off the remainder of the bones, no doubt with additional zest. OSWALD FITCH

Woodend, Fortis Green, N., April 12

The Zodiacal Light (?)

LAST Friday evening about 7 p.m. my attention was called to a peculiar appearance in the western sky. The sun had set not long before. No clouds were visible but one long thin streak, and there were the usual mists near the horizon. Above where the sun might be, a pillar of light faintly red in colour, with soft edges, but fairly well defined, rose vertically from near the horizon to the height of perhaps a few degrees. It did not look like an illuminated cloud nor like rays of light shot up through a cloud, nor like anything local; in fact I am told that it moved northwards with the sun. Was this the zodiacal light, or merely some sunset effect? It began to grow dim about 7.10 p.m., but was visible later than this. J. W. B.

New Kingswood School, Lansdown, Bath, April 10

Braces or Waistband?

THE writer has for the last thirty years dispensed with the use of either braces or a belt, having had his waistcoats made with short elastic straps attached inside and with holes to button on to the trousers like braces, one on each side and a third in front.

They answer as well as braces in conjunction with the ordinary waistband and buckle of the trousers, and the wearer is saved the feeling of strain across the shoulders or round the waist connected with the use of braces or a belt. G. H.

April 13

THE TEACHING OF ELEMENTARY MECHANICS[1]

AT the recent Annual Meeting of the Association for the Improvement of Geometrical Teaching held, as has already been noted in our columns, at University

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[1] Association for the Improvement of Geometrical Teaching, Ninth General Report, January, 1883

College, on January 17, the following statements of work done by the Committees were presented. In *Solid Geometry* no progress had been made in consequence of the serious illness of the secretary (Mr. Merrifield), but the President in his subsequent address remarked that it was hoped that the Committee would meet at an early date and work out, upon the basis of what Mr. Merrifield had done, a syllabus of propositions corresponding to the 11th and 12th Books of Euclid and the simpler geometry of the sphere. In *Higher Plane Geometry* the Committee had revised about half of the syllabus issued in 1879 and had added chapters on the geometry of the triangle and on geometrical maxima and minima (copies were distributed amongst the members present, and have been subsequently circulated). In *Geometrical Conics* the former syllabus had also been revised and continued to the end of the hyperbola (this syllabus is also in the hands of members). In *Elementary Plane Geometry* the proofs of Book I. of the *Syllabus* had been revised, the proofs of Book II. drawn up, and a collection of Exercises on Books I. and II. had been added (the motion in connection with the adoption of these proofs, which was down in the President's name, had to be postponed in consequence of copies not having been circulated before the meeting).[1] Gratifying testimony to the success of the Association's efforts was afforded by the fact recorded in the Council's Report that the copies of the syllabus were all disposed of, and that it was in contemplation to bring out at once a revised edition of the work in accordance with the changes made in the books of proofs.

In addition to the usual routine business, the President closed the morning sitting with some remarks on the teaching of arithmetic. This is a subject the claims of which upon teachers he has at many previous meetings pressed upon his hearers, it being his desire that the teaching should be put upon a sounder footing than it at present in most cases occupies. A true disciple of his old master, De Morgan, he insisted strongly upon the more frequent appeal to reason than to rule. "It seemed to him to be the wrong order to give first the rule and then the reason. Teachers should take particular examples, and work them out with reasons for every step. They should lead up to a rule by a series of examples worked out from common sense, and only when these have been thoroughly grasped should the rule be introduced as a convenient embodiment and summing up of the results attained by the application of reason and common sense." A common habit with boys is to ask, "How am I to do this?" "In his own practice he never answered that question, but he said, 'What does it mean? If you will only find out what it means, then you will know how to do it.'" Another principle he advocated was "that all arithmetical processes should follow the order of thought, according to which numbers are grouped in language. . . . The order of thought in the expression of numbers was from the higher group to the lower, hundreds to tens, tens to units, &c." In this connection he referred to a lecture by the late Mr. Bidder. A reform on the principles he (the President) advocated would, he believed, be very valuable in teaching and in the practical operations of arithmetic. In the natural sciences arithmetic is applied to cases where approximate data only are employed. "Hence it was becoming more and more important that methods of approximation should be carefully and distinctly taught." This led him to enter a protest against the practice, frequent amongst University examiners, of setting in papers for schoolboys, "among the questions on decimal fractions, some examples to be done only by reducing recurring decimals to vulgar fractions, and then working out the result by vulgar fractions. To give prominence to such examples was simply to destroy the notion which a good teacher would have been endeavouring to instil into a boy's mind, that decimal fractions are useful only in general for approximate results. He did not wish to say anything against recurring decimals rightly used and in their proper place." A final point was that he would substitute Horner's process for the extraction of any roots for "the awkward and almost useless special processes usually given for extracting square and cube roots. This he would teach simply as a process; but of course with fair warning to the boy by telling him that he was for once giving him a process which would lead to the desired result, and that it would be a reward of his future mathematical attainments if he could get to the reason of it."

The novel feature, however, in this year's proceedings was the holding of an afternoon sitting, which was wholly devoted to the consideration of the subject of elementary mechanics. This meeting was the outcome of the recent extension of the Association's sphere of action, and proved to demonstration that the said extension had met with the approval of many of our most able physicists. The papers read were three in number: (1) The Teaching of Elementary Mechanics, by Mr. W. H. Besant, F.R.S.; (2) Notes on the Teaching of Elementary Dynamics, by Prof. G. M. Minchin; (3) The Basis of Statics, by Prof. H. Lamb of the University of Adelaide. (1) is remarkable as proceeding from a successful Cambridge "coach," who finds it difficult to emancipate himself "from the ideas and prejudices which are the natural results of an adherence for many years to a special set of books and to a special system of teaching. The fact constantly before us in Cambridge, that mechanics are being studied with a view to success in examinations, tends to make us forget the importance of the practical application to daily life of a knowledge of mechanics, and the temptation is to luxuriate in the flowery and ornamental problems which sometimes form the staple of examination questions," whereas "millions of people must acquire a knowledge of the laws of mechanics, practical or theoretical, or both, who are not going to be tested by a Cambridge examination." It however goes without saying that at present Cambridge methods do exercise a very large influence on the teaching of mechanics throughout the country. In the case of young students and beginners, Mr. Besant considers that the first requisite for a class-room is a set of models and a quantity of machinery (*segnius irritant animos*, &c.). "The handling of systems of pulleys, and experiments with levers and screws, will guide the student, almost unconsciously, to the ideas of the transmission of motions, and of the transmission and multiplication of force. . . . Then, again, experiments with falling bodies, and with an Attwood's machine, will illustrate the ideas of uniform motion and of accelerated motion, and generally of the action of gravity. . . . For many students this kind of experimental teaching will probably be sufficient for the work of their lives, and it will be certainly educationally useful." The Cambridge practice has been to treat the subjects of statics and dynamics separately, and to take statics first; and the teaching is so limited that the ordinary Bachelor of Arts, whose reading has been limited to statics alone, "is sent out into the world without any perception of the laws of motion, and without any knowledge of the elementary deductions from those laws, which are necessary requisites for a true appreciation of a vast range of natural phenomena." Passing next in review the change of nomenclature and of treatment inaugurated by Professors Thomson and Tait, and the late Prof. Clerk Maxwell, Mr. Besant records his opinion that Duchayla's proof is "forced and unnatural," and causes a considerable waste of time. His wish is that the examiners should have greater freedom of action. He would, following the lead of the above-named eminent physicists, commence with a study of the elementary parts of kinematics, to include "the ideas and measures of velocity and acceleration, the parallelogram of velocities, and the parallelogram of accelerations, the motion of a point with a constant acceleration, and the acceleration of a point moving uniformly in a circle." Then would come "falling bodies and projectiles." From these particular cases the student will get a general idea of the action of force, and so be prepared for a study of the laws of motion, and of the deductions from these laws. "One of the first of these is the parallelogram of forces, and I am convinced by actual experiment of the ease with which that mode of proof is appreciated by a beginner. The perception of the physical independence of forces, which is really the qualitative part of the second law of motion, is not a serious difficulty to the majority of beginners in mechanics, and from this principle, with the aid of the parallelogram of velocities, the parallelogram of forces is developed easily and naturally." Next may be taken the mechanical powers and simple cases of equilibrium of bodies and systems of rods, a statement of the laws of friction, and the determination of the centres of gravity of bodies and systems. Mr. Besant also laid great stress upon graphical modes of solution, referring to Mr. Minchin's work on Statics for numerous examples in these methods. "The discussion of the theory of moments of forces would naturally lead up to the idea of a couple and to the transformation and composition of couples." The pupil might then proceed to the impact of balls on each other: "The easiest method for every one . . . is to assume the invariability of momentum, and the constancy of the ratio of the relative velocities before and after impact. The consideration of the action of the forces of compression and of restitution is a more difficult idea, and should be deferred to a later stage of the student's progress. In the discussion of these points the idea of work and of kinetic and potential energy may be introduced and illustrated, gradually leading up to the statement of the general principle of energy." Dwelling upon the wonderful results that accompany the employment of this general principle, "the very concentrated essence of science," which in elementary mechanics "widens the path and shortens the road, and reduces to simple forms of thought many problems which used to be reckoned as belonging to advanced regions of the higher mechanics, and as depending for their solution on the complicated machinery of analytical process," the paper alluded to the fact that it was only as recently as 1877 that this principle of energy appeared in Part I. of the Tripos Examination—a result mainly to be attributed, we believe, to Prof. Clerk Maxwell's advocacy of it. The principle is carefully laid down and discussed in "On Matter and Motion," as well as elaborately discussed in Thomson and Tait's "Natural Philosophy," and in many recent elementary treatises. With such views of its importance, we are not surprised to find Mr. Besant pleading for the introduction of the idea of energy as early as possible, and that every effort should be made to "illustrate the idea by means of simple cases and so to lead the student upwards, by gradual steps, to the conception of the most important principle which lies at the root of all modern science." Another point on which the paper touches is that "ill-chosen technical terms are likely to propagate erroneous ideas and confusion of thought," and reference is made to recent remarks on the use of the word "force." Then coming to the question of a syllabus of mechanics, Mr. Besant remarks that "it will be a matter of supreme importance to discuss the definitions and axioms of the subject," and instances a common definition of the word "vertical"—that it is the line in which a stone moves when let fall. The paper closes with a few general remarks on the value of some branches of scientific study as an education, from which we select the two following extracts:—"The safest and wisest plan seems to be to let every man, who wishes to make research in physics, find out for himself the kinds of tools which he wants, and then learn as much of the use of those tools as may be necessary." "The elementary subjects, such as mechanics and astronomy, are of more educational value to the majority of students than the higher regions of science, and only a select few should be encouraged to spend much of their time in such advanced forms of study."

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[1] A special meeting for the purpose of considering the postponed motion was held at University College on the evening of March 20, at which the "proofs" were adopted and their publication sanctioned.

(2) In this dynamics includes both statics and kinetics. The writer is in favour of continuing the old way of taking statics first, and of then proceeding to kinetics, and argues strongly against the taking the former as a particular case of the latter. Two important advantages, however, of the recent mode of treatment are not ignored, viz. that the student by concentrating his attention on force solely as change of motion, it at once proves for him the fundamental proposition of dynamics, viz. that of the "parallelogram of forces," as an immediate result of the easily admitted parallelogram of velocities ("if for the beginner the choice lay between such proofs as Duchayla's, and none, I should say, 'Assume the proposition'"); and next that "the kinetical method has the very great practical advantage that it makes the student familiar at the outset with the idea of *absolute measures* of force, momentum, energy, &c., such as are used in the C.G.S. system." The notion of *acceleration* is an exceedingly difficult one for beginners, and such a one, as a matter of fact, "is confined to the consideration of acceleration of constant magnitude, and, except in the case of uniform motion in a circle, to the case of acceleration in a constant direction. Thus he gets plenty of exercise in the motion of particles down inclined planes, ... but what idea does our beginner obtain of the acceleration of the motion of a particle revolving uniformly in a circle? Is there not something *primâ facie* very difficult, if not absurd, to him in the statement that any motion which takes place with *uniform velocity* can be accompanied by *acceleration?*" After a few more remarks to the same end, Prof. Minchin says, "So far as my own work in teaching is concerned, I have not a moment's hesitation in saying that the treatment first of kinetics and then of statics as a particular case is to be rejected. So difficult for the mere beginner are the conceptions involved in Newton's second axiom, that three months' work in combating difficulties and removing false impressions would, almost to a certainty, produce a merely negligible amount of positive knowledge." Starting from the kinetical definition of force, and thereby establishing the fundamental proposition ("this does not logically compel us to continue to treat of motion, deducing rest as a particular case"), the writer, after protesting against the too great importance attached to the getting-up of "book-work,"[1] expresses the opinion that the student realises the subject only by incessant application of the principles to particular cases. For this purpose nothing, he believes, is so good as *numerical examples:* and this in contrast to examples dealing with magnitudes as algebraical symbols, and to geometrical examples. "So long as forces are X, Y, Z, and moments are L, M, N, and no particular consideration of the *units* of different quantities is required, they are comfortable enough, but when we have to deal with pounds and foot-pounds, dynes and ergs, the utter unavailability and inutility of their knowledge are made manifest." Another point strongly insisted upon is *the invariable accompaniment of figures constructed accurately to scale with all the examples.* The result should be arrived at by calculations made by means of logarithmic and trigonometrical tables, and also by graphic construction by the aid of the instruments, and on all there should be, when possible, "the perpetual

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[1] "I have met students who could write out paragraph after paragraph of general propositions in statics, and who at the same time (although such might appear to *a priori* reasoners impossible) could not make the faintest attempt to discuss any particular question involving the application of the principles of statics."

exercise of a *common-sense check*." Too much weight may be attached to *graphic statics*, "but real utility is gained by making graphic methods a companion to (though not wholly a substitute for) analysis," and Prof. Minchin would assign a more conspicuous place to them in the text-books than they at present occupy. "Their essential merit consists in their furnishing visibly to the student the whole history of a magnitude throughout a series of variations in its circumstances." Prof. Minchin would also banish such "crude" terms as "power," "weight," in the equilibrium of machines: such forces might be called "efforts" and "resistances." Passing over one or two other subjects, we come to remarks on "illogical methods of teaching"; by such a method is here meant *a process which introduces considerations that are not essentially necessary for the purpose aimed at—considerations that can be seen a priori* to be *irrelevant*. The moral is pointed by the discussion of a question of usual occurrence in the text-books. The student should be able to be *critic of his data*, and "he ought to be taught to recognise clearly the object finally aimed at in any problem, and also to see what he must be given, and what he need not be given, in order to arrive at it." For this purpose Prof. Minchin purposely uses with his students some books which, *both in their data and in their methods*, are full of illogicisms. The finale comes in pointing out the desirability of making the student carefully distinguish between the *weight* of a body and its *mass*, and here he "comes down," if we mistake not, on an episcopal writer of works on dynamics, for a "remarkable misuse of language."

(3) Prof. Lamb's object is to suggest a new basis for the science of statics, and in the course of his paper he attacks certain principles and artifices, as "the transmissibility of force," and (in hydrostatics) the solidification of matter, and also "rigid" bodies. The "point of departure" which he suggests is that "the true and proper basis of statics is to be sought for in the principle of linear and angular momentum. Regarding statics as the doctrine of the equivalence of forces, I would define the word 'equivalent,' and say that two sets of forces are 'equivalent' when, and only when, they produce the same effect on the linear and on the angular momentum of any material system to which they may be applied: *i.e.* when they produce the same rate of change of momentum in any assigned direction, and the same rate of change of moment of momentum about any assigned axis." He believes that on examination the objections arising from the supposed difficulty and abstruseness of this mode of treatment, "will disappear, and that *on the whole* the method will be found to be really much simpler than that at present in vogue. The main difficulty is at the outset."

A brief but interesting discussion followed, enlivened as it was by a friendly passage of arms over the term *force of inertia*.

R. T.

THE CHEMISTRY OF THE PLANTÉ AND FAURE ACCUMULATORS

PART V.

1. *Influence of Strength of Acid*

IN the second part of this communication in NATURE, vol. xxv. p. 461, when treating of the charging of the cell, we pointed out that in the electrolysis of dilute sulphuric acid between lead electrodes, two totally different reactions might be obtained. The positive metal becomes thinly coated with lead sulphate when the current employed is of small density, but with lead peroxide when the density of the current is of greater magnitude. This latter action is, of course, what takes place in the ordinary formation of a Planté battery. The chemical change, therefore, which goes on at the positive electrode is to a certain extent dependent upon the strength of the current. It appeared also of both theoretical and practical interest to determine whether the chemical change was also influenced by the strength of the acid employed. Our experiments consisted in passing a current of uniform strength, about 1 ampere, between electrodes of lead, 12 square inches in size, in varying strengths of sulphuric acid, and estimating in each case the amount of oxygen fixed by the positive electrode. We determined this for successive five minutes of time, and as such actions are not always very uniform, we made in each instance more than one experiment. The results are given in the following table:—

Strength of acid	Expt.	Percentage of oxygen fixed.				
		First 5 mins	Second 5 mins	Third 5 mins.	Fourth 5 mins.	Total
1 to 5	I.	38·1	28·6	28·6	33·3	128·6
	II.	39·5	30·2	25·6	30·2	125·5
1 to 10	I.	43·4	38·7	29·2	34	145·3
	II.	44·1	39·3	29·3	34·9	147·6
1 to 50	I.	48·3	39·6	35·3	22·4	145·6
	II.	46·2	43·9	23	30	143·1
	III.	54	40	35·3	35·5	165
1 to 100	I.	42	38·3	33·9	29·5	143·7
	II.	42·4	40	37·8	35·5	155·7
	III.	51·1	44·2	34·9	34·9	165·1
1 to 500	I.	46·6	32·6	27	27	132·6
	II.	46·4	27	27	18	118·4
1 to 1000	I.	90·6	81·1	76·4	57·5	305·6
	II.	90·8	77	72·3	63·1	303·2

It appears from this that the strong sulphuric acid (1 to 5) is not quite so favourable to the action as the more dilute (1 to 10); but that between this latter proportion and 1 to 500 there is no great difference in the amount of oxygen fixed, and therefore of corrosion of the plate. The appearance of the plate in every instance indicated the formation of only lead peroxide. With sulphuric acid diluted with 1000 parts of water, the amount of oxygen fixed, and therefore of corrosion, was at least doubled, while the chemical action was very different. On parts of the electrode, streaks of a mixture apparently of the yellow and puce-coloured oxides were seen. On other parts a white substance formed and was easily detached, falling in clouds into the liquid. Where this latter action took place, the plate was visibly the most corroded. This white substance gave on analysis SO_4 equivalent to 73·6 per cent. of lead sulphate, suggesting the idea that it was a basic sulphate of the composition $2PbSO_4,PbO$, which would require 73·1 per cent. As the peroxidation of the lead is required, and the corrosion of the plate is to be avoided as much as possible, it is evident that this extremely dilute acid must be avoided. It has already been shown that if the sulphuric acid is entirely removed from solution, as sometimes happens in an accumulator, the lead is simply converted into the hydrated protoxide, and therefore corroded without any good effect.

2. *Function of Hydrogen.*—In the formation of a secondary cell, after the complete reduction of oxide or sulphate to metallic lead, bubbles of hydrogen gas are seen to escape from the lead plate. It has been assumed that a portion of this is occluded by the lead, or in some other way enters into association with it, and it has been

supposed that this hydrogen compound may play an important part in the subsequent production of electromotive force. It therefore appeared desirable to obtain experimental evidence as to whether hydrogen is so absorbed. The process we adopted for this purpose was founded upon the observation of Graham that hydrogen associated with palladium reduced ferri- to ferro-cyanide of potassium, and that generally in the occluded condition the element was more active chemically. We had previously ascertained that hydrogen associated with other elements, as platinum, copper, and carbon, was capable of reducing potassium chlorate to chloride. This method seemed to give trustworthy results, and therefore we applied it in this instance. As the result of several trials, however, we found that the amount of hydrogen associated with the reduced lead was almost inappreciable. Small as this quantity is, however, it is by no means impossible that it may be the cause of the exceedingly high electromotive force observed for the first few moments, on joining up a completely formed cell immediately after its removal from the circuit of the charging current. This, however, may be due, as Planté imagined, to the gaseous hydrogen itself. The principal if not the only function of the hydrogen of the water or sulphuric acid is therefore that of reducing the lead compounds.

By a totally different process Prof. Frankland has very recently come to the same conclusion as ourselves in regard to the exceedingly small amount of occluded hydrogen.

3. *Evolution of Oxygen from the Peroxide Plate.*—Planté noticed a small escape of gas from the negative plate of his cell immediately after its removal from the influence of the charging current. This he attributed to a decomposition of water by means of local circuits between the peroxide and the subjacent lead plate in contact with it.

The explanation we gave in our first paper (NATURE, vol. xxv. p. 221) of the local action which goes on at the negative plate does not account for the escape of any gas—either oxygen or hydrogen. We therefore thought it of interest to ascertain the nature, and if possible the origin, of the gas noticed by Planté.

We found that the escape of gas from a Planté negative plate was very slight, and soon ceased; but we observed that it became much more pronounced when the temperature of the electrolytic liquid was raised. In order to get a sufficient quantity of the gas for examination, we prepared a negative plate according to the procedure of Faure, and then heated it in dilute acid, with an arrangement for collecting the gas as it was evolved. The amount of gas was still very small in comparison with that of the peroxide, but a sufficient quantity was collected to enable us to ascertain that it was oxygen. We next heated some of the electrolytic peroxide apart from the lead plate, and again noticed a similar evolution of gas, which was also found to be oxygen. This shows, therefore, that it was not a result of local action.

The gas has generally some odour of ozone, and, on testing the dilute acid between the plates of a Planté cell, we always found traces of something that bleached permanganate of potassium, and which might be either ozone or peroxide of hydrogen.

The origin of the gas noticed by Planté may be easily attributed to the oxygen which always passes off in quantity from the peroxide plate during the process of "formation." It is only necessary to suppose that some of this becomes condensed on the peroxide, and is gradually eliminated from it when the surrounding conditions are changed. But the matter is capable of another explanation. If peroxide of hydrogen be really formed in the liquid, it will exert its well-known influence on higher oxides, namely, that of reducing them and itself at the same time. As a matter of fact, if peroxide of lead is dropped into peroxide of hydrogen oxygen is evolved.

4. *Temperature and Local Action.*—Planté has recently pointed out that an elevation of temperature facilitates the formation of his secondary cell (*Comptes Rendus*, August, 1882). The character of the chemical changes which took place at the negative plate led us to think it exceedingly probable that this increase in the rate of formation arose from an augmentation in the amount of local action. Experiment showed such to be the case. Pairs of similar negative plates on Planté's model were allowed to remain in repose at 11° C. and 50° C. respectively, and the formation of the white sulphate was visibly more rapid at the higher than at the lower temperature. The same is also true with negative plates prepared by Faure's process. Thus we found that two similar plates kept in repose for an hour, the one at 11° C. and the other at 50° C., formed by local action 2·6 and 7·4 per cent. of lead sulphate respectively. On two other plates the proportions were 7·6 and 9·5 per cent. respectively. These observations of course by no means exclude the idea that an increase of temperature may facilitate the other chemical changes that take place in the formation of a lead and lead-oxide cell.

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THE LION AT REST

THE illustration which we give on next page, from *La Nature*, is after a photograph of one of the lions in the Zoological Gardens, London. This photograph may be regarded as one of the numerous triumphs of instantaneous photography, valuable both to art and science. The original was rephotographed in Paris directly on wood, by means of a special collodion, at present much used. This has assured a perfectly faithful reproduction of the original, exhibiting all the characteristic details of the lion at rest. The illustration tells its own story.

ON THE RELATIONS OF THE FIG AND THE CAPRIFIG[1]

THE relations of the fig and the caprifig, or the cultivated varieties of fig and the wild form of the Mediterranean region, have been variously explained by different writers, including those recent ones whose works are cited below. Intimately connected with this question is the process of caprification, so often and so circumstantially described by ancient and modern authors, amongst the later of whom we may mention Gasparrini. Graf Solms-Laubach's essay is an elaborate work of upwards of one hundred quarto pages, embodying the results of much research. Not the least interesting part is that treating of caprification, or perhaps we might say the manner in which fertilisation is effected. The author regards the cultivated edible varieties of fig as constituting one race, and the wild caprifig as another race of one and the same species; and the former as having developed from the latter under the influences of cultivation. Gasparrini, on the contrary, described them as distinct genera. Dr. Fritz Muller takes an altogether different view. He says it appears to him far more likely that the fig and caprifig represent, as Linnæus supposed, different forms, the male and the female, belonging together, and not proceeding the one from the other, but which developed side by side, before any cultivation, through natural selection. An examination of the facts adduced by Solms-Laubach himself seems to point to the correctness of Muller's view. But we will set them forth as briefly as possible, leaving the reader to judge for himself. The responsibility of their accuracy rests with the author whom we are quoting. It

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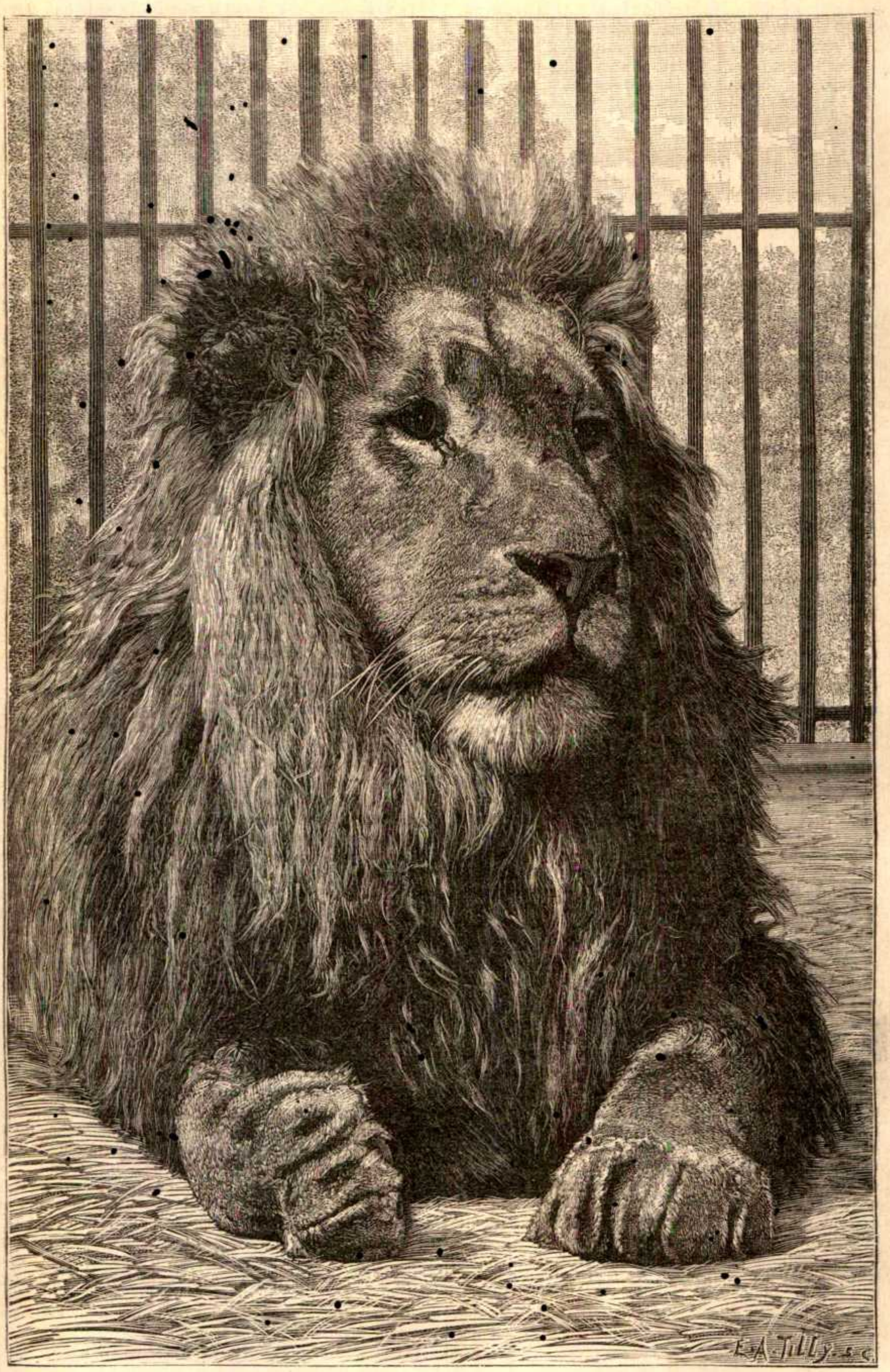
[1] "Die Herkunft, Domestication und Verbreitung des gewöhnlichen Feigenbaums (*Ficus Carica*, L.)." Von Grafen zu Solms-Laubach (Gottingen, 1882).—"Caprificus und Feigenbaum." Von Fritz Müller. *Kosmos*, xi p. 306.—"Sulla Caprificazione, &c." G. Arcangeli. Processi Verbali della Società Toscana di Scienze Naturali, November, 1882.


E. A. Tilly. sc.

may be well to explain, in the first place, the nature of the fruit of the fig, as it is something more than a seed-vessel of one flower. The fleshy part is a thickened hollow receptacle, closed, except a very narrow aperture at the top, and containing numerous minute flowers crowded together all over the inside of the cavity. Both the fig and caprifig produce three more or less distinct crops of fruit in the course of the year. Each of these crops of fig and caprifig bears a distinctive name; but the three crops of the former do not all reach maturity. In this country only one crop ripens. The varieties of the fig in Naples, whether cultivated or wild, produce fruit at least twice a year, and different varieties exhibit diverse phenomena in the degree of development and maturation of the several crops. In the fig the tissue of the receptacle or inflorescence is fleshy, and the perianth and pedicels of the individual flowers it contains thicken and abound in a sugary juice; whilst the fruit of the caprifig remains hard and milky up to maturity, or only imperfectly softens just at last without any secretion of sugar, and then shrivels and dries up. As long ago as 1770, Colin Milne[1] recorded the fact that the varieties of fig cultivated in England contained only female flowers; and Graf Solms found that male flowers were almost invariably altogether wanting in the varieties cultivated in Naples, and in the very rare exceptional instances in which they were present they were imperfectly developed and abnormal, the anthers being commonly replaced by leafy organs. On the other hand the inflorescence of the caprifig, as observed in Naples, usually contained both male and female flowers, the latter covering the greater part of the surface of the cavity, and the former restricted to a zone, variable in breadth, in the neighbourhood of the apical aperture. It is, moreover, noteworthy that the inflorescence exhibits proterogynous dichogamy in a marked degree. At the time when the female flowers are in a receptive condition the male flowers are still in a very early stage of development. The significance of this will perhaps be better understood after reading the description of caprification—that is if we may assume with Müller that this is really a process of fertilisation, in which there is a mutual adaptation of the inflorescences of the fig and caprifig and the insect which is an agent in procuring fertilisation. Before proceeding to that description, it should be mentioned that a variety of the fig exists in Brittany in which normal male flowers are abundantly produced. Yet, as in the caprifig, the males are not developed until long after the females have passed the receptive stage. The position this variety occupies in relation to other varieties and to the caprifig has not been ascertained. It may be a reversion to an original monœcious condition.

With regard to caprification, it was known to the ancients that an insect inhabits the fruit of the caprifig, and they also discovered that the visits of this insect to the fruit of the fig exercised some beneficial influence, either in accelerating ripening or in hindering the fall of the fruit before it was ripe. Consequently, branches of the caprifig were hung on the fig-trees at a certain season to insure these visits, and effect what was termed caprification. The insect that operates in this manner is a small hymenopter (*Blastophaga grossorum*, Grav syn. *Cynips psenes*, Linn.), the complete annual cycle of development of which takes place within the three crops of fruit of the caprifig, whilst only one generation visits the fig, and that, as will be seen, to no advantage to the insect itself. In order to render what follows easily understood, we will give the present Neapolitan names of the three crops of the caprifig. The fruits that hang through the winter and ripen in April are called *mamme* (*cratitires* of the ancients). These are followed by the *profichi* (*orni*), which ripen in June, and the *mammoni* (*fornites*), which ripen in August and September. If we closely examine the *profichi* when fully ripe in June, we see here and there a black-winged insect emerging from the orifice at the top, its hairy body dusted over with pollen grains that have adhered to it in its passage through the zone of male flowers. And if we cut open one of these fruits, we find a considerable number of these insects, all striving to find the way out. These are females and associated with them are some helpless wingless males, and very often a number of a slender ichneumon as well. The female of this generation visits not only the *mammoni*, but also the fruits of the fig, if there are any at hand, in order to deposit her eggs. Now the remarkable fact in connection with this is that she is able to do so effectually in the *mammoni*, but not in the edible fig, though she succeeds in penetrating the fruit far enough to convey pollen to the female flowers, perishing in the act. Furthermore the generation of the insect that develops in the *mammoni* deposits eggs in the *mamme*, and the generation proceeding therefrom finds an asylum for its progeny in the *profichi*. Respecting the reproduction of the *Blastophaga*, Graf Solms claims to have made the important discovery that the eggs must be deposited within the integuments of the ovule itself; otherwise they do not develop. The fertility of the insect is astonishing, a very few of them being able to pierce the numerous female flowers of a fruit of the caprifig. For this purpose the ovipositor is thrust between the branches of the stigma, down the pollen channel of the style into the ovary, and into the solitary ovule itself. This act causes a gall-formation, whilst it does not prevent the development of the ovule into an imperfect seed, which shelters and nourishes the larva that escapes from the egg.

The foregoing condensed extracts are perhaps sufficient to give an idea of the only way in which the female flowers of the fig are fertilised by the male flowers of the caprifig. It seems to be almost certain that seedling figs are unknown in countries where the caprifig does not exist. Where it is found apparently wild it is rather as the remains of cultivation than as plants sprung up from seeds. With regard to the origin of some of the cultivated varieties purporting to have been raised from seeds produced without the intervention of the caprifig, they offer a field for further research and experiment. Possibly they owe their origin to what has been called parthenogenesis, and more recently adventitious embryo-formation. Passing over many other interesting particulars in Graf Solm's essay, we come to one which Dr. Müller regards as strongly in favour of his view. It is this, the seedling offspring of the fig, fertilised by the caprifig, are said to consist of varieties of the fig and the caprifig, pure and simple, without any forms intermediate beween the two parents. On the other hand it is stated that a perfect seed is now and then found in the *profichi*. Prof. Arcangeli, in a later memorandum on the subject, states that he is unable to pronounce judgment in favour of one or the other of these views, and confines himself to recording the following observations on wild and cultivated forms. The *Fico verdino* and the *Fico piombinese* are commonly cultivated varieties in Pisa, yet he had never found a single perfect seed in their fruit, whereas in the fruit of the *Fico biancolino*, which is considered as a wild form, among numerous imperfect seeds he had found some perfect ones, which germinated freely. Whatever light future investigations may throw on this subject, the foregoing facts concerning the life-history of the *Blastophaga* and the fertilisation of the fig are of great interest. In conclusion it may be added that Graf Solms found the same or a closely allied insect in the species of Ficus that are most closely related to *Ficus Carica*, and which inhabit Western Asia, including North-Western India. As Müller suggests, it would be worth while looking into the matter to see whether they offer male and female forms.

W. BOTTING HEMSLEY

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[1] "A Botanical Dictionary," in the article on "Caprification."

NOTES

THE Queen has been pleased to confer the honour of Knighthood upon Dr. C. W. Siemens, F.R.S.

M. WOLF has been nominated Member of the Academy of Sciences by 32 votes against 21 given to M. Bouquet de la Grye. At Monday's meeting M. Jordan pronounced the *éloge* of Prof. Henry Smith, and M. Bertrand gave an explanation on the double prize, to which we referred last week. He stated that the Commission was aware of the existence of the paper of Prof. Henry Smith, and that it was to oblige Prof. Smith to publish his valuable secret that the prize-subject was selected.

UP to the present date, we understand, there have been received in answer to the official letter of inquiry to the Members of the British Association, as to whether they intended to go to Montreal or not, replies in the affirmative from 340. Among these are a good many who may be said to be really representative of English science, but as might be expected the younger men are present in a larger proportion than the older.

THE Annual Meeting of the Iron and Steel Institute will take place at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Wednesday, May 9th, and two following days. On Wednesday, May 9, the Bessemer Gold Medals for 1883 will be presented to Mr. George J. Snelus and Mr. Sidney Gilchrist Thomas. During the meeting the following papers will be read:—On the Value of Successive Additions to the Temperature of the Air used in Smelting Iron, by Mr. I. Lowthian Bell, D.L., F.R.S., Middlesborough; Comparison of the Working of a Blast Furnace with Blast varying in Temperature from 990° F. to 1414° F., by Mr. William Hawdon, Middlesborough; on American Anthracite Blast Furnace Practice, by Mr. Thomas Hartman, Philadelphia; on the Northampton Iron Ore District, by Mr. W. H. Butlin, Northampton; on Steel Castings for Marine Purposes, by Mr. William Parker, of Lloyds; on the Separation and Utilisation of Tar, &c., from Gas in Siemens' Gas Producers, by Mr. W. S. Sutherland, Birmingham; on Improvements in Railway and Tramway Plant, by M. Albert Riche, London; on the Estimation of Minute Quantities of Carbon by a New Colour Method, by Mr. J. E. Stead, Middlesborough; on the Tin-plate Manufacture, by Mr. Ernest Trubshaw, Llanelly, South Wales; on the Coal-washing Machinery used at Bochum, in Westphalia, by Mr. Fritz Baare, Bochum.

WE regret to announce that Dr. William Farr, C.B., formerly Superintendent of the Statistical Department of the Registrar-General's Office, died on Saturday night. He was born in 1807, at Kenley, in Shropshire, was educated at Shrewsbury, and afterwards proceeded to the Universities of Paris and London. After discharging the duties of house-surgeon of the Infirmary at Shrewsbury for a short time, he continued the practice and teaching of medicine in London, editing the *Medical Annual* and the *British Annals of Medicine*. In 1838 he was appointed Compiler of Abstracts in the Registrar-General's Office, where he organised the statistical department, of which he was made superintendent. In this capacity he assisted in taking the census in 1851, 1861, and 1871. He was author of a large number of articles, contributions to medical journals and papers relating to statistics of health and kindred subjects. He wrote many official reports on Public Health, on the Cholera Epidemic of 1849, and on the Census; and he constructed the English Life Tables, with values of annuities. Dr. Farr was Corresponding Member of the French Institute. It may be remembered that a few years ago considerable disappointment was felt that, when a vacancy occurred in the office of Registrar-General, Dr. Farr was not appointed to the post, with the work of which he had so long been credited.

MAJOR-GENERAL H. G. D. SCOTT, C.B., F.R.S., late Royal Engineers, died on Monday morning at his residence, Silverdale, Sydenham, aged 61. He was educated at the Royal Military Academy, Woolwich, and entered the Royal Engineers in 1840. He acted as instructor in surveying and practical astronomy at Chatham, and also as examiner of military topography for the Military Education Department at the War Office. He retired from the army in 1871 as major-general, and became Director of Buildings at South Kensington, acting as architect to the Royal Albert Hall and Science Schools. He was secretary to the Royal Commissioners of the 1851 Exhibition. He has just finished superintending the construction of the Great International Fisheries Exhibition.

PROF. FRANCIS MARCET, who died a few days ago in London at an advanced age, though English by birth, was a Swiss by adoption and family connection, and spent the greater part of his long life in Geneva. Marcet's achievements in science were numerous and noteworthy, and procured for him the Fellowship of the Royal Society. Some of his discoveries, especially those concerning the boiling point of water, the determination, by freezing, of the specific heat of solids, and, above all, his observations at Pregny on the increase of temperature of artesian wells, are recognised as important. Several of these observations were made in collaboration with his friend, Auguste de la Rive. In conjunction with De Candolle he made a series of researches in vegetable physiology and the action of poison on plants, and his "Manuel de Physique élémentaire," albeit now out of date, ranked forty years ago as the best scientific textbook of the period.

THE French Government are steadily continuing their excellent work of deep-sea investigation. Their vessel, the *Talisman*, is now being equipped and fitted out with the most improved machinery and apparatus, and will leave on June 15 for Morocco, the Canaries, Cape Verd Islands, Azores, and the Sea of Sargasso. Our last expedition of this kind, in the *Challenger*, although highly successful considering the great extent of area traversed by it, might be considered in one respect tentative, and ought to have led to further results. Our own seas have never been sufficiently investigated, while the Americans, Norwegians, Germans, French, and Italians have, especially of late years, been indefatigable in thoroughly exploring their parts of the North Atlantic and Mediterranean.

FROM Monday's debate it is evident that the new Patent Bill will not satisfy everybody, which was just what might be expected. It is certainly a great improvement on the existing law. The provision with regard to the Patent Museum seems to us a step in the right direction. The Bill provides that the control and management of the existing Patent Museum and its contents shall be transferred to and vested in the Department of Science and Art, subject to such directions as Her Majesty in Council may see fit to give. The Department of Science and Art, moreover, may at any time require a patentee to furnish a a model of his invention for deposit in the Patent Museum on payment to the patentee of the cost of the manufacture of the model. Another commendable provision is that the Comptroller shall cause to be issued periodically an illustrated journal of patented inventions, as well as reports of patent cases decided by courts of law, and any other information that the Comptroller may deem generally useful or important.

THE Birmingham Natural History and Microscopical Society has established a "Sociological Section," for the study of Mr. Herbert Spencer's system of philosophy. The section originated in a wish to unite, for the purpose of mutual help, those who were already students of Mr. Herbert Spencer's system, but were unknown to each other, and to introduce to the synthetic philosophy those already engaged in some special biological study, but as yet unfamiliar with the principles common to all departments of natural history. Mr. Herbert Spencer, who is already an honorary vice-president of the Society, has been communicated with, and has expressed his cordial approval of the course of work proposed to be done by the section, adding some valuable suggestions. It is intended to go through the whole of his works, discussing special points as they arise, and where practicable giving illustrations. The president of the section (Mr. W. R. Hughes, F.L.S.) will open the first meeting with a brief address.

SEVERAL contributions to the theory of the microphone have lately appeared. Mr. Shelford Bidwell has communicated to the Royal Society a series of determinations of the changes of resistance of a microphonic contact under different pressures; and comes to the conclusion that the mere fact that a current causes delicately adjusted metal contacts to adhere to each other seems sufficient to account for the superior efficiency of carbon. Mr. Bidwell also thinks that the heat generated at the contact by the current plays an important part, for in carbon this reduces the resistance, whilst in metals it increases it. Mr. Bidwell's experiments on metals were, however, confined to the metal bismuth, which, being both the most fusible and the worst conductor, is the very one which ought to have been avoided. No conclusion of any value as to the metals in general can be drawn from experiments on bismuth alone. Mr. Oliver Heaviside has also experimented on the microphone, and finds the apparent resistance of a contact to vary inversely as the square root of the current. Arguing from these observations he concludes that it is no use to arrange a number of microphones either in series or in parallel. This result is, however, contradicted by experience, for a transmitter such as the Hunnings, with many contacts in parallel, is much more powerful than the single-contact Blake transmitter. Moreover the results attained in Paris lately by M. Moser using a "battery" of microphones arranged partly in series, partly in parallel, disposes of this conclusion of Mr. Heaviside's. It may be remarked that the suggestion to use a battery of microphones was made in 1881 by Prof. Silvanus Thompson. Messrs. Munro and Warwick have lately produced some successful telephonic or microphonic transmitters with metal contacts. These experimenters regard the action of the microphone as due to the existence of a silent discharge of electricity through the thin air stratum at the contact. This view is perhaps sustained by a remarkable observation due to Mr. Stroh, that when a current is passed through a carbon microphone of a peculiar type there is a very minute repulsion observable between the two pieces of carbon, the actual movement being through a distance of ·0005 of a millimetre!

Litera scripta manet is a phrase which is literally true of China. It is generally mentioned in popular books on that country that the respect for paper on which any words are written is so great that scavengers are specially employed to collect it in the streets and preserve it. Whatever doubt existed on this score must now be set at rest, for in a recent issue of the *Peking Gazette* we find a memorial to the throne from the Police Censor of the central division of the capital, reporting that there are in that city over eighty establishments for the remanufacture of waste paper. Paper with characters on it, the memorialist complains, used to be mixed up with the waste paper and defiled by being applied to such base uses. The memorialist and his colleagues published proclamations embodying the sacred edict of the great Emperor Kang-hi, that in heaven and earth there is nothing more precious than written characters. Shopkeepers were forbidden to traffic in printed or written paper, and the manufacturers were ordered to pick out all such paper from among the waste paper purchased by them, and send it to the offices, where a certain amount per pound would be paid for it. Two temples were selected where this paper could be properly burned periodically. The police magistrates on inquiry find that now the manufacturers have some idea of the reverence due to written characters; but some permanent means of supporting the expenses of the purchase and sacred process of destruction should be established, as at present the memorialist has to pay them out of his own pocket. He further suggests that the sale of the house and furniture of a certain escaped criminal, though they will not fetch much, will be sufficient, if put out at interest, to meet these expenses; and he further requests that the sale of written paper to manufacturers be forbidden. The Imperial rescript on this memorial has not come to our notice; but in all probability the escaped criminal's house and furniture are now employed in preventing the defilement of the "*fliegende Blätter*" of Peking.

ACCORDING to the *China Mail* telegraphs in China are likely to receive a most important extension in the shape of a line from Canton to Shanghai. Should this line be constructed, the southern port will then be in direct connection with Tientsin. Lead ore, according to the same authority, has been discovered in Kwantung, the province in which Canton is situated; and it is proposed to work mines of this metal. These movements are stated to be purely Chinese, "and as signs of progress they are worthy of the most attentive consideration."

FROM Mr. J. F. Duthie's "Report on the Progress and Condition of the Government Botanical Gardens at Saharunpur and Mussoorie for the Year ending March 31, 1882," we learn that many additions have been made to the Gardens, of interesting and valuable economic plants, among them the *Cassia marilandica*, L., or North American senna plant, the wax palm of the Andes (*Ceroxylon andicola*, Humb.), upon the trunks of which large quantities of wax are formed and is easily removed by scraping, *Ferula tingitana*, L., the ammoniacum plant of Morocco, *Fraxinus ornus*, L., the manna ash of the Mediterranean region, *Guaiacum officinale*, L., the lignum vitæ of commerce, *Quassia amara*, L., one of the bitterwood trees from the West Indies, *Rheum palmatum*, L., var. *tanguticum*, a native of North-West China, one of the species which yields medicinal rhubarb. Besides these, many new fruits, vegetables, and fodder plants have been under cultivation. Mr. Duthie reports a very important item of cultivation, that of drug-yielding plants for the supply of drugs for the use of the medical department. Extract of henbane and extract of taraxacum have both been made, and Mr. Duthie has prepared a list of other drugs which he proposes to cultivate either in the hills or at Saharunpur. Amongst these may be mentioned aconite, aloes, buchu, calumba root, colchicum, digitalis, gentian, jalap, liquorice, scammony, colocynth, and others. It seems that the cost of maintaining the Saharunpur Gardens much exceeds the income derived from them; but being kept up mainly for scientific purposes they are not expected to prove directly remunerative. It further appears that sanction has lately been given to the closing of the Gardens at Mussoorie and Chajri, which it has been found impossible to work successfully. A new Hill Garden, however, is to be opened at a more eligible site.

THE valuable geological and palæontological collections from Spitzbergen made by Dr. A. Nathorst and Baron De Geer last summer will be distributed between the National Museum and the Geological Museum at Stockholm, while the duplicate specimens will be presented to the museums in Upsala, Lund, and Gothenburg.

THE Second Part of vol. i. of Thomson and Tait's "Natural Philosophy," second edition, is announced for immediate publication, edited for the most part by Prof. F. Darwin. The remaining volume, originally planned, will not be published.

PROF. O'REILLY writes from the Royal College of Science for Ireland, Dublin, that there was visible there, on the night of the 16th, between 10 and 11 o'clock p.m., an aurora appearing as a glow, but without any beams when observed. The wind on the 17th was from the south, but the temperature was still relatively low.

THE opening of the proposed International Horticultural Exhibition and Botanical Congress at St. Petersburg has been postponed to May 5, 1884.

THE Council of the Popular Observatory of the Trocadéro has decided to open a series of Sunday lectures, illustrated by experiments, during the whole of the summer season. The Thursday lectures will be devoted to astronomical topics and delivered in the evening, and will be followed by demonstrations on the sky itself, weather permitting.

DR. DOBERCK, whose appointment to Hong Kong we noted last week, has been attached to Markree and not to Dunsink Observatory.

THE additions to the Zoological Society's Gardens during the past week include a Rude Fox (*Canis rudis*) from Demerara, presented by Mr. G. H. Hawtayne, C.M.Z.S.; an Arabian Gazelle (*Gazella arabica* ♀) from Arabia, presented by Mr. J. Sewell; three Weasels (*Mustela vulgaris*), British, presented by Mr. George Lang; a Wood Owl (*Syrnium aluco*), British, presented by Capt. E. Hall; a Lanner Falcon (*Falco lanarius*) from Eastern Europe, presented by Major J. H. Hussey; a Common Raven (*Corvus corax*), British, presented by the Earl of Eldon; five Mississippi Alligators (*Alligator mississippiensis*) from the Mississippi, presented by Mr. Thos. Baring; two Common Snakes (*Tropidonotus natrix*), British, presented by Lord Londesborough, F.Z.S.; two White-fronted Capuchins (*Cebus albifrons* ♂ ♀), from South America, presented by Mr. H. Smith; a Palmated Newt (*Triton palmipes*), British, presented by Mr. J. E. Kelsall; two Amherst's Pheasants (*Thaumalea amherstiæ* ♂ ♀) from Szechuen, China, deposited; three Lions (*Felis leo* ♂ ♀ ♀) from South Africa, two Reeves's Pheasants (*Phasianus reevesi* ♂ ♀) from China, a Great Black Cockatoo (*Microglossa aterrima*) from New Guinea, a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, a Common Otter (*Lutra vulgaris*), British, purchased.

OUR ASTRONOMICAL COLUMN

D'ARREST'S COMET.—We last week referred to the discovery of D'Arrest's comet at the Observatory of Strasburg on the 3rd inst., upon the strength of a telegram received at Lord Crawford's observatory from Prof. Krueger, to the following effect:—"Dr. Hartwig discovered on April 3·610 G.M.T. D'Arrest's periodical comet in right ascension 13h. 55m. 24s., declination +8° 16′. Daily motion −44s. in R.A., and +9′ in declination." This telegram was published in the Dun Echt Circular, No. 76, but in No. 77 issued five days later we read, "Prof. Krueger telegraphs that the object observed by Dr. Hartwig was not D'Arrest's comet but a new nebula." The "daily motion" assigned to the object in the first telegram, notwithstanding its precise accordance in amount and direction with that which the comet would have had in that position, was therefore an illusion. The calculated place of the comet for April 3·610 G.M.T. is in R.A. 13h. 55m. 11s., Decl. +8° 23′·6. During the next period of absence of moonlight for which an approximate ephemeris was given in this column last week, the theoretical intensity of light will be nearly one-third greater than on April 3.

THE GREAT COMET OF 1882.—Prof. Riccò sends us the following observation of this comet made with the 10-inch refractor at Palermo:—

	M.T.		App R.A.		App. Decl.
	h. m. s		h. m. s		° ′ ″
April 6 at	8 21 29	...	5 58 5·93	...	−9 4 49·2

He states that the comet was a very faint nebulosity with an elongated nucleus containing two or more points. At this time the comet was distant from the earth 3·87, and from the sun 3·75.

In *Bulletino della Società di Scienze Naturali di Palermo* for February 8 we find some remarks by Prof. Riccò on the circumstances attending the passage of the comet through perihelion. On studying the appearance of the sun from twelve to fifteen hours afterwards, he found the prominences were by no means unusual either as regards number or dimensions; there were nine with a greater altitude than 30″, and about as many smaller ones; the highest was one of 85″ on the west-north-west limb, opposite to the part of the disk traversed by the comet, in which no prominences were visible. The comparison of observations made before and after perihelion passage, shows that no very sensible effect was produced upon the motion of the comet in its course through the coronal atmosphere, and Prof Riccò concludes, on the other hand, that his own observations, made a few hours subsequently, "possono servire a constatare che reciprocamente la cometa non disturbò per nulla il corso degli ordinari fenomeni dell' attività solare."

THE BINARY STAR *p* ERIDANI.—In a communication to the Royal Society of New South Wales in June, 1880, Mr. Russell, the director of the Observatory at Sydney, suggested, from the measures made since 1856, including his own up to 1880, that this object might not be a binary star at all, but merely afforded an instance of one star passing before another by reason of its proper motion. This opinion is repeated in the volume of double-star results obtained at Sydney, published last year. "In fact," observes Mr. Russell, "a straight line accords better with all the observations made subsequent to Herschel's than any ellipse, and it would appear that the changes are due simply to proper motion; of this I think there cannot be any doubt. . . ." The question has just been very fully and carefully considered by Mr. Downing, of the Royal Observatory, Greenwich, who arrives at an opposite conclusion to that of Mr. Russell, and considers "there is not sufficient evidence to justify us in asserting that *p* Eridani is other than a binary star." We entirely agree with Mr. Downing in his opinion. If we only compare the measures made by Jacob in 1845-46, with those of Russell and Tebbutt, 1878-80, we get the following expressions:—

$$d\,.\,\sin p = -\,4''{\cdot}361 - [8{\cdot}3894]\,(t - 1850{\cdot}0)$$
$$d\,.\,\cos p = +\,0''{\cdot}122 - [9{\cdot}1017]\,(t - 1850{\cdot}0)$$

showing differences from Herschel's mean measures, epoch 1834·996, of −5°·1 in position, and +0″·82 in distance, which are too large to be tolerated.

This star has been occasionally miscalled 6 Eridani, which would imply that it was one of Flamsteed's stars. Flamsteed, it is true, has a star which he calls 6 Eridani, and which is B.A.C. 926; the binary is B.A.C. 521. The letter *p* was attached to the star by Lacaille in the catalogue at the end of his *Cœlum Australe Stelliferum*. The number 6 is merely borrowed from Bode.

GEOGRAPHICAL NOTES

THE Geographical Society of Lisbon has awarded their gold medal for this year to Mr. Carl Bock, the distinguished eastern traveller, who has also been recently elected Corresponding Member of the Italian Anthropological Society.

THE third German Geographentag was held at Frankfort-on-the-Main on March 29 in the presence of 430 men of science. Prof. Rein (Marburg) delivered the inaugural address, and also opened the geographical exhibition, which comprised 1100 objects of interest. Amongst the most successful addresses we mention the following: Dr. Pechuel Loesche (Leipzig), on the mountain districts of the Congo River, in which he described minutely the mountain chains traversed by the Congo, according to the researches of Oscar Lenz and Güssfeldt. Prof. Ratzel (Munich), on the significance of Polar research with regard to geographical science; he proposed a resolution, "That the Geographentag recognises that the resumption of Polar research by the German Government is equally in the interest of geographical science and of the German nation." This resolution was adopted unanimously. Dr. Finger (Frankfort), on topography as an introduction to geography. Herr Mang (Baden Baden), on the method of the tellurium and lunarium. Dr. Breusing (Bremen), on the means for the determination of the position of localities at the time of great discoveries. Dr.

Buchner (Munich), on the tribe of Bantu negroes distributed through the whole of South-west Africa. Prof. Gunther (Ansbach), on the latest investigations regarding the exact shape of the earth. Lieut. Wissmann, on his journey across Africa with Dr. Pogge. The oldest among the German African travellers now living, Dr. Rüppel, had come to Frankfort to greet Wissmann upon his return. The fourth Geographentag will be held at Munich.

FROM a paper by M. Smicroff on the climate of the Caucasus (published in the Caucasian *Isvestia*, and based on the researches of Dr. Wild on the temperatures in Russia), it is evident that although enjoying a warm climate, still the climate of the Caucasus, especially in the north, is quite continental. Thus, the average mean temperatures of the year are 5°·4 Cels. at Alexandropol, 8°·5 at Stavropol, 12°·6 at Tiflis, and 14°·3 to 14°·5 at Bakou, Lenkoran, Kutais, Poti, and Redut-kaleh; but the yearly range of the average diurnal temperatures is still (with the exception of the two last places) as much as 20 to 30 degrees, while in Central and Southern Russia it varies from 22 to 35 degrees. The highest temperatures observed on the Caucasus vary from 38°·5 at Tiflis, to 34°·4 at Poti; and the lowest from −25°·6 at Stavropol, to −17°·3 at Tiflis, and −6°·6 at Redut-kaleh. It is interesting to compare these temperatures with the +38°·8 and −62° observed at Yakutsk, and −63°·2 at Verkhoyansk. Altogether, it is only in Southern Transcaucasia that localities are found where the temperature does not fall below −10°, and the southern limit of the region beyond which temperatures lower than −20° are no longer found, runs from the Crimea to the Caucasus range, and along the northern slope of this last, towards Khiva, Tashkend, and Peking. The whole range of temperatures observed at Caucasian stations is 60°·4 at Stavropol, 55°·8 at Tiflis, 45°·9 at Bakou, 42°·1 at Poti, and 41°·6 at Redut kaleh. Of course it is nothing in comparison with the range at Yakutsk, where the inhabitants must be accustomed to experience differences of temperature ranging a little more than 100° (from −62° to +38°·8). But still it is large enough, especially for the places situated on the plateaux. High-level meteorological stations are established at Goudaur (2156 metres above the sea-level) and Kvinam (2362 metres). Their average yearly temperatures respectively are 4°·1 (−8° in February and 14°·3 in August) and 1°·1 (−14° in January and 12°·3 in August).

THE last number of the *Izvestia* of the Russian Geographical Society contains an elaborate paper, by M. Malakhoff, on the anthropology of the Vyatka region; a description of inscriptions on rocks on the Yenisei, with drawings, by M. Schukin; a note on old Russian geography, by M. Arsenieff; an account on M. Balkashin's researches into the Kirghiz, being a most valuable addition to our very imperfect knowledge of them. The author comes to the conclusion that the Kirghiz are not a separate nation, but a federation of several nomad tribes who inhabited Southern Russia, the Gobi, the neighbourhoods of Dalay-nor, the sources of the Black Irtish, and the shores of the Baikal, who were mingled together by Genghiz Khan and his successors. M. Grigorieff contributes a note in which he shows that Henriette Island, which was discovered by the *Jeannette*, is only the land which was sighted by Hedenström and Sannikoff from New Siberia in 1810, and that Bennett Island was seen by Sannikoff from the northern coast of New Siberia in 1811. There can be no doubt also that the land discovered by Sannikoff to the north-west of the northern extremity of the Kotelnyi Island exists in reality, but is more distant than Sannikoff supposed. This land, which was shown in dotted lines on older maps, but disappeared since Wrangell and Anjou's journeys, ought to be reintroduced on our maps. The same number contains a note on the map of Bokhara of the Greek Vatalsi, a necrological notice of M. Tchoupin, several notes, and a new edition of the complete bibliography of the Amoor, by M. Bousse. One of the notes contained new astronomical determinations and hypsometrical measurements on the Yu-tschou, by Dr. Fritsche; the Siao-Utai-shan proved to be only 9500 feet high, instead of the 11,452 feet given by Mellendorf.

ACCORDING to intelligence from Tashkend, dated March 31, received by the *Cronstadt Courier*, it is in contemplation to send two Russian Exploring Expeditions into Central Asia during the coming summer. The ostensible object of one is to survey part of the Pamir Steppe and fix certain points astronomically, with the object of connecting the Russian surveys with the English. The purpose of the other is to determine the astronomical position of places on the Oxus from the points of passage in its upper course down as far as Khiva, in fact the whole course of the river.

FROM M. De Lesseps's examination of the ground through which it is proposed to let the waters of the Mediterranean into the Tunisian and Algerian Chotts, he concludes that the scheme is perfectly practicable, and that there will be no difficulty as to boring and excavating. The size of the proposed inland sea will be fourteen times that of the Lake of Geneva.

THE SOARING OF BIRDS[1]

THE circling, soaring flight of birds on stiff, outspread wings appears to me a much more complex problem, and less easy of explanation, than that of motionless hovering (poising). At the same time it has certain definite and characteristic features, which must depend upon and connote certain definite aërial conditions, and which should therefore afford us so many hints toward the solution. The whole phenomenon has been very clearly described in NATURE (vol. xxiii. p. 10) by Mr. S. E. Peal, who appears to have had grand opportunities of observing it at Sapakati in Assam. [The explanation which he gives is, however, insufficient, because he does not show how the bird in falling with the wind can acquire greater "impetus" *relative to the air* than it would if the air were still. But such greater "impetus" is necessary if the bird is to rise to a greater height than it would reach in still air.]

The most typical instance that I have observed was on January 8, 1882, near King's Lynn, in Norfolk. The whole country for many miles round is flat, broken only by trees, buildings, and sea walls or river embankments. The wind was strong from the south. The birds (large gulls) were drifting away northwards towards the Wash, circling as they drifted on stiff, outspread wings at a height of 200 or 300 feet, and apparently rising higher. The level nature of the land forbade the notion that the wind had received an upward throw from any fixed obstacle in its path (though I shall show below that there may be upward currents in the air without the presence of a fixed obstacle).

The circling appears to begin about 100 or 200 feet above the ground. A strong wind is a constant and (presumably) necessary condition. The bird descends with the wind, and then circles round to right or left, and rises against the wind to a greater height than it had before. Now if the whole mass of air were moving together horizontally with the same velocity throughout, this action would be wholly inexplicable, for the bird would feel no more wind in one direction than in another, and indeed would have no evidence of the existence of any wind at all except in glancing at objects on the earth. The fact that the earth is slipping away under the air in a certain direction does not affect the bird's relation to the air, and gives no reason why the bird should fall or rise at one phase of its circle more than at another. Still less does it furnish an explanation of the bird's progressive ascent. We may therefore infer as highly probable that the air in which the birds are circling does not move in a mass, but that there is some differential movement in it which makes a great difference to the bird, whether it rises or falls with or against the wind.

I think there are at least two types of differential movement in the upper air which admit of demonstration, and which should be tested in turn to see if either of them can give the meaning of the phenomenon of circling.

(1.) First, there is almost always *a greater velocity in the higher strata* of the air than in the lower. The lower strata are delayed by friction on the earth's surface, and the higher strata outrun them; just as the water of a brook is delayed by friction against its bank, but flows faster in mid-stream.

(2.) Secondly, where currents override or run past one another there is generally some *rolling* between them. This may be seen near the edge of any stream of water if the surface is smooth enough to exhibit the little swirls and whirlpools that are formed between the swifter and slower currents. In the air it may be seen on a grand scale on almost any windy day when there are separate floating clouds in the sky. Looking at right angles to the direction of the wind, each cloud is seen to have a systematic movement within itself: the rearward skirts of the cloud are climbing up its sloping back, often with little rolling curls: the top of the cloud is rolling over like a breaker in the act of tumbling on the beach. Each cloud is in fact the scene of a travelling vortex in the air revolving round a more or less horizontal axis. And the persistency with which such a cloud will preserve its individual identity indicates the persistency of the vortex. In the comparatively lower regions of the air it may be difficult to demonstrate the presence of such vortices, but similar causes are present, and it cannot be doubted that similar effects are produced and that such vortices, possessing a proportionate degree of persistency, are generated in those regions of the air which are within the range of the habitual flight of circling birds.

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[1] Lord Rayleigh's valuable letter on this subject (NATURE, vol. xxvii. p. 534) gives me confidence in offering the following considerations, which I had prepared last February, and have submitted to two or three mathematical friends. I congratulate myself on finding my own views in such close agreement with Lord Rayleigh's.—H. A.

Let us see what effect these conditions (1) and (2) separately would have upon the circling flight of a bird.

(1.) First, let us take horizontal currents increasing in velocity the higher they are above the earth; and suppose a bird at the highest point of one of its gyrations, when it has mounted against the wind and is wheeling to one side or the other, preparatory to the descent with the wind which is to give it sufficient velocity for another rise (but which could not enable it to rise to the same height as before if the air had no internal movement, for there would be no self renewing force to neutralise the ever-new force of gravity and the perpetual friction of the air). Let us regard the air at the level of the bird, at this turning-point, as *still*. Then, relative to this point, the lower strata of air have a horizontal velocity in the opposite direction to the wind (as perceived on earth); and the bird in falling apparently down the wind will really be meeting stronger and stronger adverse currents, and when it has reached the lowest point of the "circle," it will have a greater horizontal velocity relative to the air at that level than if the whole air through which it has fallen had been still. Therefore, in virtue of its greater horizontal velocity relative to the air (which is accompanied by increased air-resistance), the bird will be subject to a greater force upon its wing-surface, and will therefore be able to mount higher (*cæteris paribus*) than if it had fallen through still air. But (instead of "*cæteris paribus*") suppose the bird, as it rises, wheels gradually round and faces the wind. Then, in rising, it will enter successive strata of air having successively greater and greater velocity relative to itself (the bird) than if the air had no internal movement, and therefore the air-resistance, which is the lifting force, will ever be greater than that due to the height gained by the bird if in still air; and therefore the bird will be able to rise yet higher. But this manœuvre of wheeling to face the wind in rising will cost some time, during which gravity ceases not to act; it will also cost some friction and a slight loss of horizontal velocity, and the question is whether these losses are sufficient to destroy the advantage above described. This is a problem for the mathematicians to solve.

It seems difficult to imagine that within the narrow limits of the actual rise and fall of the bird at the different phases of its circle, there should be sufficient difference of velocity of upper and lower air-currents, to account for such a gain of elevation as Mr. Peal mentions (from 10 to 20 feet at each lap). We require, however, to know the vertical height of the bird's fall and subsequent rise. I have not seen any estimate of this, but, judging from Mr. Peal's diagram, the bird's fall appears no greater than its gain of elevation (10 or 20 feet).

Still it appears from the foregoing considerations that the bird will gain support by falling with the wind and rising against it, when the upper wind is stronger than the lower.

This result suggests that a bird might with like effect make use of two collateral currents of different velocity. Suppose two currents, fast and slow, side by side, flowing in the same direction. The bird may fall with the slow current, and so acquire a certain horizontal velocity. Then let it wheel round against the swift current, and it will be able to rise against it to a height due to the greater horizontal velocity between bird and air. Having reached full height, let it again wheel round into the slow current and recover by a sloping descent therein the horizontal velocity it has lost, which, when recovered, will enable it to mount again against the fast current.

Thus it would appear that a bird can take advantage of alternate fast and slow currents, whether collateral or superposed, rising against the fast and falling with the slow, to maintain itself in the air, while partaking in the general drift of the wind, without flapping its wings.

(2.) In the next case to be considered, we have to deal not with horizontal currents, but with the rotatory currents of rolling masses of air. A mass of air rolling about a horizontal axis will have descending currents in its front, and ascending currents in its rear. The former can be of no use to the bird for the purpose of support. The bird must keep in the rear of the roll, where it will find an upward slanting current. In a high wind this current would probably be strong enough to support the bird in motionless poise (relative to the earth), but this could only be for a few seconds, because the whole vortex is travelling rapidly with the wind (of which it forms a part) and would speedily pass and leave the poised bird behind at the mercy of the downward currents in the van of the next advancing vortex. How then is the bird to remain in the upward current, and at the same time to maintain a high velocity relative to the air in which it moves? It can only be done by circling. The bird must face the current in rising, and as it approaches at once the outskirts of the current and the limits of its own momentum (relative to the air) it must wheel round (—indeed it must have begun to wheel while rising—) and fall down the wind, for the double purpose of recovering its spent velocity and of overtaking the receding vortex.

In falling down the wind, the bird will pass out of a stronger into a weaker current, and will be able to take advantage of the difference (regarded horizontally), just as in the case (already considered) of horizontal currents of different velocity. But regarded vertically the descent into the weaker current will be a disadvantage. However, it is clear that under these conditions there will be no difficulty about the bird's support in air by a circling flight without stroke of wing.

But there is still a difficulty with regard to the progressive ascent of the bird. Mr. S. E. Peal (NATURE, vol. xxiii. p. 10) testifies that the pelican, adjutant, vulture, and cyrus rise circling from 100 or 200 to as much as 8000 feet. Can it be supposed that a rolling vortex of air would have equal range or climb to such a height? Swirls formed at the edge of a deep stream of water are seen to be drawn obliquely away from the side towards mid-stream, and I suppose that an aërial vortex with horizontal axis will in like manner be drawn obliquely upwards into the more rapid air. Moreover I remark that Mr. Peal's observations were made on the coast, and that his diagram represents the birds as rising on a wind blowing up the country towards the hills. Such a wind would have a general upward slant, and any rolling of the air would have the same slant to begin with and to rise from, so that a bird keeping to the (supposed) vortex would rise with it to the same height.

The same principles which we have found useful in dealing with the regular and rhythmical phenomenon of circling flight will, I think, help us to understand the general case of irregular sailing flight, like that of the albatross following a ship, as described by so many writers (*e.g.* the Duke of Argyll, "Reign of Law," fifth edition, pp. 153–4). This general case may be accounted for by (1) irregular alternations (either in strength or direction) of horizontal air-currents; or (2) irregular upward currents.

Currents alternating in strength are equivalent, in relation to any intermediate point, to currents alternating in direction.

To take an extreme, almost imaginary, case: let us suppose a bird on outspread wings exposed alternately to the force of exactly opposite winds. To each in turn the bird will offer the sloping under-surface of its wings, and by each in turn it will be at once uplifted and pushed back, but each will counteract the backward push of the other, while each will reinforce the other's uplifting effort. The result will be that the bird will rise in a wavy line without any effort of its own beyond what is required to keep its wings rigid, and to present them favourably to the alternate winds.

Now suppose the whole air to be travelling horizontally in a direction at right angles to the two opposite currents. This supposition will not affect the lifting power of those opposite currents, but it will make it necessary for the bird (if it is not to be swept away by the travelling air) to sacrifice some of the height it might gain for the sake of making head against the general drift of the wind. This is no longer an extreme or imaginary case, but one of very frequent occurrence. It is simply that of oscillating gusts in a high wind. The air is full of sidelong rushes of wind (probably parts of neighbouring vortices). See how the vane of a weathercock oscillates. A sidelong rush means fresh velocity relative to the bird in a new direction. The bird by a tilt of the wing can instantly convert that fresh air-pressure into a lifting force and rise upon it. And if these rushes of wind come alternately (as in an irregular fashion they are sure to do) from right and left, the bird can take advantage of their alternation to rise higher and higher, or at least to remain floating, without more effort than that which is required to give the due slope to its wings to make the most of every gust.

Next suppose the whole air with its two alternate opposite currents (as above) to be travelling horizontally in the same direction as one of the two opposite currents. Whether this supposition represents a possible state of things I hardly know, but it would correspond in some measure with the commonly observed phenomenon of a succession of alternate gusts and lulls in the wind. Under these conditions, if the air-movement be all horizontal, it is difficult to see how the bird can turn the alternate gusts to advantage, unless it can alternate its own direction accordingly, stemming the gust and wheeling round to fall back with the lull. The bird then would either circle or would follow a wavy course oblique to the direction of the wind. But I imagine that alternate gusts and lulls (as felt, say, at the top of an observatory) are generally caused by a succession of vortices, of which only one phase at a time is present to the observer. These vortices will be infinitely various in the direction of their axes and currents, and it is useless to try and imagine their relative positions. Probably the sea-birds, with their æons of inherited experience, have acquired an instinctive perception of the probable sequences and correlations of air-streams and air-swirls, and are thereby guided so to steer their course, selecting the upward and avoiding the downward currents, as to gain the greatest possible advantage of lifting force that those currents can afford, to the great economy of their muscular strength, which would otherwise have to be spent in the labour of the wing.

In reading of the way in which albatrosses and other large sea birds will follow a ship at sea with little or no flapping of the wings, it has occurred to me that the great obstacle which the ship herself offers to the wind must of necessity give the wind an upward throw and originate a vortex in the air, possibly large enough and persistent enough to be useful to the birds. If the ship be a steamer, the drift of smoke from the funnel will indicate approximately the path of the retiring vortex. It is long since I have had any opportunity of observing, but I well recollect that the gulls used often to be seen in close relation to the smoke that drifted to leeward of the steamer. It is true that any chance morsels of biscuit, &c., thrown from the steamer would probably be thrown to leeward, and this might help to determine the position of the expectant gull.

Again, at sea, the ocean waves themselves, such as roll in from the Atlantic to the Land's End, must throw the wind into rolling vortices, which would afford slant upward currents. The slant, though very flat, might well be sufficient for the purpose of support to the long-winged sea-birds that know how to use it.

On land, countless obstacles impede the lower wind and tend to throw the air into a roll.

Bearing in mind, then, the perpetual variation in strength and direction of current in a high wind, the whirls and gusts, and veering flaws, and seeing how it is possible for the bird to utilise every such variation (except a downward current) to the purpose of its bodily support, we may, I think, obtain some insight into the agency whereby the birds accomplish their marvellous feats of soaring and sailing, upborne upon stiff-strained, motionless wings.

Further observations however, are required for the full solution of the problem which I have here only tentatively approached. HUBERT AIRY

Woodbridge, February 28

SOME POINTS IN ELECTRIC LIGHTING[1]

THE science of lighting by electricity was divided by the lecturer into two principal parts—the methods of production of electric currents, and of conversion of the energy of those currents into heat at such a temperature as to be given off in radiations to which the eye was sensible. The laws known to connect together those phenomena called electrical, were essentially mechanical in form, closely correlated with mechanical laws, and might be most aptly illustrated by mechanical analogues. For example, the terms "potential," "current," and "resistance," had close analogues respectively in "head," "rate of flow," and "coefficient of friction" in the hydraulic transmission of power. Exactly as in hydraulics head multiplied by velocity of flow was power measured in foot-pounds per second, or in horse-power, so potential multiplied by current was power and was measurable in the same units. Again, just as water flowing in a pipe had inertia and required an expenditure of work to set it in motion, and was capable of producing disruptive effects if that motion were too suddenly arrested, so a current of electricity in a wire had inertia: to set it moving electromotive force must work for a finite time, and if arrested suddenly by breaking the circuit the electricity forced its way across the interval as a spark. Corresponding to mass and moments of inertia in mechanics there existed in electricity coefficients of self-induction. There was, however, this difference between the inertia of water in a pipe and the inertia of an electric current—the inertia of the water was confined to the water, whereas the inertia of the electric current resided in the surrounding medium. Hence arose the phenomena of induction of currents upon currents, and of magnets upon moving conductors—phenomena which had no immediate analogues in hydraulics.

The laws of induction were then illustrated by means of a mechanical model devised by the late Prof. Clerk Maxwell.

In the widest sense, the dynamoelectric machine might be defined as an apparatus for converting mechanical energy into the energy of an electrostatic charge, or mechanical power into its equivalent electric current through a conductor. Under this definition would be included the electrophorus and all frictional machines; but the term was used in a more restricted sense, for those machines which produced electric currents by the motion of conductors in a magnetic field, or by the motion of a magnetic field in the neighbourhood of a conductor. The laws on which the action of such machines was based had been the subject of a series of discoveries. Oersted discovered that an electric current in a conductor exerted force upon a magnet; Ampere that two conductors conveying currents generally exerted a mechanical force upon each other: Faraday discovered—what Helmholtz and Thomson subsequently proved to be the necessary consequence of the mechanical reactions between conductors conveying currents and magnets—namely, that if a closed conductor moved in a magnetic field, there would be a current induced in that conductor in one direction, if the number of lines of magnetic force passed through the conductor was increased by the movement; in the other direction if diminished. Now all dynamoelectric machines were based upon Faraday's discovery. Not only so; but however elaborate it might be desired to make the analysis of the action of a dynamo-machine, Faraday's way of presenting the phenomena of electromagnetism to the mind was in general the best point of departure. The dynamo-machine, then, essentially consisted of a conductor made to move in a magnetic field. This conductor, with the external circuit, formed a closed circuit in which electric currents were induced as the number of lines of magnetic force passing through the closed circuit varied. Since, then, if the current in a closed circuit was in one direction when the number of lines of force was increasing, and in the opposite direction when they were diminishing, it was clear that the current in each part of such circuit which passed through the magnetic field must be alternating in direction, unless indeed the circuit was such that it was continually cutting more and more lines of force, always in the same direction. Since the current in the wire of the machine was alternating, so also must be the current outside the machine, unless something in the nature of a commutator was employed to reverse the connections of the internal wires in which the current was induced, and of the external circuit. There were then broadly two classes of dynamoelectric machines—the simplest, the alternating-current machine, where no commutator was used; and the continuous-current machine, in which a commutator was used to change the connection with the external circuit just at the moment when the direction of the current would change. The theory of the alternate-current machine was then explained, and it was proved that two independently-driven alternate-current machines could not be worked in series, but that they might be worked in parallel circuit, and hence were quite suitable for distribution of electricity for lighting without the necessity of providing a separate circuit for each machine.

It was easy to see that, by introducing a commutator revolving with the armature, in an alternate-current machine, and so arranged as to reverse the connection between the armature and the external circuit just at the time when the current would reverse, it was possible to obtain a current constant always in direction; but such a current would be far from constant in intensity, and would certainly not accomplish all the results obtained in modern continuous-current machines. This irregularity might, however, be reduced to any extent by multiplying the wires of the armature, giving each its own connection to the outer circuit, and so placing them that the electromotive force attained a maximum successively in the several coils. A practically uniform electric current was first commercially produced with the ring armature of Pacinotti, as perfected by Gramme. A dynamo-machine was not a perfect instrument for converting mechanical energy into the energy of electric current. Certain losses inevitably occurred. There was the loss due to friction of bearings, and of the collecting-brushes upon the commutator; there was also the loss due to the production of electric currents in the iron of the machine. When these were accounted for, there remained the actual electrical effect of the machine in the conducting wire; but all of this was not available for external work. The current had to circulate through the armature, which inevitably had electrical resistance; electrical energy must therefore be converted into heat in the armature of the machine. Energy must also be expended in the wire of the electromagnet which produced the field, as the resistance of this also could not be reduced beyond a certain limit. The loss by the resistance of the wires of the armature and of the magnets greatly depended on the dimensions of the machine. To know the properties of any machine thoroughly, it was not enough to know its efficiency and the amount of work it was capable of doing; it was necessary to know what it would do under all circumstances of varying resistance or varying electromotive force; and, under any given conditions, what would be the electromotive force of the armature? Now this electromotive force depended on the intensity of the magnetic field, and the intensity of the magnetic field depended on the current passing round the electromagnet and the current in the armature. The current then in the machine was the proper independent variable in terms of which to express the electromotive force. The simplest case was that of the series-dynamo, in which the current in the electromagnet and in the armature was the same, for then there was only one independent variable. The relation between electromotive force and current might be most conveniently expressed by a curve.

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[1] Abstract of lecture delivered at the Institution of Civil Engineers on Thursday evening, April 5, by Dr. John Hopkinson, F.R.S., M Inst.C E.

When four years ago the lecturer first used such a curve (since named by Deprez the "characteristic curve") for the purpose of expressing the results of his experiments on the Siemens dynamo-machine, he pointed out that it was capable of solving almost any problem relating to a particular machine, and that it was also capable of giving good indications of the results of changes in the winding of the magnets, or of the armatures of such machines. The use of the characteristic curve was illustrated with reference to charging accumulators and Jacobi's law of electric transmission of power.

When the dynamo-machine was not a series-dynamo, but the current in the armature and in the electromagnet, though possibly dependent upon each other were not necessarily equal, the problem was not so simple. In that case there were two variables, the current in the electromagnet and the current in the armature; and the proper representation of the properties of the machine would be by a characteristic surface, of which a model was exhibited. By the aid of such a surface any problem relating to a dynamo-machine could be dealt with, no matter how its electromagnets and its armature were connected together. Of course in actual practice the model of the surface would not be used, but the projections of its sections.

The properties of a machine depended much upon its dimensions. Suppose two machines alike in every particular, excepting that the one had all its linear dimensions double that of the other. The electrical resistances in the larger machine would be one-half those of the smaller. The current required to produce a given intensity of magnetic field would be twice as great in the larger machine as in the smaller. The comparative characteristic curves of the two machines when driven at the same speed were shown in a diagram. The two curves were one the projection of the other, having corresponding points with abscisses in the ratio of one to two, and the ordinates in the ratio of one to four. At first sight it would seem that the work done by the larger machine should be thirty-two times as much as that which would be done by the smaller. Practically, however, no such result could possibly be attained for many reasons. First, the iron of the magnets became saturated, and consequently, instead of eight times the electromotive force, there would only be four times the electromotive force. Secondly, the current which the armature could carry was limited by the rate at which the heat generated in the armature could escape. Again, the larger machine could not run at so great an angular velocity as the smaller one. And lastly, since in the larger machine the current in the armature was greater in proportion to the saturated magnetic field than in the smaller one, the displacement of the point of contact of the brushes with the commutator would be greater. Shortly, the capacity of similar dynamo-machines was pretty nearly proportionate to their weight, that was to the cube of their linear dimensions; the work wasted in producing the magnetic field was directly as the linear dimensions; and the work wasted in heating the wires of the armature was as the square of the linear dimensions.

A consideration of the properties of similar machines had another important practical use. Mr. Froude was able to control the design of ironclad ships by experiments upon models made in paraffin wax. It was a much easier thing to predict what the performance of a large dynamo-machine would be, from laboratory experiments made upon a model of a very small fraction of its dimensions. As a proof of the practical utility of such methods, the lecturer stated that by laboratory experiments he had succeeded in greatly increasing the capacity of the Edison machines without increasing their cost, and with a small increase of their percentage of efficiency, remarkably high as that efficiency already was.

The electric properties of the electric arc were experimentally illustrated; in particular it was shown that the difference of potential between the carbons was nearly independent of the current.

When a current of electricity passed through a continuous conductor it encountered resistance, and heat was generated, as shown by Joule, at a rate represented by the resistance multiplied by the square of the current. If the current was sufficiently great, heat would be generated at such a rate that the conductor would become incandescent and radiate light. Attempts had been made to use platinum and platinum iridium as the incandescent conductor. But these bodies were too expensive for general use, and besides that, refractory though they were, they were not refractory enough to stand the high temperature required for incandescent lighting, which should be economical of power. Commercial success was not realised until very thin and very uniform threads or filaments of carbon were produced and inclosed in reservoirs of glass, from which the air was exhausted to the utmost possible limit. Such were the lamps made by Mr. Edison with which the Institution was temporarily lighted. The electrical properties of such a lamp were examined, and in particular it was shown that its efficiency increased and its resistance diminished with increase of current.

The building was lighted by about 230 lamps, each giving sixteen candles light, produced each by 75 Watts of power developed in the lamp. To produce the same sixteen candles' light in ordinary good flat-flame gas-burners, would require between 7 and 8 cubic feet of gas per hour, contributing heat to the atmosphere at the rate of 3,400,000 foot-pounds per hour, equivalent to 1250 Watts, or nearly seventeen times as much heat as the incandescence lamp of equal power.

At the present time, lighting by electricity in London must cost something more than lighting by gas. What were the prospects of reduction of this cost? Beginning with the engine and boiler, the electrician had no right to look forward to any marked and exceptional advance in their economy. Next came the dynamo, the best of these were so good that there was little room for economy in the conversion of mechanical into electrical energy; but the prime cost of the dynamo-machine was sure to be greatly reduced. Hope of considerably increased economy must be mainly based upon probable improvements in the incandescence lamp, and to this the greatest attention ought to be directed. It had been shown that marked economy of power could be obtained by working the lamps at high pressure, but then they soon broke down. In ordinary practice, from 140 to 200 candles were obtained from 1 horse-power, developed in the lamps, but for a short time he had seen over 1000 candles per horse-power from incandescence lamps. The problem, then, was so to improve the lamp in details, that it would last a reasonable time when pressed to that degree of efficiency. There was no theoretical bar to such improvements, and it must be remembered that incandescence lamps had only been articles of commerce for about three years, and already much had been done. If such an improvement were realised, it would mean that it would be possible to get five times as much light for a sovereign as could be done now. At present electric lighting would succeed commercially where other considerations than cost had weight. Improvements in the lamps were certain, and there was a probability that these improvements might go so far as to reduce the cost to one-fifth of what it now was. He left the meeting to judge whether or not it was probable, nay, almost certain, that lighting by electricity was to be the lighting of the future.

HARDENING AND TEMPERING STEEL

ONE of a series of lectures to the Liverymen and Apprentices of the Company of Cutlers of London was delivered on Thursday last by Prof. W. Chandler Roberts, F.R.S., "On some Theoretical Considerations connected with Hardening and Tempering Steel."

The Master of the Company, Mr. J. Thorne, presided, and the Lecturer observed that the phenomena with which they had to deal, although admittedly as interesting and remarkable as any in the whole range of metallurgy, are but little understood.

If the fact that steel can be hardened had not been known, the whole course of our industrial and even political history would probably have been widely different, and the dagger, which occupies so prominent a place in the armorial bearings of the City of London, would have represented a survival of implements made, not of steel, but of copper hardened with tin.

It has long been known that there are extraordinary differences between the properties of wrought iron, steel, and cast iron, but our knowledge that these differences depend upon the presence or absence of carbon is only a century old, for it was not until the year 1781 that Bergman, Professor in the University of Upsala, showed that wro ght iron, steel, and cast iron, when dissolved in certain acids, leave amounts of a graphitic residue, varying from $\frac{1}{10}$ to $2\frac{1}{2}$ per cent., which are essential to the constitution of these three varieties of metal. Bergman's work led many early experimenters, notably Clouet in 1796, to attempt to establish the importance of the part played by carbon, and Clouet converted pure iron into steel by contact at a high temperature with the diamond, which was the purest form of carbon he could command. Prof. Roberts said that this experiment had been repeated by many other observers with varying success, as in all the earlier work the furnace gases, which had not been excluded, might have converted the iron into steel without the intervention of the diamond. It remained for a distinguished Master of the Cutlers' Company, Mr. W. H. Pepys, to repeat Clouet's fundamental experiment under conditions which rendered the results unequivocal, by employing electricity as a source of heat. This experiment, which had been communicated to the Royal Society in 1815, was performed in the way Pepys had indicated.

It was then shown that in soft, tempered, and hardened steel respectively the carbon has a distinct "mode of existence," as is indicated by the widely different action of solvents on the metal in these three states.

The evidence as to whether carbon in steel is *combined* in the chemical sense, or is merely *dissolved*, was then considered at some length, special reference being made to the results obtained by various experimenters, from Berzelius and Karsten to Sir Frederick Abel of the War Department.

Prof. Roberts stated that the researches of Troost and Hautefeuille afforded strong evidence that in "white cast-iron" and steel the carbon is merely dissolved, a view which he adopted, as he did not consider it to be at all in opposition to the facts recently established by Sir Frederick Abel, who had shown that the carbon may be left by the slow action of solvents on soft steel as a carbide of iron.

The various physical, as distinguished from the chemical theories that had been propounded from the time of Réaumur, (1722) to that of Akerman (1879), to account for the "intimacy of the relation" of carbon and iron in hard as compared with soft steel, were then described at some length, and the remarkable experiments of Réaumur, who cooled steel slowly in a Torricellian vacuum in order to show that the absorption of gas did not take place during cooling, was illustrated.

In recent years much importance has been attached to the physical evidence as to the peculiar constitution of steel, and it has been shown that there is a remarkable relation between the amount of carbon contained in different varieties of steel and their electrical resistance. Some of the very interesting experiments of Prof. Hughes on this point were then exhibited and described, and Prof. Roberts concluded by saying that the value of the early work by Bergman and Réaumur had rather been lost sight of in recent discussions, Bergman's work being specially remarkable, as he attempted, by thermometric measurement, to determine the heat equivalent of the phlogiston he believed iron and steel to contain.

The importance of the degree of carburisation of steel from the point of view of its technical application was illustrated by reference to a series of curves, and it was incidentally mentioned that, in the case of the variety of steel used for the manufacture of coinage-dies, the presence of $\frac{1}{10}$ per cent. of carbon more or less than a certain standard quantity makes all the difference in the quality of the metal.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The new Board of the Faculty of Natural Science has issued its first list of lectures this term. The lectures are divided under the following heads:—Physics, Chemistry, Animal Morphology, Geology, and Botany. No lectures are scheduled this term under Mineralogy or Physiology.

In Physics Prof. Clifton lectures on "Instruments and Methods of Measurement employed in the Study of Optics." These lectures are given in the Clarendon laboratory, where practical instruction in Physics is given by the Professor, assisted by Messrs. Stocker and Heaton. At Christ Church Mr. Baynes lectures on Electrokinematics and Electrodynamics, and gives practical instruction on Electric and Magnetic Measurements. At Balliol Mr. Dixon gives a course of experimental lectures on Elementary Heat and Light.

In Chemistry Dr. Odling lectures at the Museum on the Composition of Air and Water; Mr. Fisher lectures on Inorganic Chemistry; and Dr. Watts on the Cyanogen Series. At Christ Church Mr. Harcourt has a class for Quantitative Analysis, and Mr. Dixon a class for Gas Analysis.

In Animal Morphology Prof. Moseley lectures on Comparative Anatomy, and gives practical instruction to his class after each lecture; Mr. Hickson lectures on the Development of the Chick, Mr. Hatchett Jackson on Mammalian Osteology and the Principles of Embryology, Mr. Poulton on the Distribution of Animals, and Mr. Lewis Morgan on the Vertebrate Exoskeleton and on Human Osteology.

In Botany Mr. Chapman gives practical instruction on Vegetable Morphology at the Botanic Gardens.

In Geology Prof. Prestwich will give a series of lectures on Friday afternoons on the strata and fossils to be visited on his Saturday excursions.

On June 19 an examination will be held in common by Magdalen, Merton, and Corpus Christi Colleges for electing a Scholar in Physical Science at each College. At Merton and Corpus the chief subjects will be Chemistry and Physics.

Jesus College offers a Welsh Scholarship in Natural Science. The examination will be held on June 14.

Examinations for the degree of Bachelor of Medicine (both First and Second) will be held this term. Candidates are to send in their names before May 1.

CAMBRIDGE.—Prof. Huxley's Rede Lecture at Cambridge University will be given on June 12, at 3 p.m., in the Senate House. The subject is not yet announced.

Dr. Michael Foster leaves the Lectures on Elementary Biology for this term in the hands of Dr. Vines and Mr. Sedgwick, and will hold Catechetical Classes in Physiology for the Natural Sciences Tripos.

Dr. F. Darwin will give six Demonstrations on the Physiology of Plants (Growth, Movement, &c.) at the Physiological Laboratory on Saturdays at noon, beginning April 21.

Prof. Liveing will lecture on the Chemistry of the Heavenly Bodies, beginning May 1.

LONDON.—Mr. A. H. Keane has been appointed to the Hindustani Lectureship at University College.

THE Winter Session at the College of Agriculture, Downton, near Salisbury, ended on Monday, when the certificates and prizes were presented to the successful students by Archdeacon Sanctuary. The certificate of membership, obtainable on examination after completion of the two years' course of study, was granted to Mr. Arthur Herbert Kerr, Crookham, Farnham, and to Mr. Henry Blair Mayne, Brantridge Park, Balcombe. The Scholarship, open to first-year students, was awarded to Mr. Robert Alan Benson, Clifton.

SOCIETIES AND ACADEMIES

London

Mathematical Society, April 12.—Prof. Henrici, F.R.S., president, in the chair.—The Chairman announced that Prof. Rowe, of University College, London, had been elected a Member of the Council in the room of the late Prof. Henry Smith, F.R.S.—The following communications were made:—Equations of the loci of the intersections of three tangent lines and of three tangent planes to any quadric $u = 0$, Prof. Wolstenholme.—Investigation of the character of the equilibrium of an incompressible heavy fluid of variable density, Lord Rayleigh, F.R.S.—On the normal integrals connected with Abel's theorem, Prof. Forsyth.—Spherical functions, Part 1, Rev. M. M. U. Wilkinson.—Calculation of the equation which determines the anharmonic ratios of the roots of a quintic, Prof. M. J. M. Hill.—On simultaneous differential equations, with special reference to (1) the roots of the fundamental determinant, (2) the method of multipliers, Mr. E. J. Routh, F.R.S.

Chemical Society, April 5.—Dr. W. H. Perkin, president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting of the Society (April 19).—The following papers were read:—On the estimation of hydrogen sulphide and carbonic anhydride in coal-gas, by L. T. Wright. The coal-gas, dried and freed from ammonia, is passed through two weighed U-tubes, the first containing precipitated cupric phosphate dried at 100° and calcium chloride, the second, soda lime, slightly moist, and calcium chloride. Three cubic feet of clean coal-gas are first passed through the U-tubes to "saturate" the reagents. The increase of weight of the first U-tube, after the passage of the crude coal-gas, then gives the hydrogen sulphide, and the increase in weight of the second the carbonic anhydride.—Some compounds of antimony and bismuth containing two halogens, by R. W. Atkinson.—On the theory of a molecular combination, when antimonious chloride is mixed with potassium bromide and antimonious bromide with potassium chloride, two distinct compounds should be produced. The author finds that but one is formed, the two compounds being identical in composition as well as in colour, crystalline form, and other physical characters. This body has the formula $Sb_2Cl_6Br_6K_6 + 3H_2O$. An attempt to form the corresponding bismuth compound was not successful.—Contribution to the chemistry of the cerite metals, by B. Brauner. The author has determined the atomic weight of didymium with the greatest care, and fixes it at 145·4; the higher numbers previously obtained were due to the presence of a metal having a higher atomic weight; this metal is proved by the author to be samarium, the atomic weight of which he calculates to be 150. The author also proves that the principal gadolinite earths—yttria, terbia, erbia, &c.—are present in cerite, but not in large quantities.

Institution of Civil Engineers, April 3.—Mr. Brunlees, president, in the chair.—The paper read was "On the Summit-Level Tunnel of the Bettws and Festiniog Railway," by Mr. William Smith, M.Inst.C.E.

April 10.—Mr. Brunlees, president, in the chair.—The paper read was on "The Introduction of Irrigation into New Countries, as illustrated in North-Eastern Colorado," by Mr. P. O'Meara, M.Inst.C.E.

Edinburgh

Royal Society, April 2.—Mr. John Murray in the chair.—Dr. Gibson, in a communication on some laboratory arrangements, described and exhibited a modification of Bunsen's method of filtration. The modification consisted essentially in placing the vessel which received the filtrate inside a bell-jar, which was connected with the exhausting apparatus and perforated above so as to admit the funnel through which the liquid filtered. By a suitable three-way stopcock arrangement the adjusting of the internal partial vacuum was kept quite under the control of the experimenter. A contrivance for the more convenient use and better preservation of sulphuretted hydrogen water was also described and shown.—Prof. Tait, in a short note on the thermoelectric position of pure cobalt, described recent experiments which fully bore out results formerly obtained with other specimens. The cobalt line runs nearly parallel to the iron line, but far down on the diagram below palladium and nickel. Prof. Tait also indicated the solution of certain problems of heat conduction in heterogeneous bodies as affected by the Peltier and Thomson effects.—Prof. George Forbes read a paper on transmission of power by alternate currents, in which he pointed out the value of alternate current machines as electromotors, especially in cases in which perfect isochronism was of importance.—Prof. Herdman, in a paper on the so-called hypophysis in the Tunicata, described the structure of the neural (hypophysal) gland and the dorsal tubercle in various Ascidians, and suggested that possibly the connection of the neural gland (and also of the vertebrate hypophysis cerebri) with the pharynx might be a secondary modification caused by one or more of a series of primitive lateral excretory ducts, opening either upon the exterior of the body or into the peribranchial cavity, having come to open into a lost sense organ, in the Stomodæum represented by the dorsal tubercle. These lateral ducts are found in *Ascidia mammillata*, in some cases existing along with a median duct opening into the pharynx at the dorsal tubercle, and in other cases without this connection with the supposed sense-organ.—Prof. Tait presented a paper on the quaternion expression for the displacements of a rigid system, by Dr. G. Plarr.

Mathematical Society, April 13.—Mr. A. J. G. Barclay, M.A., in the chair.—Mr. J. S. Mackay, president, read a paper on the triangle and its six-scribed circles, adding historical notes on the discovery of the various properties enumerated. The name *medioscribed* circle (il circolo medioscritto) was suggested for use instead of nine-point circle, as had been proposed twenty years ago by G. B. Marsano, "Considerazioni sul Triangolo Rettilineo," Genova, 1863, p. 11.

Berlin

Physiological Society, March 9.—Prof. Du Bois Reymond in the chair.—Dr. Wernicke gave a short sketch of the illness of a patient who fell sick, exhibiting all the symptoms of a cerebral tumour except epileptic attacks, and who manifested a disturbance of speech that was characterised by Dr. Wernicke as a "sensorial aphasia," and by others as "word-deafness." A sensorial aphasia consists, according to Dr. Wernicke, in the fact that the patients, though in possession of a large vocabulary, no longer understand the meaning of words, that they use these confusedly, and so that their speech is quite muddled; moreover they do not understand what one says to them at all, so that it is impossible to arrive at an understanding with them. The patient in question soon succumbed to an intercurrent disease, and it was possible to make a thorough dissection of the brain, which exhibited a bilateral affection of the cerebral cortex at the first temporal convolution. An accurate dissection of the ears showed that the deafness that had been observed during life was not brought about by any disease of the sound-conducting apparatus, but that it was rather to be regarded as a central deafness conditioned by the disease of the cortex of the first spheno-semporal convolution in which, as Dr. Wernicke made probable to long as ten years ago, the terminal expansion of the acoustic nerve has its seat. Now the local disease of the brain-cortex and the consequent observed disturbances in hearing and speech correspond to the manifestations of "soul-deafness" that were experimentally produced by Dr. Munk in animals by extirpation of the auditory sphere (*Horsphäre*), and consequently establish the results of experiments on animals as true for man also. The total deafness of the patient had only set in at a later period towards the end of the disease, when the affection of the brain had passed from the cortex into the deeper structures and had destroyed the acoustic fibres. The physiological import of the above case consists in the clearly proved limitation of the disease to the first temporo-sphenoidal convolution in a case where the clinical phenomena corresponded accurately to those of "soul-deafness."—Dr. J. Munk had found in previous experiments that the function of neutral fats in nutrition can just as well be performed by the fatty acids. Animals manifested absolutely no disturbance of nutrition when supplied with fatty acids instead of fats; the fatty acids were made into an emulsion, and absorbed by the villi in precisely the same fashion as the fats, and afterwards the chyle-vessels were found just as densely filled with a milky fluid as after a meal of fat. The examination of the chyle had, however, shown that the fatty acids that were supplied were no longer to be found, but only neutral fats, and hence Dr. Munk had assumed that a synthesis of neutral fats took place as the fatty acids passed out of the intestinal villi into the chyle, and that the glycerine was supplied by the animal body, probably by the breaking down of albumen. Dr. Munk has only just lately been able to advance a proof of the truth of this assumption of a synthesis of neutral fat out of fatty acids after it had been shown by other observers that heterogeneous neutral fats could be taken up by animals and also be deposited as such in the body. Dr. Munk now fed a dog, which had been greatly reduced in weight by prolonged starvation, with large quantities of the fatty acids of mutton and with a little lean meat. The animal very soon increased considerably in weight upon this diet, and after fourteen days had deposited 1100 grms. of fat in various organs under the skin, in the mesentery, in the heart, and in the liver. Analysis of this fat elicited the fact that it consisted of at least 96 per cent. of neutral mutton fat. And in the dog it is evident that the mutton fat could only have arisen by a synthesis of the fatty acids of mutton that were eaten.

PARIS

Academy of Sciences, April 9.—M. Blanchard in the chair.—The following papers were read:—On charbonous vaccination, by M. Pasteur. Some Turin professors having found that vaccinated as well as unvaccinated sheep died after virulent inoculation, M. Pasteur made inquiry, and came to the conclusion that the blood used for such inoculation was septic as well as charbonous (the sheep was dead twenty-four hours before its blood was taken). He challenges the Turin men to a test of this.—Description of a means of obtaining a wholly automatic action of the sluice with oscillating liquid columns, without cataract; experimental realisation of this system during the emptying of the sluice of l'Aubois, by M. de Caligny.—Units of mechanics and of physics, by M. Ledieu.—The salt lands of the South-East, by M. de Gasparin. The problem of freeing such ground from salt is (unlike the formation of a *polder*) an indeterminate one, and may be insoluble; many years' submersion and drainage may be ineffectual.—Report on electrodynamic machines applied to the transmission of mechanical work, by M. Marcel Deprez. The dynamometric return (viewed apart from the mechanical motor) was over 48 per cent.—On surfaces with *nil* mean curvature, on which may be limited a finite portion of the surface by four straight lines situated on the surface, by M. Schwarz.—A letter of invitation to the Institute to the second session of the Royal Society of Canada at Ottawa, on May 22, was read.—Observation of the transit of Venus at Punta-Arenas (Straits of Magellan), by M. Cruls. The four contacts were observed under excellent conditions.—Observations of the Swift-Brooks comet, by M. Périgaud.—Observations of comet II., 1882, at Algiers Observatory, by M. Trépied.—On uniform functions affected by sections, and on a class of linear differential equations, by M. Appell.—Law of periods, by M. de Jonquières.—Remarks on the primitivity of groups, by M. Dyck.—Determination of arithmetical progressions, whose terms are only known approximately, by M. Lucas.—On a theorem of M. Stieltjes, by M. Cesaro.—On an improvement applicable to the Jonval turbine, by M. Léauté.—On the radiation of silver at the moment of solidification, by M. Violle. The radiation decreases at first, more or less rapidly; then the decrease slackens, and when solidification begins at the border of the vessel, there is a slight increase: till solidification reaches the central part the radiation of the liquid remains constant, then there is slight increase, followed by rapid decrease.—On several optical apparatuses for testing plane surfaces, parallel, perpendicular, and oblique, by M. Laurent.—Very powerful direct-vision spectroscope, by M. Zenger. By adding to the dispersion parallelipiped a light crown glass prism, he gets a dispersion of 150° (A to H); this is surpassed only by M. Thollon's spectroscope, in which the number of sulphide of carbon prisms and the multiple reflections diminish greatly the intensity of the light.—On the upper limit of the perceptibility of sounds, by M. Pauchon. He used a steam-driven syren, also metallic rods of diminishing length fixed at one end, and rubbed. *Inter alia*, an acoustic cornet slightly removed the limit of perceptibility; exciting the rods with various substances (colophany, alcohol, &c.) also changed the limit-length, sometimes to the extent of double. A sound that had become too high for the ear still acted on a sensitive flame.—On a process for obviating boiler explosions, by M. Trèves. He recommends a thermomanometer, and a methodic feeding according to it; also the introduction of a tube for injection of air.—On some experiments made with dynamoelectric machines, by M. Pollard.—Reply to M. Reynier, by M. Trouvé.—Production of crystalline vanadates by the dry way, by M. Ditte.—Action of sulphur on alkaline phosphates, by MM. Filhol and Senderens.—On a combination of phosphoric acid and silica, by MM. Hautefeuille and Margottet. The formula is PhO_5SiO_2.—On various kinds of borotungstates, by M. Klein.—Application of the phenomena of supersaturation to the theory of hardening of some cements and mastics, by M. Le Chatelier.—On chloride of pyrosulphuryl, by M. Konowaloff.—On the difference of reactional aptitude of halogen bodies in mixed halogen ethers; first part, ethylenic compounds, by M. Henry.—On liquid chlorhydrates of turpentine, by M. Barbier.—The structure of the ovary and formation of eggs in the Phallusiadeæ, by M. Roule.—On the organs of flight in insects, by M. Amans. In both theories of flights he considers (M. Marey's and Mr. Pettigrew's) it is overlooked that the base of the wing is formed of two planes, with obtuse angles, so that in the descending stroke the posterior plane presents its concavity to the column of air struck; the resultant, on the two axillæ, raises the bird.—On the trichomatic origin and formation of some cystoliths, by M. Chareyre.—Physiological researches on Champignons, by MM. Bonnier and Mangin. The ratio of oxygen absorbed to carbonic acid emitted does not vary sensibly with the temperature in a given species. Respiration increases very sensibly with the hygrometric state of the air, diminishes in diffused light, is greatest in the more refrangible rays. Transpiration is greater in diffuse light than in darkness.—Scientific exploration in the Straits of Magellan, on Terra-del-Fuego, and on the coast of Patagonia, with the Brazilian corvette *Parnahyba*, by M. Cruls.—The perception of colours and the perception of differences of brightness, by M. Charpentier. The perception of colour is merely the appreciation of the difference of excitation, by certain rays, of the apparatus of luminous sensibility on the one hand, and of that of visual sensibility, or distinction of forms, on the other.—Experimental researches on the physiological effects of cinchonidine, by MM. Sée and Bochefontaine. Its place (with quinine and cinchonine) is among substances which depress the nervous system after momentary stimulation of the circulation.—On the effects of prolonged stay in an atmosphere charged with vapours of creosote, by M. Poincaré. There was hardening of the brain, sclerosis of liver and kidneys, effacement of the pulmonary cavities, &c.—On the circulation of the fingers and derivative circulation of the extremities, by M. Bourceret. In the last phalanx of the fingers there is a special arrangement for rapid return of the blood; it consists of large, very short capillaries, and is merely a modification of the general type. One cannot speak properly of a derivative circulation.—On the attenuation of the virulence of the bacterium of charbon by antiseptic substances, by MM. Chamberland and Roux. This was proved with carbolic acid and bichromate of potash.

CONTENTS

PAGE
THE SCOTCH UNIVERSITIES BILL 573
THE SCHEME OF THE GROCERS' COMPANY FOR THE ENCOURAGEMENT OF ORIGINAL RESEARCH IN SANITARY SCIENCE 574
ELEMENTARY METEOROLOGY 575
SALVADORI'S PAPUAN ORNITHOLOGY 577
OUR BOOK SHELF:—
  Smith's "Cutting Tools Worked by Hand and Machine" . . . 577
LETTERS TO THE EDITOR:—
  Metamorphic Origin of Granite.—Prehistoric "Giants."—THE DUKE OF ARGYLL 578
  "The Ether and its Functions."—S. TOLVER PRESTON 579
  "Krao."—A RESIDENT 579
  Singing, Speaking, and Stammering.—JAMES LECKY; W. H. STONE, M.B., F.R.C.P., K. 580
  A Curious Case of Ignition.—Lieut. BERTRAM GWYNNE, R.N. . . 580
  Fibreballs.—J. H. 580
  Benevolence in Animals.—OSWALD FITCH 580
  The Zodiacal Light (?)—J. W. B 580
  Braces or Waistband?—G. H. 580
THE TEACHING OF ELEMENTARY MECHANICS 580
THE CHEMISTRY OF THE PLANTÉ AND FAURE ACCUMULATORS, V. By Dr. J. H. GLADSTONE, F.R.S., and Dr. ALFRED TRIBE, F.R.S. . . 583
THE LION AT REST (*With Illustration*) 584
ON THE RELATIONS OF THE FIG AND THE CAPRIFIG. By W. BOTTING HEMSLEY 584
NOTES 587
OUR ASTRONOMICAL COLUMN:—
  D'Arrest's Comet 589
  The Great Comet of 1882 589
  The Binary Star *p* Eridani 589
GEOGRAPHICAL NOTES 589
THE SOARING OF BIRDS. By Dr HUBERT AIRY 590
SOME POINTS IN ELECTRIC LIGHTING. By Dr JOHN HOPKINSON, F.R.S., M.Inst.C.E. 592
HARDENING AND TEMPERING STEEL. By Prof. W. CHANDLER ROBERTS, F.R.S. 594
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 594
SOCIETIES AND ACADEMIES 595

THURSDAY, APRIL 26, 1883

SCIENTIFIC WORTHIES

XXI.—WILLIAM SPOTTISWOODE

WILLIAM SPOTTISWOODE, President of the Royal Society, was born in London, Jan. 11, 1825. He belongs to an ancient Scottish family, many members of which have risen to distinction in Scotland and also in the New World.[1] He was first sent to a private (we believe) school at Laleham under Mr. Buckland, brother of Dean Buckland. Here, we read, "the discipline was of a severity unknown at the present day." Thence he was removed to Eton, where however his stay was short. The poet writes, "the child is father of the man"; but science in those days did not hold the place it does now in the scholastic curriculum, and so the future President, venturing to make some researches into the effects produced by the combination of various detonants, came into collision with the "powers that be";[2] the upshot of this *contretemps* was that the brothers Spottiswoode were transferred to Harrow, then under the rule of the present Bishop of Lincoln. His house tutor was Mr. Harris, of the Park.[3] On entrance he was placed in the upper shell, a high form in those days for a newcomer: here he was a very studious, quiet, and thoughtful boy, not much given to athletic games. He remained at Harrow three years, and in 1842 obtained a Lyon Scholarship.[4] In this same year he entered Balliol College, Oxford, and had the present Bishop of Exeter for his mathematical tutor; subsequently, in 1845, the last year of his residence as an undergraduate, he read with the Rev. Bartholomew Price, of Pembroke College. This gentleman writes: "He showed extraordinary liking for, and great skill in, what I may call the morphology of mathematics, such as the theory of simultaneous equations and the results deducible from the *form* of these equations, a department in which he has since shown great ability. He had, I think, greater taste for these branches in their algebraical and geometrical developments than for any other. His power of work was very great and his industry equally so; he read a great deal outside the usual range." In 1845 he took a first class in mathematics, and he afterwards won the Junior (1846) and Senior (1847) University Mathematical Scholarships. He returned to Oxford for a term or two, and gave a course of lectures in Balliol College on Geometry of Three Dimensions—a favourite subject of his. He was Examiner in the Mathematical Schools in 1857-58.[5] On leaving Oxford, he immediately, we believe, took an active part in the working management of the business of the Queen's printers, about this time resigned to him by his father, Andrew Spottiswoode, brother of the Laird of Spottiswoode. The business has largely developed under his hands.

Other subjects than mathematics have occupied his attention: at an early age he studied languages, as well Oriental as European; of his acquaintance with these ample evidence is furnished by his contributions more particularly referred to below.

In 1856 Mr. Spottiswoode made a journey through Eastern Russia; of this he has published a graphic and, in parts, very lively account in his book entitled "A Tarantasse Journey through Eastern Russia in the Autumn of 1856" (Longmans, 1857). "I neither made the journey, nor do I now write, with any political object, but simply as a traveller to whom every square mile of the earth's surface is interesting, and the more so in proportion as it is less known."[1]

In 1860 the brothers Spottiswoode, accompanied by a sister, went through Croatia and Hungary.[2] In 1861 Mr. Spottiswoode married the eldest daughter of the late William Urquhart Arbuthnot, a distinguished member of the Indian Council. His exceptional qualifications as an organiser have not only served to advance his business in the way we have mentioned above, but these same qualifications, together with the broad and liberal education on which they were based, have combined to raise him to his present high position in science. As Treasurer and President he has been continuously on the Council of the Royal Society for a great many years, and through his exceptional gifts as an administrator he has rendered it invaluable services. He has rendered similar services to the British Association, to the London Mathematical Society, and to the Royal Institution.[3] We have permission to make the following extract from a letter written by a friend of many years standing: "In the councils (of the various societies) he has always been distinguished by his sound judgment and his deep sympathy with their purest and highest aims. There never was a trace of partisanship in his action, or of narrowness in his sympathies. On the contrary, every one engaged in thoroughly scientific work has felt that he had a warm supporter in Spottiswoode, on whose opportune aid he might surely count. The same breadth of sympathy and generosity of sentiment has marked also his relations to those more entirely dependent upon him. The workmen in his large establishment all feel that they have in him a true and trustworthy friend. He has always identified himself with their educational and social well-being."[4] We give here a list of some of the offices Mr. Spottiswoode has held, and of the honours that have been bestowed upon him: Treasurer of the British Association from 1861 to 1874, of the Royal Institution from 1865 to 1873, and of the Royal Society from 1871 to 1878. In 1871 he succeeded Dr. Bence

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[1] John Spottiswoode, born 1565, Archbishop of St. Andrews, "had few equals, and was excelled by none", another John (1616) was "a youth of extraordinary parts"; and Sir Robert Spottiswoode, second son of the Archbishop, was "a man of extraordinary parts, learning, and merit:" ("Genealogy of the Spotswood Family in Scotland and Virginia," by C. Campbell. Albany, 1868.)

[2] "The feeling and opinion" at Harrow "were that no blame whatever attached to them."

[3] One who knew Mr. Spottiswoode in his earliest days says: "Our numbers at the school were comparatively very small, but I remember well the great ease with which he did all his school work. I knew him well at Oxford, and he several times lent me his horse—a sturdy, Roman-nosed animal of great courage and strength—for a day's hunting. He rode but little himself, and did not read much in an orderly way." He also gives other particulars of interest, which we forbear to give here.

[4] A son of Bishop Colenso also obtained a scholarship in the same year. The mathematical prizes of the present day were not then founded, so that the name of Spottiswoode does not occur among the prizemen of that time.

[5] He also acted as an Examiner in the Civil Service Commission in its first year of operation, and subsequently for the Society of Arts, and also for the Cowper Street Middle-Class Schools.

[1] The hotel accommodation was of the scantiest (p. 23); the description of the vehicles is pleasanter to read than to realise. The only peculiarly personal statement is that the writer was a non-smoker. There are several illustrations by the author, and a route map of Russia.

[2] For a description, see a paper by Mr. G. A. Spottiswoode in Galton's "Vacation Tourist in 1860."

[3] He has, we believe, also rendered valuable services to the Astronomical and Geographical Societies.

[4] This last statement we have corroborated from other sources. "Spottiswoode's people" have "many institutions for healthful recreation as well as mental improvement, such as library, rowing and cricket clubs, a choral society, and a volunteer corps."

Jones as Honorary Secretary to the Royal Institution. *President* of Section A, 1865; of the British Association, 1878; of the London Mathematical Society, 1870 to 1872; of the Royal Society, 1879, which office he still holds. Correspondent of the Institut (Académie des Sciences), March 27, 1876. He is also LL.D. of the Universities of Cambridge, Dublin, and Edinburgh, D.C.L. of Oxford, and F.R.A.S., F.R.G.S., F R S.E. In addition to these honours he has many other literary and scientific distinctions.

Of Mr. Spottiswoode's willingness to communicate from his stores of knowledge many have had frequent proof. We are breaking no faith, we believe, when we mention that it was his wish to purchase the late Prof. De' Morgan's valuable library to present it to the Mathematical Society, of which that distinguished mathematician had been the first President.

Few students of the present day are acquainted with Mr. Spottiswoode's earliest work which appeared in the shape of five quarto pamphlets (136 pp. in all) with the title, "Meditationes Analyticæ" (London, 1847). The author's dedication runs thus: "To those who love to wander on the shore till the day when their eyes shall be opened and they shall see clearly the works of God in the unfathomed ocean of truth, these papers are inscribed;" and in his preface he says, "The following papers have been written at various periods, as the subjects presented themselves to notice from time to time. If leisure had been afforded, an attempt would have been made to draw some of them up into a distinct treatise; but it was thought that even in their present form they might interest some of those who take pleasure in the pursuit of mathematical science. Some of the papers are entirely original." The papers are entitled, "Symmetrical Investigations of Formulæ relative to Plane Triangles," "On some Theorems relative to Sections of Surfaces of the Second Order," "On the Reduction of the General Equation of the Second Order," "On the Partial Differential Equations of certain Classes of Surfaces," "On some Theorems relating to the Curvature of Surfaces," "On certain Formulæ for the Transformation of Coordinates," "On the Principle of Virtual Velocities," "On Infinitesimal Analysis," "Examples of the Application of the Infinitesimal Calculus," "On certain Formulæ made use of in Physical Astronomy," "On the Calculus of Variations," "Problems in the Calculus of Variations," and "Note on Lagrange's Condition for Maxima and Minima of Two Variables"—a fair epitome of his subsequent mathematical labours. The treatment calls for no special comment, except that we may note that "in the form of the equations symmetry has been preserved wherever the circumstances of the case would permit."

At a slightly later date (1851) appeared, of a uniform appearance with the "Meditationes," a much more notable pamphlet (63 and viii. pp.), "Elementary Theorems relating to Determinants," of which a writer remarks, "full of interest for the mathematician, but terrible to the unmathematical vision." A second edition of this, rewritten and much enlarged, was published in *Crelle's Journal* (vol. li. 1856, occupying pp. 209-271, 328-381).[1] This was the earliest elementary treatise on a subject which has since risen to such importance, and contains a good sketch of what had previously been done in the same direction. The friend, some of whose words we have already cited, remarks, "that Spottiswoode should have devoted himself at an early period to its cultivation is to me perfectly natural, for the prevailing character of all his mathematical work is *symmetry* (one might generalise still further indeed and say that it, combined with graceful elegance, is the salient feature of all his activity, mathematical, physical, and literary). Bertrand once said of Serret that he was '*un artiste en formules*,' and in a far more general sense one might say that Spottiswoode is the 'incarnation of symmetry.'" To go back to the criticism just now quoted, Mr. Spottiswoode is indeed a leviathan in symbols, and he takes his pastime amongst them: the "gay determinant" is a familiar form nowadays, and "Hamilton's weird delta turned" (the *Nabla* of Clerk Maxwell) is conspicuous on many a page devoted to physics, but in some of the papers we are about to describe there are not only inverted deltas, but Nablas turned to the right and to the left run riot on the pages.

It is since 1870 that Mr. Spottiswoode has more especially divided his attention between physics and mathematics. "His nearest friends," we are informed, "induced him to take up the less abstract one of these two branches of science in order that the general public might have better opportunities of appreciating his abilities. His work in the new field has been of the same character as in the former one. It aims less perhaps at exhaustive treatment than at a study of subtle and beautiful phenomena."

An early consequence of his new study was the publication in 1874, in the NATURE Series, of his "Polarisation of Light." This contains a popular exposition of the subject, and its pages "constitute a talk" with his work-people "rather than a treatise" on "this beautiful branch of optics."[1]

Before we give a list of the several papers which, of course, do not admit of quotation and passing over, as still within the recollection of most of our readers, the most admirable address delivered before the British Association at Dublin in 1878[2]—though one finds it hard to pass over the many brilliant passages, of more special interest however to the mathematician, who alone can be supposed to care for any other than the ordinary space of three dimensions—we must trespass to the extent of taking the following passage from the earlier address to Section A in 1865. This address, in the words of Prof. Sylvester, is a combined history of the progress of mathematics and physics, and of it Clerk Maxwell said he had endeavoured to follow Mr. Spottiswoode, "as with far-reaching vision he distinguishes the systems of science into which phenomena, our knowledge of which is still in the nebulous stage, are growing."

"A detailed summary of recent progress in pure mathematics would probably prove either interesting to the mathematician or unintelligible to the general hearer;

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[1] "On the request of the editor of this *Journal* to reproduce it he (Mr. Spottiswoode) requested permission to revise the work. The subject had, however, been so extensively developed in the interim, that it proved necessary not merely to revise but entirely to rewrite the work. The result is given in the following pages."

[1] He has also contributed a lecture on the same subject to the "Science Lectures at South Kensington" Series.

[2] See NATURE, vol xviii pp 404-415.

with a view to sparing the patience of both, I shall restrict myself to a few general remarks. In both the great branches of mathematics, viz. geometry and algebra, new schools have arisen within the last few years. In its primary aspect the movement has tended to separate the two; geometry has become more purely geometrical in its conceptions and methods, algebra more independent of geometrical considerations. The geometry of to-day is more like the Greek than was that of fifty years ago; and yet at the same time they have not only many principles really in common, but many methods which, although independent, are strictly analogous. Geometry regards its figures, algebra its forms, not as isolated individuals, but as associated with others (concomitants, as they are called) whose properties characterise those of their primitives. The principles of both may be regarded as the same, but dual in their application. Geometry, again, is dual within itself: points and lines may be so viewed that theorems concerning the one give rise to analogies concerning the other; the principle the same, but dual in its manifestation. In this way we seem to be rising to laws which transcend the distinctions between the two parts of geometry—between geometry and algebra.

"Descending a little further into particulars, in another way again we seem to be gaining some steps—but as yet only a few steps—towards a higher scheme both of geometry and algebra. There are a few certain relations so elementary in their conception, yet so universal in their application, that they seem capable of forming the basis of extensive theories: such, for example, in geometry, is that of Anharmonic Ratio—a particular kind of ratio applicable alike to points and rays, to lines and to angles, on which M. Chasles has founded his new and classical work on Conic Sections. Such, again, in algebra, are those of homogeneity and of symmetry, which prove to be not merely improvements in form, but actually new powers for progress in the hands of the mathematician. The calculus of homogeneous forms has marked a new era in the history of algebra; the theory of equations has been transfigured in its light; mechanics, both ordinary and molecular, have been elucidated by it; and the remote applications of the integral calculus have felt its ever-extending influence. Under these, as it were, new fundamental conceptions, whole theories may be coordinated, and of these, again, perhaps some coordination may one day be contemplated. As another instance of this generalisation of principles and of this dual aspect of the principles so generalised within almost the present generation, it has been discovered, or at all events been duly realised, that symbols of operation combine according to definite laws, comprising as a particular case those of ordinary number. This fertile idea has, year by year, been receiving fuller developments, till it has at last assumed the form of a complete calculus."

We, too, must join our apologies with those of the learned speaker for lingering so long upon a favourite subject.

The following is as complete a list of Mr. Spottiswoode's papers as we have been able to make:[1] they are grouped, not according to subjects nor in order of time, but as they occur in the several journals in which they originally appeared:—

Phil. Magazine.—(1) On the Equation $Q=q\,(\omega, x, y, z)=w+ix+jy+kz$ (vol. xxxvi. 1850); this is a theorem of considerable importance in the calculus of quaternions, and indeed essential for the application of that method to geometrical and physical problems. (2) On the Quaternion Expressions of Coplanarity and Homoconicism (*ib.*). (3) On the Geometrical Interpretation of Quaternions (vol. xxxvii. 1850), the working out on other lines of results stated in a previous volume by Prof. Donkin. (31) On a Geometrical Theorem (*ib.*, 1850), viz. if three cones of the second order, having a common vortex, intersect one another two and two, the nine lines of intersection (three being selected from each pair of cones) will lie on a cone of the fourth order. (7) On a Problem in Combinational Analysis (vol. iii. 1852) connected with the 15-girl Problem and a more general form of it, the solution of which turns upon certain determinants. (41) On some Experiments on Successive Polarisation of Light made by Sir C. Wheatstone (vol. xli. 1871); the introduction of instrumental means for converting the plane of polarisation of the ordinary apparatus into successive, or, as it is more commonly called, circular polarisation, and the explanation of the phenomena thence arising, constitutes the main purpose of the communication. See also *Proc. of R. Inst.*, vol. vi. 1872. 1875 (*a*) on a Revolving Polariscope; 1882 (*b*) on a Separator and Shunt for Alternate Currents.

Camb. and Dub. Math. Journal.—(4) On certain Geometrical Theorems (vol. vi. 1851). This is an anonymous article which gives simple algebraical demonstrations of certain of Steiner's Theorems in the *Systematische Entwickelung*, and also of some relations given by M. Chasles in his "Aperçu." (9) On Certain Theorems in the Calculus of Operations (vol. viii. 1853); an extension of theorems by Boole (*Phil. Trans.*, 1844), relating to the operation symbol $D=x\frac{d}{dx}$, and by Carmichael relating to the symbol $\nabla = x_1\frac{d}{dx_1} + x_2\frac{d}{dx_2}\ \ldots$ to the cases (1) in which the order of the Variables by which the Symbols of Differentiation are Multiplied is not the same as that of the Variables with respect to which the Differentiations are to be performed; (2) in which the Variables by which the Symbols of Differentiation are Multiplied are any linear Function of the Given Variables; (10) on Certain Geometrical Theorems (*ib.* 1853); two Elementary Theorems in anharmonics proved by aid of determinants. (11) On the Curvature of Curves in Space (vol. ix. 1854); on this M. Chasles ("Rapports," p. 162) remarks: "M. W. Spottiswoode est parvenu à la même expression dans une Note . . ." *i.e.* to the expression—

$$\frac{1}{\rho} = \operatorname{cosec}\phi\left\{\frac{1}{\rho^2} + \frac{1}{{\rho_1}^2} - \frac{2\cos\phi}{\rho\,\rho_1}\right\}^{\frac{1}{2}}.$$

Quarterly Journal of Mathematics.—(15) Note on Axes of Equilibrium (vol. i. 1857). The axes (Möbius, "Statik") possess the property of allowing the body to be turned about them, the forces retaining their directions in space without a disturbance of equilibrium. The paper is an application of formulæ given by Rodrigues to a proof of the property. (16) On a Theorem in Statics (*ib.* 1857) is a proof of the following, due to Möbius ("Statik"): "If there be any forces in equilibrium, and a series of pyramids be constructed having for one edge a common line, and for their opposite edges the lines which represent the forces, in both magnitude and direction, respectively, the algebraical sum of the volumes of the pyramids will vanish. It is of this M. Chasles ("Rapport," p. 59) writes: "M. W. Spottiswoode, à qui toutes les ressources des nouvelles théories de l'analyse sont si familières, s'est plu à les appliquer à la démonstration de cette proposition (*i.e.* Möbius's) et d'un autre passage du traité de statique de Mobius, sur les axes de l'équilibre." (23) On Petzval's Asymptotic Method of Solving Differential Equations (vol. v. 1862). Also in a somewhat different form in *Brit. Assoc. Report* (part ii.), 1861. (29)

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[1] We trust our readers will pardon our imperfect treatment of these papers: we had formed quite a mass of notes—a "rudis indigestaque moles"—but we have had, through circumstances over which we had no control, an utterly inadequate period in which to prune them and shape them into comely form The prefixed numbers are those of the "Royal Society's Catalogue" and the notes are in most cases derived from the papers themselves. In our haste we have preferred to insert notes to the less familiar papers; the papers read before the Royal and Mathematical Societies are without doubt those by which Mr Spottiswoode's rank as a mathematician has been determined, but these are just the ones that are most familiar to students.

On Differential Resolvents (vol. vi. 1863). A subject first brought into notice by Mr. J. Cockle, subsequently discussed by Rev. R. Harley. The functions considered are derived from equations in a factorial form; see also *Manch. Phil. Soc. Memoirs*, ii. 1865. [In the *R. S. Catalogue* these are also numbered (33)]. (37) Note on the Contact of Curves (vol. vii. 1866). "In my former paper" (*Phil. Trans.* 1862, see *infra*) "one set of expressions is unsymmetrical with respect to the variables; the other, although symmetrical, involves certain arbitrary quantities which remain to be eliminated by special methods in the course of the developments"—the object of the note is to establish general expressions which are both symmetrical and free from arbitrary quantities. (38) Note on the Resolution of a Ternary Cubic into Linear Factors (*ib.* 1866) is in effect a note on a paper by Mr. J. J. Walker in the previous volume, entitled "On the Resolution of Composite Quantities into Linear Factors."

Crelle's Journal.—(5) Mémoires sur les points singuliers d'une courbe à double courbure (vol. xlii. 1852). (6) Mémoire sur quelques formules relatives aux surfaces du second ordre (*ib.* 1852). (12) Correspondence between Prof. Donkin and Mr. Spottiswoode (vol. xlvii. 1854); extracts from letters (one from each) on a Method for Determining Two Cyclic Sections of a Surface of the Second Order. (14) The Memoir on Determinants (vol li. 1856). (25) Sur quelques formules générales dans le calcul des opérations (vol. lix. 1861), connected with a *Phil. Trans.* paper (17). In this he shows the method by which he obtained the formulæ in (17). (32) Note sur la transformation de la cubique ternaire en sa forme canonique (vol. lxiii. 1864).

Tortolini Annali di Scienze.—(8) Sulla trasformazione delle equazioni differenziali lineari dell' ordine secondo (vol. iii. 1852).

R. Soc. Proc.[1]—(13) Researches on the Theory of Invariants (vol. vii. 1854). "The view of invariants here taken has suggested a series of other functions of which invariants form the last term. These functions I propose to call *variants*. With the exact relation between these functions and covariants I am not at present acquainted." (17) On an Extended Form of the Index Symbol in the Calculus of Operations (vol. x. 1859, *Phil. Trans.* 1860). A more detailed form of (9). (20) On the Calculus of Functions (vol. xi. 1861). (21) On Internal and External Division in the Calculus of Symbols (*ib.*). Connected with a paper by Mr. W. H. L. Russell (*Phil. Trans.* 1861), a generalisation and an extension. (30) On the Equations of Rotation of a Solid Body about a Fixed Point (vol. xiii. 1863). In treating the equations of rotation of a solid body about a fixed point it is usual to employ principal axes of the body as the moving system of coordinates. Cases, however, occur in which it is advisable to employ other systems. The object of the paper is to develop the fundamental formulæ of transformation and integration for any system. [This is also given as (34) in the *R. S. Cat.*]. (35) On the Sextactic Points of a Plane Curve (vol. xiv. 1865; *Phil. Trans.* 1865). (40) On the Contact of Conics with Surfaces (vol. xviii. 1870; *Phil. Trans.* 1870). (43) On the Contact of Surfaces (vol. xx. 1872; *Phil. Trans.* 1872). (45) On the Rings Produced by Crystals when Submitted to Circularly Polarised Light (vol. xx. 1872); 1874 (*a*) On Combinations of Colour by Polarised Light; 1874-5 (*b*) On Stratified Discharges through Rarefied Gases; 1875-6 (*c*) On Multiple Contact of Surfaces; (*d*) An Experiment in Electromagnetic Rotation; 1876-7 (*e*) On Stratified Discharges (ii.); Observations with a Revolving Mirror (iii.); (*f*) On a Rapid Contact Breaker and the Phenomena of the Flow; 1877 (*g*) On Hyperjacobian Surfaces and Curves; (*h*) Stratified Discharges (iv.): Stratified and Unstratified Forms of the Jar-Discharge; (*i*) Photographic Image of the Stratified Discharge; 1878 (*j*) Stratified Discharge (v.); Discharge from a Condenser of Large Capacity; 1879 (*k*) On the Sensitive State of Electrical Discharges through Rarefied Gases [with J. F. Moulton], *Phil. Trans.*; 1879-80 (*l*) On some of the Effects Produced by an Induction Coil with a De Meriten's Magneto Electric Machine; (*m*) On the Sensitive State (ii.) (with J. F. M.); *Phil. Trans.*; 1880-1 (*n*) On the 48 Coordinates of a Cubic Curve in Space (*Phil. Trans.*); 1881 (*o*) On Stratified Discharges (vi.), Shadows of Striæ (with J. F. M.); and (*p*) Multiple Radiations from Negative Terminal; 1881-2 (*q*) Note on Mr. Russell's Paper on Definite Integrals; (*r*) Note on Mr. Russell's Paper on Certain Geometrical Theorems; (*s*) On the Movement of Gas in Vacuum Discharges (with J. F. M.).[1]

R. Asiatic Soc. Journal.—(18) Note on the supposed Discovery of the Principle of the Differential Calculus by an Indian Astronomer (vol. xvii. 1860). While not granting that Bhóskarácharya had discovered the principle, "it must be admitted that the penetration shown by him in his analysis is in the highest degree remarkable, and that the formula which he establishes and his method of establishing it bear more than a mere resemblance—they bear a strong analogy—to the corresponding process in modern astronomy." (28) On the "Súrya Siddhánta" and the Hindoo Method of calculating Eclipses (vol. xx. 1863). It had been suggested that Mr. Spottiswoode should undertake an edition of the above work. For reasons stated, the attempt was not made; but the object of this paper is the translation into modern mathematical language and formulæ of the rules of the work in question.

R. Geog. Soc. Proc.—(19) On Typical Mountain Ranges: an application of the Calculus of Probability to Physical Geography (vol. iv. 1861; *Journal*, vol. xxxi. 1861).

R. Astron. Soc. Memoirs.—(22) On a Method for determining Longitude by Means of Observations on the Moon's greatest Altitude (vol. xxix. 1861; also in *Geog. Soc. Proc.* vol. v. 1861).

British Assoc. Report.—(24) On the Reduction of the Decadic Binary Quantic to its Canonical Form (1861, part 2); (36) Address to Section A (1865); (*a*) Address to the Association (1878).

Phil. Trans.—(26) On the Contact of Curves (1862); (27) On the Calculus of Symbols (1862); 1874 (*a*) On the Contact of Quadrics with other Surfaces. See also above under *R. Soc. Proc.*

Comptes Rendus.—(39) Note sur l'équilibre des forces dans l'espace (vol. lxvi. 1868); (48) Note sur la représentation algébrique des lignes droites dans l'espace (vol. lxxvi. 1873); (49) Sur les plans tangents triples à une surface (vol. lxxvii. 1873); 1874 (*a*) Sur les surfaces osculatrices; 1875 (*b*) Sur la représentation des figures de géométrie à *n* dimensions par les figures corrélatives de géométrie ordinaire; 1876 (*c*) Sur le contact d'une courbe avec un faisceau de courbes doublement infini.

R. Inst. Proc.—(44) On Optical Phenomena produced by Crystals submitted to Circularly Polarised Light (vol. vii. 1872. See also *Phil. Mag.* vol. xliv. 1872); (46) On the Old and New Laboratories at the Royal Institution (vol. vii. 1873); (47) On Spectra of Polarised Light (*ib.* 1873); 1874 (*a*) On Combinations of Colour by Polarised Light; 1878 (*b*) A Nocturne in Black and Yellow; (*c*) Quartz: an old chapter rewritten; 1880 (*d*) Electricity in Transitu; 1882 (*e*) Matter and Magnetoelectric Action.

Musical Society Proc.—1879 (*a*) Lecture on Beats and Combination Tones.

Royal Society.—Presidential Addresses for the Years 1879, 1880, 1881, 1882.

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[1] When there is a paper in the *Phil. Trans.* as well, the reference is also given under this head.

[1] For analyses of the papers on "Sensitive Discharges," &c., consult vol. ii. of "A Physical Treatise on Electricity and Magnetism," by J. E. H. Gordon, 1880 (see pp. 47-50, 71-81, 88-111).

L. Math. Soc. Proc.—1866 (*a*) A Problem in Probabilities connected with Parliamentary Elections; 1868 (*b*) Equilibrium of Forces in Space; 1871 (*c*) Question in the Mathematical Theory of Vibrating Strings; 1872 (*d*) On some recent Generalisations in Algebra (Presidential Address); 1874 (*e*) On the Contact of Quadrics with other Surfaces; 1876 (*f*) On Determinants of Alternate Numbers; (*g*) On Curves having Four-point Contact with a Triply-infinite Pencil of Curves; 1879 (*h*) On the Twenty-one Coordinates of a Conic in Space; (*i*) On Clifford's Graphs; 1881 (*j*) On the Polar Planes of Four Quadrics; 1882 (*k*) On Quartic Curves in Space.

"A MANUAL OF THE INFUSORIA"

A Manual of the Infusoria; Including a Description of all known Flagellate, Ciliate, and Tentaculiferous Protozoa. By W. Saville Kent, F.L.S., F.Z.S. (London: David Bogue, 1882.)

THE *Philosophical Transactions of the Royal Society of London* for the year 1677 contain the first published account of the minute organisms to which the term "Infusoria" is now very generally applied. The account is by "Mr. Antony van Leeuwenhoek," who, taking up the line of study so successfully pursued by his compatriot, Swammerdam, was the first to apply the microscope to the investigation of the otherwise invisible fauna and flora which teem in inconceivable abundance in the waters of ponds, rivers, and seas, in the infusions of organic substances prepared by man's agency, and in even the minutest drops of moisture which accumulate on the surfaces of natural objects.

Henry Baker (1742), O. F. Muller (1773), and other names are connected with the history of this field of investigation in the period antecedent to Ehrenberg, who in 1836 gave a new aspect to the subject by his descriptions and figures of a great number of forms and of their intimate organisation. The minute creatures at one time spoken of as "animalculæ," and later as "Infusoria," are now known to comprise many very diverse series of organisms—unicellular plants, variously organised unicellular animals, as well as animals of multicellular structure and high organisation, although of minute size. The improvement of the microscope within the last forty years and the studies of a host of observers, among whom are Dujardin (1841), von Siebold (1845), Stein (1854), Claparède and Lachmann (1858), Max Schultze (1860), and more recently of Haeckel, Engelmann, and Butschli—have gradually resulted in the recognition of a series of minute animals included amongst the "animalculæ" and "Infusoria" of earlier writers, which are characterised by having their living substance in the form of one single nucleated corpuscle or "cell," whilst nevertheless exhibiting a considerable degree of organisation, possessing a mouth into which solid particles of food are taken, pulsating spaces within the protoplasm of the cell, special organs of locomotion, prehension, and protection. These are the mouth-bearing Protozoa, distinguished as such from the other unicellular animals which have not a special orifice for the ingestion of food and constitute the mouthless Protozoa.

It is to these mouth-bearing Protozoa and a few allied mouthless forms that Mr. Saville Kent restricts (as is not unusual) the old term Infusoria. Among them the most numerous and highly organised are the Ciliata; far less abundant and varied are the Tentaculifera (Acinetæ), whilst the Flagellata have, on account of their excessive minuteness, not been properly understood (and were for the most part altogether unknown) until very recently, some important features in their organisation having been first made known by the author of the book which forms the text of this article.

Mr. Kent's "Manual of the Infusoria" consists of two large volumes and an atlas of fifty-one plates. The first volume contains chapters on the history, the organisation, the affinities, and the classification of the Infusoria. Then the three classes, Flagellata, Ciliata, and Tentaculifera, are taken up one by one and systematically divided into orders and families, genera and species—a diagnosis and usually a figure being given of every species. The systematic treatment of the Ciliata and Tentaculifera occupies the second volume. Altogether Mr. Kent describes 900 species of Infusoria, arranged in 300 genera and 80 families. To go over this ground in any case involves a vast amount of labour and perseverance. To do so in the thorough and conscientious manner which distinguishes Mr. Kent's work requires special capacity. Mr. Kent has spared no pains to make his work a trustworthy source of information on all points relating to the group with which it deals; the most comprehensive as well as the smallest and most obscure of recent publications relating to the organisation or to particular species of Infusoria have their contents duly set forth in the proper place in Mr. Kent's work. So far as a frequent reference to these volumes enables one to come to a conclusion, little if anything of importance, whether published in English, French, German, or Italian, has been overlooked by our author. Even the quite recent observations of Foettinger on the parasitic *Benedenia* found in Cephalopoda, and of Joseph Leidy on the parasitic Ciliata occurring in the Termites, are incorporated, as well as the observations of Cunningham on *Protomyxomyces*, little more than a year old.

This is by no means the only merit of Mr. Kent's work. He might have contented himself with recasting the materials to be found in the three great volumes published by Stein, in Claparède and Lachmann, and in Pritchard's "Infusoria" (a valuable book in its day), and have simply incorporated with these the results scattered through the various English and foreign journals and transactions of the past twenty-five years. Mr. Kent has duly done all this, but he has done more, since he has himself made a very careful and prolonged study of a large number of Infusoria. Accordingly we find throughout the present work original observations brought forward for the first time. These include a number of new species and genera, especially among the Flagellate and Tentaculiferous forms. The beautiful cup-forming monads mounted on branching stalks like a colony of Vorticellæ were first brought prominently into notice by that keen observer, the late Prof. James-Clark of Boston, and Mr. Kent has followed up the study of these beautiful forms in a very thorough manner. On the whole, it may be said that the portion of Mr. Kent's work devoted to the Flagellata will have, for those naturalists who have not very closely followed the periodical literature of the subject, the charm of complete novelty, for very many of these forms were completely unknown or misunderstood till within the last ten years, and have not found their way into general treatises and text-books before the present occasion.

With Mr. Kent's views as to the affinities of the different classes of Infusoria it is not necessary to agree in order to appreciate the value of his work in general. The view is maintained in the "Manual" that the Sponges are genetically related to the Flagellata, whilst an opinion is quoted to the effect that the Ciliata are related to the Turbellaria. Without discussing the grounds for either of these views, we must simply express our entire disagreement with Mr. Kent, who is not (it seems to us) so happy in these speculations as in the more substantial portion of his work. The woodcut on p. 477, vol. ii., comparing the disposition of the ciliated bands of various Infusoria with that of the similar ciliated bands of various larvæ of higher animals, is exceedingly instructive and useful. It serves to point out the close similarity in form and disposition which such ciliated bands may assume in organisms totally unrelated to one another in the genealogical sense. Mr. Kent, however, takes the view, which we think will not be shared by many zoologists, that there is a deeper significance in the occurrence of these similar modifications of similar structures in forms so widely apart as unicellular Protozoa and multicellular Molluscan, Polyzoan, and Echinoderm larvæ; according to Mr. Kent they indicate "affinity," "phylogenetic connection," and "biogenetic relationship." In every direction Mr. Kent detects possible instances of such affinity, which he sets forth in a tabular form on p. 479; but it is not always quite obvious what Mr. Kent means when he speaks of certain Infusoria as "prototypes of" and as "foreshadowing" higher organisms. Had he confined himself to drawing attention to the remarkable parallelism or homoplasy presented by Infusoria on the one hand, and certain higher organisms on the other, we could have appreciated his capacity for detecting structural coincidences. But it appears to be Mr. Kent's opinion that the Holotrichous Ciliata are the forefathers of the Annelida, which are *also* traced by him to the Peritricha. The latter have (according to Mr. Kent) given rise to the Polyzoa, Mollusca, and Echinoderms; the Hypotricha are the ancestors of the Rotifera and of the Arthropoda! whilst the Tentaculifera are the progenitors of the Cœlenterata and the Choano-flagellata of the Sponges.

Mr. Saville Kent is no doubt entitled to hold and to promulgate an opinion on these matters, but we regret, inasmuch as his opinion is a very singular one, that he should have allowed it to take a prominent position in this "Manual."

Upon the question of spontaneous generation Mr. Kent is in accord with the prevalent doctrine, and gives a clear exposition of the history of the discussion of the subject, and so exhibits the importance of the researches carried on by Messrs. Dallinger and Drysdale upon the reproductive process in certain flagellate Infusoria, and the power of the ultra-minute germs of these Flagellata to resist the destructive influence of high temperatures.

With regard to the normal generation of Infusoria Mr. Kent is not so satisfactory. He distinguishes Fission, Gemmation, Sporular Multiplication, and "Genetic" Reproduction—the latter term being, without explanation, applied to sexual reproduction. Our author clearly has not—amongst his numerous and widespread researches upon the Infusoria—devoted any time to a personal investigation of the phenomena of conjugation and rejuvenescence amongst the Ciliata. The account which he gives of the work of Engelmann and Bütschli is meagre in comparison with the space which he has devoted to speculative digressions, and we find no figures illustrative of the exceedingly important results attained by those authors. In view of the great biological interest of the phenomena of conjugation in unicellular organisms generally, this is a serious omission. It is a mistake to have introduced the bygone error of attributing sexual reproduction to the Infusoria into this work at all, as Mr. Kent has done by the heading of his paragraph. Fission, gemmation, and possibly spore-formation preceded at a certain period in the family history by conjugation, constitute all that is *known* to occur in Infusoria. Mr. Kent makes too much of the isolated cases of spore-formation among Ciliata which stand upon good evidence. Admitting them as cases of spore-formation (that is of multiple fission), it is not possible to use them in support of the exploded view as to the production of embryos in the Ciliata by the breaking up of the nucleus after conjugation. Mr. Kent still clings to this notion of a special and peculiar formation of embryos within the parent ciliate Infusorian after, and as the immediate result of, conjugation, but he does not adduce any new fact in support of it. There is no reason adduced by Mr. Kent for regarding the nucleus of Infusoria as anything more than a cell-nucleus, and one is surprised to find that he should express so strong a disagreement with Butschli as to the fate of the cast-out fragments of the nuclei of conjugated Ciliata, when he does not detail to us any original observations made by him upon the process in question. The cast-out fragments of the nucleus of the conjugated Ciliate possibly have the same significance (Mr. Kent calls it an "unprofitable destiny," p. 98) as the cast-out præseminal apoblasts or "directive corpuscles" of an ordinary egg-cell.

Undoubtedly the best part of Mr. Kent's book, and one which will prove of constant value to that large body of working naturalists who are scattered throughout English-speaking lands, who delight to follow with care and accuracy, by the aid of the microscope, the forms and life-histories of the minute beings first made known by Mr. Antony van Leeuwenhoek, is that which contains the systematic description of every known species of Infusoria.

Accuracy is one of the first requisites in any attempt at scientific work, and Mr. Kent's descriptions and figures will enable numberless good observers in country places and small towns where there are no libraries containing the big books of Stein and Ehrenberg to *accurately* identify the organisms which they observe. By familiarity with Mr. Kent's book such an observer will be able to tell whether he has observed a new species or a new fact about a known species, and he will rise at once from the position of an isolated spectator of the curiosities of microscopic life to that of a possible contributor to the world's knowledge of animal structure, a fellow-worker with all the naturalists of civilised humanity. It is an excellent thing for the cause of science in England, and an excellent thing for other good causes too, that there are so many unprofessional naturalists in all classes of the community and in all parts of the kingdom. Our dilettanti naturalists not only pursue their favourite study with a devotion and energy which Englishmen always exhibit in regard to a "hobby," but they assist in all quarters in gaining for science true appreciation and popularity. Not merely so, but from their ranks many honoured leaders have sprung. It is chiefly for the service which he has rendered to this class of students that we consider Mr. Saville Kent is entitled to thanks, as was Andrew Pritchard and his editors in a past generation.

In conclusion we may briefly epitomise the classification of the Infusoria followed by Mr. Kent. He regards the Infusoria as a legion or section of the Protozoa or unicellular animals characterised by having appendages which are *not* pseudopodia, lobose, or radiate (Rhizopoda), but are either flagelliform, cilia, or tentaculiform. The character of possessing a distinct mouth or mouths cannot be strictly applied to the whole group, since some few flagellate forms have not even a localised ingestive area. The classes and orders and families recognised in this legion are as follows:—

CLASS I.—FLAGELLATA.

Order 1. TRYPANOSOMATA (Trypanosoma).

Order 2. RHIZOFLAGELLATA (Mastigamœba).

Order 3. RADIOFLAGELLATA.

Family 1. Actinomonadidæ; 2. Euchitonidæ.

Order 4. PANTOSTOMATA.

Family 1. Monadidæ; 2. Pleuromonadidæ; 3. Cercomonadidæ; 4. Codonœcidæ; 5. Dendromonadidæ; 6. Bikœcidæ; 7. Amphimonadidæ; 8. Spongomonadidæ; 9. Heteromitidæ; 10. Trepomonadidæ; 11. Polytomidæ; 12. Pseudosporidæ; 13. Spumellidæ; 14. Trimastigidæ; 15. Tetramitidæ; 16. Hexamitidæ; 17. Lophomonadidæ; 18. Catallactidæ.

Order 5. CHOANOFLAGELLATA or DISCOSTOMATA.

Section I. Gymnozoida.

Family 1. Codonosigidæ; 2. Salpingœcidæ; 3. Phalansteriidæ.

Section II. Sarcocrypta (The Sponges).

Order 6. EUSTOMATA.

Family 1. Paramonadidæ; 2. Astasiadæ; 3. Euglenidæ; 4. Noctilucidæ; 5. Chrysomonadidæ; 6 Zygoselmidæ; 7. Chilomonadidæ; 8. Anisonemidæ; 9. Sphenomonadidæ.

Order 7. CILIOFLAGELLATA.

Family 1. Peridiniidæ; 2. Heteromastigidæ; 3. Mallomonadidæ; 4. Stephanomonadidæ; 5. Trichonemidæ.

CLASS II.—CILIATA.

Order 1. HOLOTRICHA.

Family 1. Paramœcidæ; 2. Prorodontidæ; 3. Trachelophyllidæ; 4 Colepidæ; 5. Euchelyidæ; 6. Trachelocercidæ; 7. Tracheliidæ; 8. Ichthyophthiriidæ; 9. Ophryoglenidæ; 10. Pleuronemidæ; 11. Lembidæ; 12. Trichonymphidæ; 13. Opalinidæ.

Order 2. HETEROTRICHA.

Family 1. Bursariidæ; 2. Spirostomidæ; 3. Stentoridæ; 4. Tintinnodæ; 5. Trichodinopsidæ; 6. Codonellidæ; 7. Calceolidæ.

Order 3. PERITRICHA.

Family 1. Torquatellidæ; 3. Dictyocystidæ; 3. Actinobolidæ; 4. Halteriidæ; 5. Gyrocoridæ; 6. Urceolariidæ; 7. Ophryoscolecidæ; 8. Vorticellidæ.

Order 4. HYPOTRICHA.

Family 1. Litonotidæ; 2. Chlamydodontidæ; 3. Dysteriidæ; 4. Peritromidæ; 5. Oxytrichidæ; 6. Euplotidæ.

CLASS III.—TENTACULIFERA.

Order 1. SUCTORIA.

Family 1. Rhynchetidæ; 2. Acinetidæ; 3. Dendrocometidæ; 4. Dendrosomidæ.

Order 2. ACTINARIA.

Family 1. Ephelotidæ; 2. Ophryodendridæ.

The only feature in the above classification upon which it occurs to us to offer a remark is the limitation assigned to the class Flagellata. Putting aside the author's speciality as to the inclusion of the Sponges in that group, it seems that he has drawn up a very neat and, on the whole, satisfactory classification of the group. But on the one hand exception may be taken to the inclusion amongst the Flagellata of such forms as Mastigamœba and Euchitonia, whilst, on the other hand, those who follow Stein will ask why such forms as Volvox and Chlamydomonas are excluded. Further we cannot accept as satisfactory the subordinate position assigned to Noctiluca, the proboscis of which is no ordinary flagellum, but of so special a character as to entitle its owner to a distinct order or even a class. The fact is that it is excessively difficult to say what monadiform or flagellate unicellular organisms should be associated with forms such as the Choanoflagellata and Eustomata which undoubtedly are rightly placed in one legion with the Ciliata, and what should be left among lower plants, or again in association with the pseudopodic Rhizopods. A flagellate condition in the early stages of development (a "monad form") is common to a vast number of Protozoa and Thallophyta, and the mere flagellate character is not a sufficient basis for the construction of a natural group. Mr. Kent very properly proposes to separate as plants those flagellate forms which do not ingest solid particles of nutriment; but he is no doubt aware of the difficulty of observation in this matter, and of the statement (probably an erroneous one) by Stein, that certain Volvocineæ actually possess a cell-mouth and gullet.

The true limitations of the natural group of the Flagellate Infusoria will probably be ultimately found in the characters afforded by the series of events constituting the life-history of the various flagellate organisms which at first sight may appear to have a claim to be placed in that group.

E. RAY LANKESTER

OUR BOOK SHELF

The Micrographic Dictionary. By J. W. Griffith, M.D., and A. Henfrey. Fourth Edition, Edited by J. W. Griffith, M.D. (London: Van Voorst, 1883.)

THE interval of eight years since the publication of the third edition of the "Micrographic Dictionary" has been marked by substantial progress, not only in the microscope itself, but also in our knowledge of the structure of various classes of organisms included in the subjects specially treated. In the former of these two departments the present edition may be regarded as fairly keeping pace with the advance of science; and the introduction forms a very useful treatise on the structure and use of the microscope and of the various appliances and reagents which the microscopist should have at his command, as well as of the mode of examination of microscopic objects. In the second department the editor has been again assisted by the Rev. M. J. Berkeley in the cryptogamic articles, and by Prof. Rupert Jones in those on Geology and on Foraminifera, as well as by other specialists. To put new wine into old wine-skins is proverbially an unsatisfactory proceeding; and we do not know that it has been more successful here than elsewhere. We are far from saying that the syndicate who have assisted the editor have not contributed much from their vast stores to bring down the work to the date which it now bears on its title-page; but in some of the articles which we have had occasion to consult for work that we have happened to have in hand, the most recent observations are certainly not alluded to, and the system of classification is not the best or newest. But granting these defects, the work is one which no practical microscopist can afford to be without, and which must always lie on his table for ready reference. The present edition is enriched by five new plates, and some new woodcuts.

A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Speke and Grant's Zebra

IN or about 1882 a zebra was presented to the French Government by King Menelik of Shoa (which is in lat. 10° N., southeast of Abyssinia). It differed in certain respects from zebras hitherto supposed to have been described, and being regarded therefore as a new species or variety was named by Mr. Milne Edwards *Equus Grevyi*, after the President of the French Republic.

The species of zebra hitherto known were *E. quagga*, *E. Burchelli*, and *E. zebra*. The new one recently received in Paris apparently approximates to *E. zebra*, but the arrangement of the stripes, which are more numerous and more closely set, especially on the haunches, as well as its geographical distribution, seem to give sufficiently distinctive characters to entitle it to rank as a new species or variety.[1]

In a recent communication to the Zoological Society of London, Col. A. Grant, C.B., F.R.S., has called attention to the fact that the late Capt. Speke and he observed, hunted, and shot zebras during their expedition (chiefly in the lake regions of Equatorial Africa) in 1860–63, which from his description are either identical with the zebra from Shoa, or, if not, are entitled to be considered as a new species or variety.

Col. Grant has described the animal both in his notes written during the expedition and also in a paper to the Geographical Society in 1872, of which extracts are subjoined.

Should further examination and comparison show that the zebra described by Grant in 1861, and again in 1872, is identical with the Paris animal, it would seem that priority of discovery, although hitherto unclaimed, is due in title, as it is in fact, to Speke and Grant. If, on the other hand, the animal described by them should turn out to be distinct from any other form yet described, it would appear to have a claim to be named after these discoverers.[2]

The following extracts from Col. Grant's notes and papers addressed to the Geographical Society—written many years ago—appear to substantiate Speke and Grant's claim to the discovery and description of a new variety of zebra:—

Speke and Grant's Expedition of 1860-63, from Journal of Royal Geographical Society of London, 1872

"*Equus zebra* (?), *Native name. 'Phoonda.'*—This animal was frequent in Ugogo, Unyamezi, and north of Uganda. He differs from the *Equus Burchelli* of Regent's Park Gardens in being larger and differently striped. The stripes of our zebra were black upon white (not yellow) ground, and extended to the hoofs, whereas *Burchelli* has broader stripes, yellow ground, and the stripes on the legs are few. However, a sketch of an old mare shot by me shows the same black muzzle and hog mane as *Burchelli*, and Mr. Blyth says my sketch is of this last species."[1]

From Notes of Expedition

"Oct. 25, 1860.—Zebra shot through chest, shape superb, scarcely any pile, thickly striped over every inch of it, feet, legs, and all; fine hoofs, immense intestines. Flesh had quite the look of prime beef, ears rounded like deer's; a mare. A second zebra brought in by Ruyter. This was at Zungomero.

"Dec. 18.—Halt, in lat. 6° 22′ S., long. 30° 50′ E., altitude 2500 feet to 3329 feet. Zebra spoor again. . . . Shot another zebra.

"Dec. 21.—Again got zebras! . . .

"Dec. 22.—Do. do. . . .

"Jan. 2, 1861.—Eight to ten zebras.

"Jan. 6.—Zebras came among camp donkeys." . . .

Recent Note by Col. Grant, C.B., F.R.S.

"When we were shooting these zebras in Africa, we thought we were shooting the zebra which is common to Africa; but after our return, on my seeing Burchell's at the Zoological Gardens, I felt convinced we had never seen a Burchell's zebra; I said so to Blyth, who looked at my sketch, but who never saw Speke's specimens, and he seems to have called our zebras *Burchelli*. As soon as I saw Speke's specimens in 1873, and on hearing Prof. Flower describe by drawings the various zebras, I brought forward the matter, and got Speke's specimens up from Speke's brother. My journal notices *the stripes to be an inch apart all over the body, and extended to the hoofs*; but it says nothing of the marks on the haunches, though I believe that in our zebras, as well as in all other species, the haunch stripes are farther than an inch apart."

"The twelve zebras which were shot by the Speke and Grant expedition in 1861-3, were found at the undermentioned places in Africa:—

Places.	Lat.		Long.		Alt. above Sea. Feet.
	° ′		° ′		
Zungomero ...	7 27 S.	...	37 36 E.	...	516
Jiwe la M'koa	6 0 S.	...	34 0 E.	..	4690
Rubuga ...	5 0 S.	...	33 0 E.	...	3402
Usui District ...	2 49 S.	...	32 0 E.	...	About 4000
Uganda District	0 52 N.	..	32 30 E.	...	About 4000

"The zebras pasture in the forest and also in open country which is covered with bushy jungle, or where granite crops up, as this bears the richest grass, whilst hills with running water are always within their access."

However the zebras in question may be named, it seems right that the facts connected with Speke and Grant's discovery should be known.

J. FAYRER

April 17

Leaves and their Environment

I AM taking steps to have some analyses instituted, by a highly qualified authority, of the atmosphere (or water) in the natural environment of certain typical plants, in order, if possible, to produce experimental evidence upon the points impugned by Prof. Thiselton Dyer. The results of such (necessarily very inconclusive) evidence I shall publish in NATURE, if the Editor will grant me space, whether they are favourable or otherwise to my own allegations.

Meanwhile, as Prof. Dyer has himself relied upon purely *a priori* considerations, may I urge (1) that in the papers themselves I did not overlook the other factors of the problem to which he alludes; (2) that in woods, hedgerows, and thickets, the air is generally very still; (3) that the layer of air from time to time in actual contact with the surface of plants must always be in course of being deprived of its carbonic acid; and (4) that wherever plants get free access to the open air above, it was one of my own assertions that they must necessarily obtain carbonic acid in abundance. It seems to me difficult to understand how in a still place, where many plants at once are engaged in deoxidising a compound which only normally forms 0·03 per cent. of the atmosphere, there can always be as much of it left as any of them can possibly want. I do not presume to argue with Prof. Dyer upon the subject; but as far as my own comprehension goes, he has not made this point clear to me.

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[1] It should be added that *E. Burchelli* is the only zebra known to occur *north* of the equator, and that *E. zebra* has not been seen for many years. (Refer to figure in *Proc. Zool. Soc.* 1882, iv. p 721, published April 1, 1883.)

[2] It is possible that it may be a local variety of *E. zebra*, hitherto found much further south.

[1] Evidently Blyth was mistaken, as the zebra was thickly striped on the legs, which is not the case with *E. Burchelli*.

May I venture also to suggest that perhaps another danger surrounds biology, and especially botany—the danger of becoming too technical and too academic? Now that perfect instruments, immense collections, and a long technical training are necessary in order to do anything in biology by the regular road, does not the science run just a little risk of falling into a groove? And is it not well from this point of view that there should be an outside body of amateurs, who will take occasionally a fresh non professional view of the subject, handling their own problems in their own way, and publishing their own little guesses or glimpses for what they may be worth? No doubt they will often go demonstrably wrong; no doubt the masters of the science will usually find numerous blunders of detail in their work, and may often see reason to disagree with them altogether; and in that case the amateurs ought to receive their corrections with all humility; but is it not a healthy thing after all that the amateurs should do their best, and try to follow out their own lights to their own conclusions? GRANT ALLEN

Forms of Leaves

YOU have recently inserted several letters from Mr. Grant Allen on the forms of leaves, a question in which I have myself been working lately. Mr. Grant Allen's letters open up a number of interesting questions, but for the moment I will only refer to his suggestion with reference to the reason why water plants so often have their leaves cut up into fine filaments. He tells us that this is because the proportion of carbonic acid held in solution by water is very small, and that therefore for this amount there is a great competition among the various aquatic plants.

The question has already been asked on what grounds Mr. Allen makes this statement with reference to the proportionate amount of carbonic acid. Without entering on this point, I would, however, venture to suggest that the reason for this tendency in the leaves of water plants is mechanical rather than chemical.

It is, of course, important for all leaves to present a large surface for the purposes of absorption with as little expenditure of material for purposes of support as possible. Now delicate filaments such as those of water plants present a very large area of surface in proportion to their mass. On the other hand, they are unsuited to terrestrial plants, because they are deficient in strength and unable to support themselves in air. Take, for instance, a handful of the submerged leaves of an aquatic ranunculus out of the water, and, as every one knows, the filaments collapse. This seems to me the real reason why this form of leaves is an advantage to water plants. It is perhaps for the same reason that low-growing herbs, which are thus protected from the wind so often have much divided leaves.

April JOHN LUBBOCK

The Föhn

MAY I be allowed the space of a few lines to point out a defect in the account of the Föhn, given by Mr. Scott in his recent work on "Meteorology," and quoted in the review of that work which appeared in NATURE, vol. xxvii. p. 575. This phenomenon has been fully and clearly explained by Dr. J. Hann in a paper entitled "Einführung in die Meteorologie der Alpen," published under the auspices of the *D. und O. Alpenverein.* Mr. Scott's account of the Föhn attributes rightly the dryness and the cooling of the wind at high altitudes to expansion; but he appears to entirely overlook the *heating effect due to condensation of moisture* during the ascent of the wind.

From observations made in Switzerland, where the Föhn is chiefly felt, Hann has established the following rule: the Föhn is as many half degrees C. warmer at any place in its descent, than it is at an equal altitude during its previous ascent on the other side, as the place is hundreds of metres below the mountain ridge. This he explains by the fact that compression during the descent of the Föhn reverses the loss of temperature due to rarefaction during its previous ascent; while the wind brings with it over the mountain ridge the heat gained by the liberation of latent heat in the condensation of moisture. This latter amounts at 15° C. to about half a degree C. for each ascent of 100 metres for saturated air. "Therein," says Hann, "lies the explanation of the heat of the Föhn." A. IRVING

Wellington College, Berks, April 21

The Zodiacal Light (?)

THE same "peculiar appearance in the western sky" as that described by your correspondent, "J. W. B.," was observed here by me on the same evening, April 6. At 7h. 0m. G.M.T., or fifteen minutes after sunset, I noticed a bright, golden-coloured column of light, well defined, about 4° in length and slightly more than 1° in width, and inclined towards the south. "J. W. B." says it "rose vertically from near the horizon" at his station, Bath. Here it was decidedly inclined to an angle of about 15° towards the south. At 7h. 20m. no traces of it were visible. I have not seen any similar appearance since.

W. H. ROBINSON

N.B.—In the observer's book this observation is entered as "Bright zodiacal light (?), seen at 7h. 0m." E. J. STONE

Radcliffe Observatory, Oxford, April 20

REFERRING to the letter of your correspondent, "J. W. B.," Bath, in your last issue (p. 580), allow me to say that this peculiar ray of brilliant light was seen here by myself and many other people at about 6.40 p.m. on Friday, April 6. The sunset was brilliant and cloudless, but from the horizon to about 25° in height immediately above the spot where the sun had disappeared there appeared a ray of light of great beauty and extreme brilliancy; its centre, a delicate rose colour, graduating to the edges into the purest gold. This single ray was perpendicular, and appeared to be little, if at all affected, in its brilliancy by the approaching dusk of evening, but continued to exhibit itself with little-diminished brilliancy for nearly half an hour, finally disappearing with the twilight.

ROBERT DWARRIS GIBNEY

Glan-y-dwr, Crickhowell, South Wales, April 21

WHAT your correspondent, "J. W. B.," saw after sunset was not the zodiacal light, which is easily distinguishable by its great extent of area, lenticular shape, and invisibility during strong twilight, but it may be not incorrectly termed a sun column. I find the following entry in an old journal, of a similar appearance:—"1868, April 17.—Sun column, continuing half an hour after sunset, which was perfectly bright, without clouds." Perhaps some of your readers may be able to explain the cause of it. E. BROWN

Further Barton, Cirencester, April 21

THE phenomenon observed on the evening of Friday, the 6th inst., in Bath, by your correspondent J. W. B. (vol. xxvii. p. 580) was seen at Dolgelly by the writer when on a tour through Wales. On his pointing it out to a companion and some of the townsfolk, all agreed it was quite unique in their experience.

A bright, slender pillar of light, hazy toward the edges, rose majestically from the western horizon, in a cloudless sky, and so continued for about three-quarters of an hour after the sun had set. To one long habituated in meteorological observation it was of a character differing *toto cœlo* from the path of sunbeams through a cloud-rift, which is invariably divergent in appearance, as if from a focus. The "pillar" was uniform in width, perfectly vertical, and straight, the centre line alone brilliant. The height was, however, greater than your correspondent indicates.

Having fortunately with me a pocket-compass, with plumb-bob for "dip" measurements, I determined (1) the light-pillar was exactly vertical; (2) the height, which scarcely varied during visibility, was 20°, dying out faintly at that elevation; (3) the azimuth 25° north of west. By terrestrial bearings there was an appearance or a slight movement northward, but smallness of the compass dial (1 inch diameter) precluded any reliable angular determination of azimuthal change.

Further, the evening was very cold, and a continuous easterly wind had during the day obscured the hills, which still showed many unmelted snowdrifts upon their summits and flanks. First observed at 7 p.m., the strange appearance faded out at 7.30 p.m.

From the verticality, linear form, and condition of atmosphere I was led to remark at the time to my companion that the phenomenon appeared more of the nature of parhelia than referable to the zodiacal light. An intensely cold easterly wind encountering ocean-warmed airs to the westward would not improbably lead to the ice-molecule condition of atmosphere now assumed to be associated with the occurrence of parhelia.

It may be added (though of little probable significance) that the time corresponded roughly with the time of high water along that coast. D. J. ROWAN

Kingstown, April 24

On the Value of the "Neoarctic" as one of the Primary Zoological Regions

PERMIT me to make a few remarks relative to Mr. Wallace's criticisms (NATURE, vol. xxvii. p. 482) of my paper on "The Value of the Neoarctic as one of the Primary Zoological Regions." Briefly stated, it is maintained in the early portion of this paper (1) that the Neoarctic[1] and Palæarctic faunas taken individually exhibit, in comparison with the other regional faunas (at least the Neotropical, Ethiopian, and Australian), a marked absence of *positive* distinguishing characters, a deficiency which in the mammalia extends to families, genera, and species, and one which, in the case of the Neoarctic region, also equally (or nearly so) distinguishes the reptilian and amphibian faunas; (2) that this deficiency is principally due to the circumstance that many groups of animals which would otherwise be peculiar to, or very characteristic of, one or other of the regions, are prevented from being such by reason of their being held in common by the two regions; and (3) that the Neoarctic and Palæarctic faunas taken collectively are more clearly defined from any or all of the other faunas than either the Neoarctic or Palæarctic taken individually.

In reference to these points Mr. Wallace, while not denying the facts, remarks: "The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups and geographical individuality." Numerous families and genera from the classes of mammals and birds are then cited as being entirely wanting in the western hemisphere, and which—in many cases almost sufficient to "characterise the Old World as compared with the New"—"must surely be allowed to have great weight in determining this question." No one can deny that the absence from a given region of certain widespread groups of animals is a factor of very considerable importance in determining the zoological relationship of that region, and one that is not likely to be overlooked by any fair-minded investigator of the subject. But the value of this *negative* character afforded by the absence of certain animal groups as distinguishing a given fauna, is in great measure proportional to the extent of the positive character—that furnished by the presence of peculiar groups—and indeed may be said to be entirely dependent on it. No region can be said to be satisfactorily distinguished from another without its possessing both positive and negative distinguishing characters. Mr. Wallace has in his several publications laid considerable stress upon the negative features of the Neoarctic fauna as separating it from the Palæarctic or from any other, but he has not, it appears to me, sufficiently emphasised the great lack, *when compared to the other faunas*, of the positive element, the consideration of which is the point aimed at in the first portion of my paper, and which has led to the conclusions already stated, that only by uniting the Neoarctic and Palæarctic regions do we produce a collective fauna which is broadly distinguished by both positive and negative characters from that of any other region. If, as Mr. Wallace seems to argue, the absence from North America of the "families of hedgehogs, swine, and dormice, and of the genera *Meles*, *Equus*, *Bos*, *Gazella*, *Mus*, *Cricetus*, *Meriones*, *Dipus*, and *Hystrix*" be sufficient, as far as the mammalian fauna is concerned, to separate that region from the Palæarctic, could not on nearly equally strong grounds a separation be effected in the Palæarctic region itself? Thus, if were to consider the western division of the Palæarctic region, or what corresponds to the continent of Europe of geographers, as constituting an independent region of its own, it would be distinguished from the remainder of what now belongs to the Palæarctic region by negative characters probably fully as important as those indicated by Mr. Wallace as separating the Neoarctic from the Palæarctic region. The European mammalian fauna would be wholly deficient, or nearly so, in the genera *Equus*, *Moschus*, *Camelus*, *Poephagus*, *Gazella*, *Oryx*, *Addox*, *Saiga*, *Ovis*, *Lagomys*, *Tamias*, in several of the larger *Felidæ*, as the tiger and leopard, and in a host of other forms. A similar selection could be made from the class of birds (among the most striking of these the *Phasianidæ* and *Struthionidæ*), but it is scarcely necessary in this place to enter upon an enumeration of characteristic forms. Divisions of this kind, to be characterised principally or largely by negative faunal features, could be effected in all the regions, and in some instances with probably more reason than in the case under discussion.

But the question suggests itself, What amount of characters, whether positive or negative, or both, is sufficient to distinguish one regional fauna from another? Mr. Wallace states: "There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions." Is it to be inferred from this quotation that Mr. Wallace recognises no such general equivalence? Is a region holding in its fauna, say, from 15 to 20 per cent. of peculiar or highly characteristic forms to be considered equivalent in value to one where the faunal peculiarity amounts to [illegible]0 to 80 per cent? If there be no equivalence of any kind required, why not give to many of the subregions, as now recognised, the full value of region?

Surely, on this method of looking at the question, a province could readily be raised to the rank of a full region. In the matter of geographical individuality little need be said, as the circumstance, whether it be or be not so, that the "temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate," does not affect the present issue, seeing that the peculiarity is greatly increased by uniting the two regions in question; nor does it directly affect the question of the Neoarctic-Palæarctic relationship.

The second part of my paper deals with the examination of the reptilian and amphibian faunas, and the general conclusion arrived at is: "that by the community of its mammalian, batrachian, and reptilian characters, the Neoarctic fauna (excluding therefrom the local faunas of the Sonoran and Lower Californian subregions, which are Neotropical) is shown to be of a distinctively Old World type, and to be indissolubly linked to the Palæarctic (of which it forms only a lateral extension)." Towards this conclusion, which, it is claimed, is also borne out by the land and freshwater mollusca and the butterflies among insects, I am now happy to add the further testimony of Mr. Wallace (overlooked when preparing my article respecting the *Coleoptera* ("Distribution," "Encycl. Britann." 9th ed. vii. p. 274).

As regards the name "Triarctic," by which I intended to designate the combined Neoarctic and Palæarctic regions, and which may or may not be "somewhat awkward," I beg to state that, at the suggestion of Prof. Alfred Newton (who, as he informs me, has arrived from a study of the bird faunas at conclusions approximately identical with my own), it has been replaced by "Holarctic." In conclusion, I would say that, while the views enunciated in my paper may not meet with general acceptance at the hands of naturalists, it is to be hoped that they will not be rejected because they may "open up questions as regards the remaining regions which it will not be easy to set at rest." ANGELO HEILPRIN

Academy of Natural Sciences, Philadelphia, April 6

Mock Moons

A LITTLE before midnight on Monday, the 16th inst., the moon, being nine days old and about 30° above the western horizon, was surrounded by an unusual halo. Its radius was certainly more than the normal 22°. By careful estimation I judged it to be about 30°, the lower edge resting on the horizon. On the right and left limbs of the ring were very distinct bright patches, rather broader than the ring itself, and slightly elongated outwards. The right-hand patch appeared to be in its normal position on a line passing through the moon, parallel with the horizon, but the left-hand patch was distinctly elevated above this line, and seemed to be unaccountably out of place. As, however, the moon was little past the first quarter, and the terminator nearly a straight line, and only slightly inclined from the vertical, a line drawn perpendicular to it would have passed through the left-hand patch, and I imagine that its position was due to this inequality in shape of the two sides of the visible moon. The atmosphere was hazy, the moon though clearly visible appearing as in a slight fog. No colours were distinguishable at any part of the halo.

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[1] In the paper under consideration I have given what appear to me satisfactory reasons for detaching certain portions of the South-western United States from the Neoarctic (my Triarctic), and uniting them with the Neotropical region.

F. T. MOTT

Birstal Hill, Leicester, April 17

Benevolence in Animals

TWO or three years ago Dr. Allen Thomson gave me an instance of benevolence in a cat which is so closely similar to one communicated to you by Mr. Oswald Fitch that for the sake of corroboration I may state it.

The cat belonged to Dr. Thomson, and one day came into the kitchen, pulled the cook by the dress, and otherwise made signs showing a persistent desire to attract attention. Eventually the cat led the cook out of doors and showed her a famishing stranger cat. The cook thereupon gave the stranger some food, and while this was being discussed, Dr. Thomson's cat paraded round and round her companion, purring loudly with a satisfied sense of well-doing.

GEORGE J. ROMANES

"Medioscribed Circle"

IN this week's NATURE (p. 595) the use of the *medioscribed* circle is suggested in place of the well-known "nine-point" circle. If a change is desirable, would not "mid-point" circle be equally expressive?

R. T.

April 20

AGRICULTURE IN MADRAS[1]

THE Government establishment at Saidápet has now been in existence about twelve years. It consists in part of an experimental farm, and in part of an educational establishment, in which, at the date of the last report, forty-one native students were receiving instruction in the science and practice of agriculture. The whole is under the superintendence of Mr. W. R. Robertson. The object in view is to improve the condition of agriculture in the Presidency. This is indeed urgently needed. With a large and increasing population, the soil is in general wretchedly cultivated, and reduced to a low state of fertility. The farm at Saidápet is the centre of many useful agencies. Here new crops, new breeds of cattle, and improved implements are carefully tried. Here the teaching of European science is reduced to practice, and methods of cultivation suitable to the conditions of Indian agriculture are perfected. While by means of the educational department, by tours in the country, distributions of seed, ploughing competitions with different implements, and various other agencies, the endeavour is made to bring these improved methods into use by the native farmers.

The meteorological records kept at the farm exhibit in a striking manner the difficulties under which Indian agriculture must be pursued. Thus in the season 1880-81 the rainfall in September was 10·9, in October 10·7, and in November 19·6 inches, while during the whole six months from January to June only 2·35 inches were recorded. Long-continued heat and drought are thus followed, on the arrival of the monsoon, by a deluge of rain. It is pleasing to notice that the director of the farm is quite abreast of the latest scientific teaching respecting the best mode of meeting the difficulty in question. It is plain that in the rainy season the land will be washed clear of all soluble plant-food; all nitrates formed in the soil during the hot season will thus be lost, unless they have been already assimilated by a crop. Mr. Robertson recommends that, whenever possible, advantage should be taken of the first commencement of rain in June or July to sow the land with a green leguminous crop (horse-gram). In most years there will be enough rain to maintain such a crop in growth during the summer months. This crop will collect and assimilate a great part of the nitrates in the soil. At the commencement of the wet season the green crop is to be ploughed into the soil, and forms an excellent manure for the principal crop of the year, which is then sown. Mr. Robertson refers apparently to the experiments at Rothamsted when speaking of the quantity of nitrates annually formed in a soil; the amount he mentions (40 lbs. of nitrates[1] per acre) is, however, far below the truth. The quantity of nitrates found in five successive years in the drainage water from uncropped and unmanured land at Rothamsted amounts, indeed, on an average, to nearly 3 cwts. of Indian saltpetre per acre per annum.

In India agriculture depends much for its success and permanence on irrigation, and vast sums have been, and will be, expended on irrigation works. Here again, however, the question of the presence or absence of nitrates is an important factor, which has been almost entirely overlooked, engineering rather than chemical skill having been employed in the direction of the work. It should always be borne in mind that a water containing nitrates supplies not only water but *manure*. The native farmers are generally quite aware of the difference in value of different water-supplies, and reckon the water from the village well as worth far more than that procured from the Government canal. To the engineer it appears a ridiculous waste of power to lift water from a well when a water-supply is available at the level of the land. But the native is right; his well-water is rich in nitrates, and for the farmer's purpose far more valuable than the purer water of surface drainage found in rivers and canals. It should always be borne in mind in plans for irrigation, that the drainage from arable land, and from inhabited districts will always yield the best irrigation water. By restoring to land in time of drought the plant-food lost in time of flood we are pursuing a truly scientific economy.

R. W.

ANTHROPOLOGICAL NOTES IN THE SOLOMON ISLANDS

WITHOUT going into the general question as to the position which these islanders hold to the other Pacific races, I will briefly state the results of numerous measurements and observations which I made during my visit to these islands in 1882. As the surveying work of this ship was confined for the most part to the large island of St. Christoval and the adjacent small islands, my remarks will refer more particularly to the natives of this part of the group.

The average height of a man of St. Christoval is about 5 feet 3 inches or 5 feet 4 inches, whilst the span of the arms generally exceeds the length of the body by from 4 to 5 inches. Both men and women are usually of a good physique, robust and well-proportioned; but one may find in the same village community weak, puny, thin-limbed individuals associated with others of a strong muscular frame, with well-rounded limbs and a good carriage.

The colour of the skin varies considerably in shade from a very dark brown, approaching black, to a dark copper hue. The elderly adults are as a rule more dark-skinned than those of younger years, the difference in shade being attributable partly to a longer exposure by reason of their age to the influence of sun and weather, and partly to those structural changes in the skin which accompany advancing years. Not unfrequently, amongst a group of dark-skinned natives a man may be observed whose skin is of a pale sickly hue, and who at the first glance may be thought to afford an example of recent intermingling between the Melanesian and Polynesian races. On a closer examination I always found that such men were covered from head to foot with an inveterate form of body ringworm—a scaly skin-eruption, which affects in a greater or less degree quite two-fifths of the natives of this part of the group—and that in all their other physical characters they belonged to the Melanesian type. In its most aggravated and chronic condition this parasitical disease implicates the skin to such a degree that the rapid desiccation and desquamation of the epidermal cells lead to a partial decoloration of the deeper parts of the cuticle, as though the rate of the production of pigment was less rapid than the rate of its removal in the desquamative process.

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[1] Annual Reports on Government Agricultural Operations in the Madras Presidency, 1880-81 and 1881-82.

[1] Possibly in Mr. Robertson's Report "nitrates" is here a misprint for "nitrogen."

The hair of the head is generally black, frizzly, and bushy; more particularly amongst the younger adults of both sexes this last character prevails. Amongst middle-aged men I have sometimes observed that the hair arranges itself into entangled corkscrew-like spirals, the whole head of hair having much the appearance of a mop placed erect on its handle. Now and then, though rarely, the hair shows a tendency to become straight; I met with one such native near Cape Keibeck, on the north coast of St Christoval; and I am informed that straight-haired varieties do exist among the hill-tribes in the interior of the island. With regard to the amount of hair on the face, limbs, and body, great diversity is observed amongst natives of the same village. Epilation is commonly employed, but there can be no doubt that the development of hair varies quite independently of such a custom. Out of ten men taken promiscuously from any village, perhaps five would have smooth faces, three would possess a small growth of hair on the chin and upper lip, the ninth would wear a beard, a moustache, and whiskers of moderate growth, whilst the tenth would present a shaggy beard and a hairy visage. The surfaces of the body and limbs are as a rule comparatively free from hair; but hairy men are to be met with in most villages; and on one occasion when in the vicinity of Cape Surville—the eastern extremity of St. Christoval—I visited a village where the proportion of hairy-bodied, hairy-visaged men was in excess of the smooth-skinned element.

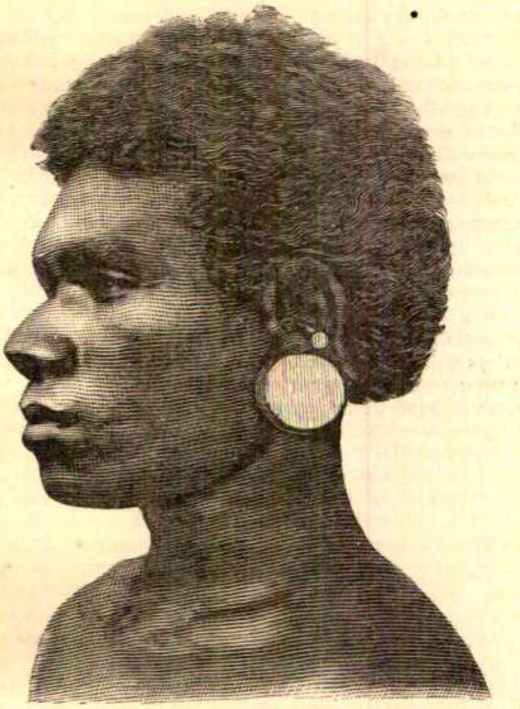


FIG. 1.—Native of Santa Anna (an island off the east extremity of St. Christoval). The round disk of wood in the lobe of the ear should be quite white, the dark spots being due to the imperfections of the dry plate. The faint linear markings on the cheek due to a form of tattoo are rarely well marked.

From my measurements the form of the skull would appear to be mesocephalic: the cephalic indices ranged between 73 and 82—the greater number of them being included between 74 and 77. The facial angle varied in amount between 85° and 90°. The nose is generally straight, coarse, and somewhat short, the nostrils wide, and the bridge depressed in some instances. Not uncommonly the nose is arched or aquiline; out of fifty natives amongst whom I took especial notice of this feature, I found that ten possessed an aquiline nose. The countenances of the younger of both sexes are often prepossessing, and amongst the adults I have frequently met with men of some intellectual expression.

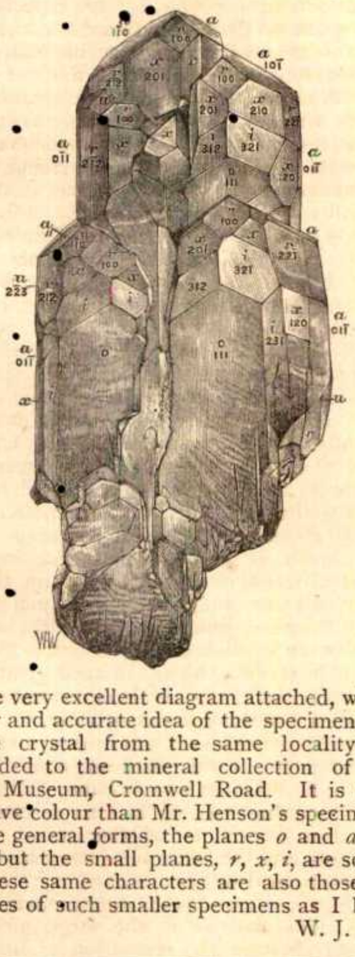
Such are some of the leading physical characters of the natives of this part of the Solomon group. To the inhabitants of the small island of Santa Anna, which lies off the east extremity of St. Christoval, the same description will apply; but we find in the still smaller adjacent island of Santa Catalina a subvariety of the Melanesian type characterised by a lighter colour and probably a greater height, although I made no measurements there. The few natives which I saw belonging to the large island of Malayta, which we did not visit, resembled in appearance those of St. Christoval; and from a few measurements and observations which I made in the Florida subgroup, where the St. Christoval type prevails, it was evident that thus far to the westward the same description of a native of the Solomon Islands was equally applicable. The large neighbouring island of Guadalcanar I had no opportunity of visiting. In the small island of Simbo, further to the west, I found no important difference in the physical characters of the natives except perhaps a rather darker shade of colour. Proceeding westward as far as Treasury Island, our furthest point in that direction, we for the first time came upon a distinct variation in the type of native—a difference which has been a subject for remark even by such usually unobservant people as the masters of trading ships. In their greater height and in the almost black colour of the skin, the natives of Treasury Island are at once distinguished from the prevailing native type to the eastward. Their features are more finely cut, and the form of the skull, as shown by the cephalic indices, is more brachycephalic—the range of seven measurements being 78 to 84, and the mean cephalic index 81. In some individuals the cheekbones were prominent and the foreheads retreating. As a race the Treasury Islanders are said to evince a fiercer disposition than do the natives in the eastern islands of the Solomon group. The natives of the large adjacent island of Bougainville have the reputation of being amongst the most daring and warlike of the inhabitants of this archipelago; and probably the examination of their physical characters will exhibit them as a more pronounced type of the Treasury Islanders. H. B. GUPPY

H.M.S. *Lark*, Auckland, N.Z., February 27

ON A FINE SPECIMEN OF APATITE FROM TYROL, LATELY IN THE POSSESSION OF MR. SAMUEL HENSON

THE specimen of apatite represented in the diagram was submitted to my inspection by Mr. Henson last November, and is the most beautiful specimen of this mineral which I have seen. The faces observed were not, however, determined on the specimen itself, but from a plaster cast and a smaller specimen with which Mr. Henson supplied me. From these latter approximate measurements of some of the more prominent angles were obtained by means of a contact-goniometer, which, on comparison with the table of angles given in Miller's "Mineralogy," rendered the identification of the more conspicuous planes easy. The remaining planes were then easily determined from the relation which connects three planes lying in a zone. The forms present are: $o\{111\}$, $a\{10\bar{1}\}$, $r\{100\}$, $r_1\{22\bar{1}\}$, $x\{210\}$, $i\{321\}$, $u\pi\{4\bar{1}0\}$, $u_1\pi\{2\bar{2}3\}$.

I had no intention of describing the specimen at the time it was shown me, and did not pay enough attention to the physical characters of the faces to be able to recall them. The specimen was for the most part remarkably limpid, with a pale mauve tint in its purest portions. It was in part penetrated by fine delicate needles of epidote,



ao	90°	0′
io	157	05
xo	139	47
xa	130	13
ro	124	20
aa′	120	0
ra	135	39
rr	131	14

as is shown in the very excellent diagram attached, which gives a very clear and accurate idea of the specimen. A remarkably large crystal from the same locality has recently been added to the mineral collection of the Natural History Museum, Cromwell Road. It is of a much deeper mauve colour than Mr. Henson's specimen; it shows the same general forms, the planes *o* and *a* are bright and even, but the small planes, *r*, *x*, *i*, are somewhat rough. These same characters are also those observed on the faces of such smaller specimens as I have examined.

W. J. L.

THE EVOLUTION OF THE AMERICAN TROTTING-HORSE.[1]

THE American trotting-horse is an example of a new breed of animals in process of formation. As yet it can hardly be called a definite breed in which the special and distinctive character is either fully developed in quality or satisfactorily fixed by heredity. Great progress has, however, been made, many individual animals have attained great speed, and all the better ones have derived their trotting excellence in part, at least, through heredity.

The origin of most breeds is involved in considerable obscurity, as to how much they are due to conscious and how much to unconscious selection, what motives led to this selection, how far the enhancement of the special qualities have been due to physical environment, and how far to education, training, nourishment, or cultivation. The formation of this new breed is so recent, the development of a special quality has been so marked, there is such an abundant literature pertaining to its history, the system of sporting "records" is so carefully planned and comprehensively conducted, and withal has become so extensive, that we have the data for a reasonably accurate determination of the influences at work which led to this new breed being made, the materials of which it is made, and the rate of progress of the special evolution.

It is as an implement of gambling and sport that the trotter has his chief value to the biological student. Sporting events are published or recorded as the mere everyday use of animals is not, and the records of races give numerical data by which to measure the rate of progress. Similar data do not exist for the study of the evolution of any other breed.

Incidental to the preparation of a paper pertaining to this matter for farmers and breeders, I have compiled and collated certain data which have a scientific as well as economic value, the more interesting portion of which I condense for this paper.

The horse has several gaits which he uses naturally, that is, instinctively. And besides those which are natural, he has been taught several artificial ones, some of which have been much used, particularly in the middle ages. But to trot fast was not natural to horses; when urged to speed they never assumed it, and until within a century the gait was neither cultivated nor wanted by any class of horsemen. A breed of fast trotters, had it been miraculously created, would doubtless soon have perished in that it would have had no use, satisfied no fancy, and found no place in either the social or industrial world as it then was.

Before the present century the chief and almost sole uses of the horse were as an implement of war, an instrument of sport and ceremony, an index of rank and wealth, and an article of luxury.

For all these uses, as then pursued, a fast trotter was not suited, nor was he better adapted to the heavy coaches over rough roads, or the slow waggon-trains of armies. The horse best adapted to all these, however much he may have varied as to size, strength, and fleetness, was one whose fast gait was the gallop or run rather than the trot. For leisurely horseback travelling the ambling gait (or *pacing* gait as it came to be called in America) was preferred. With increasing use of horses for draft, certain heavy but slow breeds were developed in the Old World, of which the Dutch, Clydesdale, and Norman breeds are examples.

The causes which led to the cultivation of the trotting gait in this country, and the evolution of a breed with which it should be instinctively the fast gait, were various, and the separate value of each as a factor in the problem would be very differently estimated by different persons studying the subject from different points of view. Now that he is so valuable and plays such a part as a horse of use, it is easy to see why a breed of trotting roadsters should be produced to meet certain important demands of our modern civilisation. But this does not explain how the process actually began.

Reasoning *a priori*, the trotter, as a horse of use, should have originated in western Europe; as a matter of fact, he not only did not begin there, but he was unpopular there until well developed here. Locomotives began to draw armies to the battle-field, the war-horse declined in actual as well as relative importance, the modern, light, steel-spring, one-horse, convenient business waggon as well as the modern buggy came into common use after trotting as a sport was established; and after the gait had been extensively cultivated and bred to. The trotting-horse is specially adapted to various modern uses, but these uses followed his development, rather than led it, although in later days this factor has been an important one in the rate of progress.

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[1] By Wm. H. Brewer, from the *American Journal of Science*.

The influences which originally led to the starting of the breed were more social than economical; a similar fact a century earlier marked the founding of that famous running breed, the English thoroughbred. The origin of the trotter, however, was not so simple as that, and several diverse social factors were involved, only the chief of which will here be noticed.

From early colonial times horses have been more generally owned by the masses of the people here than in any country of western Europe. They have had a more general use in agriculture and in business, their ownership or possession has had less social significance, and they have had less importance as instruments of gambling. The colonists who settled north of Delaware Bay, although of various nationalities, were largely those whose religious prejudices and social education was opposed to horse-racing. With the great majority of them it was considered a sort of aristocratic sport, and at best led to unthrifty ways, even if not open to the objection of positive immorality. Consequently but few race-horses were imported into this region in colonial times. The original horse stock of the northern colonies came from several European sources. England, Holland, France, and Spain certainly, and Sweden, Denmark, Germany, Ireland, and Italy probably, contributed to it. The blood from this variety of sources, variously mingled, formed the mongrel stock of the country. This was further modified by local conditions and local breeding assuming different characters in different places, and the hardships of horse life incident to a new country, with strange forage and a rough climate, caused deterioration in size and form. Early writers are unanimous on this point, but many add that what was lost in size and beauty was gained in hardiness and other useful qualities.

After the war of independence there was an improvement in the live stock of the country. English thoroughbred horses were imported both for sporting and to improve the horse stock of the country, and horse-racing rapidly grew in favour as wealth and leisure increased. The export trade in horses to the West Indies increased, particularly from New England. Pacers were most sought for this trade, but sometimes trotters were advertised for.

As horse-racing increased in the last years of the last century the opposition to it revived, and in the earlier years of the present century this became ascendant, and stringent laws forbidding the sport were passed in most of the northern States. The prohibition was sweeping and the penalties severe.

Horse-racing was then a contest between running-horses, and during this repression of racing, trotting as a sport began, at first in a very unostentatious, irregular, and innocent sort of way. Probably no people or class of people have ever bred good horses which they prized and were proud of, who did not find pleasure in seeing them compete in speed or show their fleetness in some way, and during the repression of racing (which meant running), trotting came in as a substitute, poor though it was at first. It had a sort of encouragement from very many thrifty people who were not sportsmen, and was in a measure considered a sort of democratic sport in which even plough-horses could take part. Racing of any kind in those days was a strife between two or more things, as it still is in most countries; no one thought that a single horse could run a race alone, but the instinctive inclination to see a spirited horse in action could be mildly gratified by letting him trot, even if single and alone, and testing by the watch how quickly a given distance could be covered. So "timing" animals came to be practised. We hear of it on the Harlem racecourse in 1806, four years after the laws forbidding horse-racing had been enacted, and again, a little later, near Boston, and it was reputed that certain horses could trot a mile in three minutes. This speed seemed so extraordinary that in 1818 a bet of a thousand dollars was staked (and lost) that no horse could be found that could trot a mile in three minutes. Some authorities date the beginning of trotting as a sport with this event. It is said that in the betting the odds against the successful performance of the feat were great, which shows, strikingly, the enormous progress since made in developing speed at this gait.

In 1821, certain persons on Long Island were allowed by special statute to train, trot, etc., horses on a certain track, under certain restrictions, exempt from the penalties against horse-racing. Other organisations followed, and by 1830 the "training" of trotters was going on at several points, and trotting may be said to have become established as a sport. During this decade the record had been successively lowered to 2.40, 2.34, and 2.32. The times of performance were carefully taken at these "trials of speed," as the statute called them, and "records" became established by more formal sporting codes.

The ostensible object of these associations was the "improvement of the breed of roadsters;" driving single horses to waggons became fashionable, and this led to the improvement of light one-horse waggons for business and pleasure. Those with steel springs were rare luxuries in 1830; by 1843, when the record of mile heats dropped to below 2.30, they were already common. During this thirteen years, the record had been lowered only half a second on mile heats, but three-minute horses were no longer rare.

The fashion of wealthy men driving a single fast trotter for pleasure was for a long time a peculiarly American one, and played an important part in the development of this breed. But, as stated earlier, many influences have contributed: changes in the modes of travel, changes in the methods of war, sentiments regarding horse-racing, the incentives of the course, the general improvement of roads, improvement in carriages, the needs of modern business requiring quick roadsters, these and other influences have all been at work.[1]

The material out of which this new breed is made is a liberal infusion of English thoroughbred blood (usually more than two generations removed), with the mongrel country stock, previously described. There is a voluminous literature relating to special pedigrees, and much speculation as to the comparative merits of the several ingredients of this composite blood.

Regarding the ideal trotter there is as yet a difference of opinion as to what the form should be, and it is too early to decide from actual results. That the gait is now hereditary, that it is the instinctive fast gait with some animals is certain, but whether this is due to inherited habit, inherited training, or to mere adventitious variation and selection, I will not discuss.

The gain in speed is given in the following table, which is the best records at mile heats, omitting the names of the special performers:

Date.	Best Record.	Date.	Best Record.
1818,	3	1865,	2.18¼
1824,	2.40	1866,	2.18
,,	2.34	1867,	2.17¼
1830,	2.32	1871,	2.17
1834,	2.31½	1872,	2.16¾
1843,	2.28	1874,	2.14
1844,	2.26½	1878,	2.13¼
1852,	2.26	1879,	2.12¾
1853,	2.25½	1880,	2.10¾
1856,	2.24½	1881,	2.10¼
1859,	2.19¾		

A sporting paper published in 1873 a list of three hundred and twenty-three horses, with their best records, down to the close of the preceding year. This first list of the kind known to me was very imperfect in its details; it was revised for the next year, and since that time many lists, in one form or another, have been published. The figures for the animals with records of 2.25, or better, are reasonably accurate; for the others there is much discrepancy. In the following table the numbers are my own, counting down to 1872, inclusive; the numbers after that date are derived from various lists published since that time in the sporting and breeding periodicals. From the very nature of the case, the table cannot be accurate in the larger numbers, but the numbers do not lose their value for comparison with each other from such faults as to the details of the larger numbers, and, as such, it is undoubtedly the most significant series of numbers ever compiled to show progress in evolution, whether of a breed or species. The number of horses with records of 2.40, or better, is now stated to be over five thousand.

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[1] For more details regarding the history of this development and the factors involved, see the paper already cited, *Rep. Conn. Bd. Agr.* for 1882, p. 215.

I leave it to mathematicians to plot the curves which immediately suggest themselves, and determine how fast horses will ultimately trot, and when this maximum will be reached.

Table showing the numbers of Horses under the respective Records.

	2.30 or better.	2.27 or better.	2.25 or better.	2.23 or better.	2.21 or better.	2.19 or better.	2.17 or better.	2.15 or better	2.13 or better.	2.11 or better
1843	1									
1844	2	1								
1849	7	2								
1852	10	3								
1853	14	5								
1854	16	6								
1855	19	6								
1856	24	7	1							
1857	26	7	2							
1858	38	7	2							
1859	32	9	2	1	1					
1860	40	11	4	2	1					
1861	48	14	4	2	1					
1862	54	17	7	4	1					
1863	59	19	9	4	1					
1864	66	22	12	4	1					
1865	84	29	15	5	2	1				
1866	101	32	17	6	3	1				
1867	124	42	21	9	5	2				
1868	146	52	28	13	6	2				
1869	171	63	34	15	10	4				
1870	194	72	35	16	11	5				
1871	233	99	40	17	12	6	1			
1872	323	—	—	—	—	—	—			
1873	376	—	74	28	15	5	2			
1874	505	—	98	40	16	11	5	1		
1875	—	—	134	61	30	13	5	2		
1876	794	—	165	81	39	16	6	2		
1877	836	—	214	105	51	19	8	2		
1878	1,025	—	270	129	68	24	9	4		
1879	1,142	—	325	164	88	33	11	5	1	
1880	1,210	—	366	192	106	41	14	6	2	1
1881	1,532	—	419	227	126	49	15	7	2	1
1882	1,684	—	495	275	156	60	18	8	2	1

INSTITUTION OF MECHANICAL ENGINEERS

THIS Institution held their usual Spring meeting at the Institution of Civil Engineers, 25, Great George Street, on April 11 and 12, the president, Mr. Percy G. B. Westmacott, in the chair. Three papers were read, and discussed at length; a fourth, by Mr. A. C. Bagot, on "The Application of Electricity to Coal Mines," was postponed for want of time.

The first paper was by Prof. A. G. Greenhill, of Woolwich Arsenal, and dealt with the strength of shafting when exposed both to torsion and end-thrust. He has worked out for this case, by a complete mathematical investigation to be published in the *Proceedings*, the following formula:—

$$\frac{\pi^2}{l^2} = \frac{P}{EI} + \frac{T^2}{4E^2I^2}$$

where P = end-thrust, T = twisting moment, I = moment of inertia of cross-section, E = modulus of elasticity, l = maximum distance between bearings, which will allow a shaft to be stable.

When there is no twisting moment, as in a long column, the second part of the right-hand expression vanishes, and we have the ordinary formula of Euler. If there be no end-thrust, as in ordinary mill shafting, the first part vanishes. The special case where both occur together is that of the screw-shaft of a steamer; but here, it appears, on working the figures out with ordinary dimensions, that the second part is small in comparison with the first, and may be neglected. Hence a screw-shaft may so far be treated as if it were a long column only; and it follows at once that the numerous bearings interposed between the engines and propeller (say, about every 25 feet) are quite unnecessary so far as stiffness is concerned. If retained, as seems desirable, simply to support the weight of the shaft, they might at least be made in some way elastic, so as to enable the shaft to accommodate itself to the sagging and straining of the vessel. It was, in fact, admitted on all hands that screw-shafts never give way from twist or thrust, but always by cross-breaking through strains induced by the unequal movements of the ship; and if so, there seems every reason for taking some steps at least in the direction which Prof. Greenhill indicates.

Another point which the paper touched upon was the question of hollow *versus* solid shafts. Now that shafts can be conveniently cast out of ingot steel, they are frequently made hollow, with the obvious advantage of increasing the stiffness as compared with the weight. Thus, in the case of the screw-shaft of the *City of Rome*, which is 25 inches diameter, with an internal hole of 14 inches diameter, it appears that the moment of inertia is 0·9 of that of an equal solid shaft, while the weight of the latter would be 1·45 that of the former. Again, if a solid shaft were used of the same weight as the hollow shaft, or 20·7 inches diameter, its moment of inertia, and therefore its stiffness, would be barely half that of the latter. Even if a transverse crack, 1 inch deep, were to occur in the hollow shaft (which it might be urged would place it at a serious disadvantage) the loss of stiffness comes out to be 6 per cent., whereas in a solid shaft of equal diameter the corresponding loss would be 5 per cent.; so that even here the advantage on the side of solidity is only 1 per cent.

These figures might seem to be conclusive, yet the solid shaft has its defenders. Mr. Edward Reynolds, of Messrs. Vickers and Co., stated roundly that the history of hollow screw-shafts was a mere history of disaster (which, however, was denied by a subsequent speaker); and he quoted some experiments of his own on shafts one-fourth the size of that in the *City of Rome*, where, tested under a 1-ton weight falling from about 20 feet, the hollow shaft was rapidly destroyed, while the solid shaft remained uninjured. This occurred even when great care was taken to prevent the hollow shaft from getting flattened during the process. His explanation was that the comparatively unstrained fibres towards the centre of the section came in to support and relieve the exterior parts, whenever, by cracks or otherwise, these became unduly loaded. Prof. Kennedy, who followed, seemed to lean to the same view, and quoted the increase of strength observable in the metal between the holes of a drilled plate, as being due, in some unexplained manner, to the influence of the unstrained metal behind the holes. A very satisfactory explanation of this fact was, however, given by Mr. Wrightson at the last meeting of the British

Association. The real question to which Mr. Reynolds's tests point is probably how far theories which rest on the hypothesis that elasticity is perfect can properly be applied to cases where the breaking point has been nearly reached; and this is a question on which more light is very urgently needed, especially with reference to such cases as screw-shafts, where fractures, as a matter of fact, do very commonly occur.

The second paper, by Mr. W. Ford Smith, dealt with twist drills, milling machines, and other methods for the cutting and dressing of metal surfaces, which have been introduced within the last few years; and was almost entirely of a practical character. The third paper, by Mr. John Jameson, was on "Improvements in the Manufacture of Coke," and dealt with a new method, invented by the author, for recovering the gas, gas-tar, and ammoniacal liquor, which are separated from coking coal during the process of carbonisation. As the paper points out, these products are not originally present in the coal. There is, for instance, no ammonia in coal; but there are combinations containing nitrogen and hydrogen, and in almost any process of distillation parts of the evolved nitrogen and hydrogen unite, under very obscure conditions, to form ammonia, which, however, is not stable, but readily decomposes in the presence *e.g.* of oxygen. Every process of distillation, in fact (but some much more than others), favours the formation of gas on the one hand and of condensable hydrocarbons on the other. With regard to the former, its value in the neighbourhood of coke-ovens is not usually high, and it is a question whether it may not best be burnt in the oven itself, to furnish the heat required in any case for the distilling process; but the value of tar and ammonia is great, and would probably not fall very low, even if the production were largely increased. At the same time, as a fuel they are not even equal to the same weight of pure carbon. It will be seen, therefore, that there is ample room for a process which will enable us to separate and utilise these by-products, instead of simply using them as fuel, or, which is far worse, discharging them unburnt to poison the air and destroy vegetation. Mr. Jameson's method of effecting this end is very simple. He takes an ordinary "beehive" coke-oven, makes it tolerably airtight by letting tar soak into the brickwork, and covers the floor with an impervious substance, in which are inserted some large bricks or quarls, pierced with holes. Below these is a chamber connected with a pipe, which leads, through any convenient form of condenser, to a small exhausting fan. The oven is now charged and lighted from the top, to which alone air is admitted. The heat of combustion, penetrating downwards, gradually distils the pitch and gases out of the coal, and the fan being set to work, these products, instead of passing upwards to the fire, are sucked downwards through the holes in the floor, and afterwards separated, the tar being left in one condenser, the ammoniacal liquor in another, and the gas either used at once for steam-raising, &c., or stored in a gas-holder till required.

In the discussion which followed, the advantage of saving the waste products was fully admitted, though some rather startling estimates of the author (who had assumed that 75,000,000*l.* per annum was practically wasted under our present system of coal consumption) were sharply criticised. But by the ironmasters who were present it was strongly laid down that the first duty of a coke-oven was to make good coke—such coke as would give the best results in a blast-furnace; and that to this duty all consideration of by-products must give way. It was further suggested that pitch was a valuable ingredient in coke, and that this pitch was left in it by the present system, but withdrawn on the new one. This idea, however, seems to be founded on a misapprehension. Mr. Jameson and others were able to state positively that the coke made by his process could not be distinguished in quality from the product of the old beehive oven; that the quantity per ton of coal was the same; and that the by-products, though differing very greatly in quantity according to the character of the coal, method of condensation, &c., were almost always sufficient to repay, within a few months or even weeks, the 10*l.* or 15*l.* required to adapt an existing oven to the new arrangement. If these results are confirmed by more extended trials in different localities, the process seems likely, as one speaker phrased it, "to take a pretty prominent position among the great inventions of the present day."

CORONERS' SCIENCE IN CHINA

WHETHER Chinamen are or are not believers in the principle that it is better that nine guilty persons should escape rather than that one innocent person should suffer, they do at all events, by their manner of conducting inquests, leave open a wide door for the escape of murderers. A deeply-rooted repugnance to dissection of the human body and a consequently slight acquaintance with anatomy, coupled with an entire ignorance of the action of poisons, deprive coroners of every means of arriving at decisions except those furnished by outward symptoms and appearances. From early times, however, the importance attaching to human life has been recognised by the custom of holding inquests in cases of sudden death, and various works have been published embodying all the knowledge available on the subject to assist coroners in their duty of investigation. The best-known of these was the *Se yuen luh*, or "Record of the washing away of wrongs," which was given to the world in the thirteenth century, and which, under the same title, subsequently received the *imprimatur* of the officers of the Board of Punishments, who, in the exercise of their legislative function, issued it as a manual for coroners. In this work is expounded the whole system of Chinese medical jurisprudence, of which the following is a slight sketch

One of the first directions given to coroners reminds one of Mrs. Glasse's celebrated dictum, and is to the effect that before issuing a warrant for an inquest they should be quite sure that there really is a corpse. This admonition is no less curious than the reason which makes it necessary. It appears to be not uncommon for unscrupulous swindlers to demand inquests on imaginary corpses for the purpose of extorting money from the wealthy owners of the houses where the bodies are said to be, who, rather than fall into the clutches of the law, generally pay the sum demanded on condition that all proceedings are stayed. But being well assured of the existence of a corpse, the coroner should proceed to the spot well provided with onions, red pepper, salt, white prunes, and vinegar with the lees. If death has just taken place, he should examine the top of the head, back of the ears, throat, and any other vital part where a sharp-pointed instrument may have been inserted. In case of his failing to find any such cause of death, he should interrogate the friends and neighbours, and then proceed to examine any wounds there may be on any other part of the person.

An infallible guide to the date of a wound is found in the colour of the bone affected. If it is a recent one or of a slight nature, the bone will be red, but if old and severe, the bone will be of a dark blue colour. Particular care should, however, be taken to ascertain that these colours are genuine, and not manufactured to agree with the story told by the relatives. A red tint may be given to the bone by painting it with an ointment of genuine safflover, sapanwood, black plums, and alum, with the addition of boiling vinegar. On the other hand, green alum or nutgalls, mixed with vinegar, impart a dark blue or black hue. These counterfeit colours may, however, be distinguished by their want of brightness. Again, not uncommonly a fictitious wound is made after death by burning the spot with lighted strips of bamboo, but such a wound will be level with the surrounding flesh and be soft to the touch. If willow bark has been used for the same purpose, the flesh will be rotten and black, livid all round, and free from hardness. A lighted paper placed inside a cup and applied to the flesh makes a wound which resembles the result of a blow with the fist; but it will be observed that all round there is a red, scorched mark, that the flesh inside is yellow, and that although it swells, it does not become hard. On the other hand, a genuine wound can be distinguished by the well-defined colours of the surrounding flesh. At the extremity of the wound there should be "a halo-like appearance, like rain seen from a distance, or like fleecy clouds, vague and indistinct."

Murders, it is held, are seldom the result of premeditation, but are in a great majority of cases to be traced to drunken brawls; and further, coroners should remember that the relatives of a wounded man, unless their ties be of the closest, desire his death that they may extort money from his slayer. It becomes their duty, therefore, on hearing of a fray in which any one has been seriously wounded, to see that the injured man be carefully tended and provided for. If death ensues, a careful examination of the corpse should be made, beginning from the head downwards, and in doing so, should it be suspected that tattoo-marks on his cheeks or elsewhere have been obliterated, such parts should be tapped with a slip of bamboo, which will have the effect of making the marks reappear. Attention should be given to see if the ears have been bitten or torn, whether the nostrils have been wounded, and whether the lips are open or closed. The teeth should be counted, the jaws examined, and the limbs carefully scrutinised down to the finger- and toe-nails. If the body bears marks of corporal chastisement, it should be noted, and any scars there may be, both on the inside and outside of the ankle-bones, may be safely set down to torture. When the mark of a wound which is known to have been inflicted cannot be traced, vinegar with the lees should be poured on the spot, and a transparent piece of oilcloth be held between the sun and part to be observed. On a dull day live charcoal must take the place of the sun. If the result be not satisfactory, spread powdered white prunes, with more vinegar and lees, and examine closely. Should this also prove unsatisfactory, then a cake composed of the flesh of white prunes, red pepper, onions, salt, and lees should be made very hot over a fire and applied to the parts, when the wound will appear.

In the same way, when violence is suspected, but no injury is at first sight apparent, it is directed that vinegar with the lees should be poured on the body, over which the clothes of the deceased saturated again with hot vinegar should be laid, and, covering all, mats spread to keep the steam in. The temperature of the vinegar should be regulated by the season of the year, and in very cold weather, when the vinegar, however hot, is insufficient to relax the rigidity, the corpse should be laid in a hole in the ground, in which a roaring fire has been subdued by copious sprinklings of vinegar. The fumes of steam which will then arise may be expected to accomplish the object. A careful examination should then be made, and if the marks of a wound or wounds are observed on the skin, their size, shape, and position are to be carefully noted, and death attributed to the one on the most vulnerable part. One of the most curiously perverted pieces of coroners' science is contained in the assertion that, if death has arisen from a blow on the lower part of the abdomen, the injury is discoverable by the condition of the roots of either the top or bottom teeth in the case of men, and in that of women by the appearance of the gums.

If the services of the coroner should not be called in until the body is in so advanced a stage of decomposition that the condition of the bones is the only test left him, he should choose a bright day, and having steamed them in the fumes of hot vinegar he should examine them under a red oilcloth umbrella. The blood having soaked into the injured parts, these will at once become visible, and will leave clearly-defined red, dark blue, or black marks. A long-shaped, dark-coloured mark so discovered points to a wound inflicted by a weapon, a round one to a blow of the fist, a large one to a butt of the head, and a small one to a kick. The fact of saturation of blood in the bone is evidence that the wound was inflicted before death. Should there be any doubt as to the identification of the bones, it is only necessary for a child or grandchild of the deceased to cut himself and herself with a knife, so that the blood may drip upon the bones, when, if they be really those of the parent, the blood will soak into them, otherwise it will not. In connection with this test it is curious to find stated the old-world belief that the blood of relations, if dripped into a basin, will mix, and not in the case of others. This test would appear to be often appealed to, since coroners—though it is difficult to see what it has to do with coroners—are warned to see that those interested in proving a relationship do not smear the basin with salt or vinegar, under the influence of either of which any bloods will mix.

Observations have shown, so coroners are told, that a man who has been killed with a knife dies with his mouth and eyes open and his hands clenched. The skin and flesh about the wounds will be shrunken, and in case of a limb having been cut off the bone will be protruding. Where decapitation has taken place, the muscles will have shrunk backwards, the skin will have curled over, and the shoulders will be shrugged up. These appearances will be wanting if the wounds have been made after death has taken place. It is necessary to be particular on these points, we are told, as murderers constantly endeavour to mislead coroners by inflicting wounds after death in such a way as to lend a colour to vamped-up stories of suicide. The exact frame of mind in which a man was when committing suicide can be readily discovered by the features of the corpse. If the teeth are firmly set, the eyes slightly open and looking upwards, a fit of violent passion prompted the act; if the eyes are closed, but not tightly, the mouth slightly open, and the teeth not shut, then it was due to an excess of pent-up rage; if fear of punishment has driven him to it, his eyes and mouth will be placidly closed, "for he looks on death merely as a return home and a happy release from the responsibilities of life." The hands also furnish a test when there is a doubt whether the case of a man whose throat has been cut be one of murder or suicide. The hand with which a suicide commits the deed will remain soft for a time, and will curl up a day or two after death, neither of which symptoms will occur when death has been caused by another person.

Strangulation is one of the commonest means by which persons tired of life "shuffle off this mortal coil," and full directions are given as to the points to be observed when holding inquests on such cases. The exact position of the body, the kind of scar on the neck, the existence or absence of the mark of a knot, the expression of the face, and a thousand other matters are detailed at length, and are contrasted with similar appearances in the case of murders. One curious piece of superstition receives the sanction of the Board of Punishment in connection with suicide by hanging. Beneath the spot where the crime was committed, at the depth of three or more feet below the surface of the soil, there will be found a deposit of charcoal, and by this test, should any doubt exist as to the scene of the suicide, the matter may be settled. The directions given in the case of deaths by drowning are voluminous, and, speaking generally, accurate. The habit of generalising from insufficient data, which is so common with Chinamen, occasionally leads them astray here as elsewhere. It has been reserved for them, for example, to discover the law that bodies take a longer time to float in winter and the beginning of spring than in the summer and end of autumn. That a drowned man floats on his face and a woman on her back is mentioned, and it is left to be implied that in case of bodies having been thrown into the water after death this does not hold good. With the same minuteness every possible circumstance connected with death by fire is gone into at length, the presence of traces of ashes in the mouth and nose being described as "a crucial test of death by burning."

The chapters on poison are, as might be expected in the absence of dissection, the most unsatisfactory in the book. Practically very little light is thrown on the distinguishing symptoms arising from the effects of different poisons. The common test applied to most is that of inserting a silver needle washed with a decoction of *Gleditschia sinensis*, into the mouth of the corpse. If, when after a time this is withdrawn, it should be stained a dark colour, and remain so stained after it has been again washed with the decoction, poison has been the cause of death. Another proof is furnished by the effect which a pellet of rice, after having been some time in the mouth of the corpse, has on poultry who can be induced to swallow it. The commonest poisons are said to be opium, arsenic, and certain noxious essences derived from herbs. But besides these, other things are taken by suicides and given by murderers to cause death. In some of the southern provinces there exists a particular kind of silkworm, known as the Golden Silkworm, which is reared by miscreants to serve either purpose as occasion may require. Quicksilver, which is also used with fatal effect, is either swallowed, or, like the "juice of cursed hebenon" which sent Hamlet's father to his account, is poured into the ear. The torture necessarily consequent on this last method of using it must be so excessive that it may safely be assumed that it finds favour only with murderers. Swallowing gold, on the other hand, seems to be the favourite way of seeking death with wealthy suicides. It has been held by some writers that the expression "swallowing gold" is but a metaphorical phrase meaning "swallowing poison," just as when a notable culprit is ordered to strangle himself he is said to have had "a silken cord" sent to him. But the "Coroners' Manual" puts it beyond question that gold is actually swallowed, and it prescribes the remedies which should be adopted to effect a cure. Gold not being a poison, death is the result either of suffocation or laceration of the intestines. When suffocation is imminent, draughts of strained rice-water, we are told, should be given to wash the gold downwards, and when this object has been attained, the flesh of partridges, among other things, should be eaten by the patient to "soften the gold" and thus prevent its doing injury. Silver is also taken in the same way. But though wealthy Chinamen thus find a pleasure in seeking extinction by means of the precious metals, they have never gone the length of pounding diamonds to get rid of either themselves or their enemies after the manner of Indian potentates.

ROBERT K. DOUGLAS

ZOOLOGY IN JAPAN

A CORRESPONDENT in Tokio sends us the following:—During the late summer and autumn some good work has been done in the ornithological way. Mr. P. L. Jouy, of the Smithsonian Institution, collected extensively in the region of Mount Fujiyama, at Chiu-senji Lake, near the celebrated shrines of Nikko, and on Tateyama Range, between the borders of the provinces of Shinshiu and Hida. A large number of beautifully prepared skins, with a good deal of information regarding the breeding habits of some of the rarer birds, is the result, which will be recorded in the February number of the *Chrysanthemum*, a magazine published at Yokohama, appearing in enlarged form with the commencement of this year. An article contributed by Capt. Blakiston in the January number, follows up those of his for September, October, and November, 1882, on ornithological work in Yezo during the past summer; in which is noticeable the occurrence of *Locustella certhiola* (Pall), and *Phylloscopus borealis* (Blasius) on that island; and the discovery of a new species of *Motacilla* (probably described by Seebohm in the *Ibis* for January, 1883), allied to *M. ocularis* (Swinhoe) and *M. amurensis* (Seebohm), which has hitherto somehow been mixed up with *M. lugens* of the "Fauna Japonica," which latter is now found to be —to quote Capt. Blakiston's words (*Chrysanthemum*, January, 1883, p. 31)—"a species unique in its genus, having in the adult state the same appearance winter and summer, and in which the young pass at once before their first winter into the adult dress"

Messrs. Owston, Snow, and Co.'s otter hunters at the Kuril Islands have also during the past season added some new localities for Japan birds. The specimens are in the hands of Capt. Blakiston, and will be duly mentioned in the following number of the *Chrysanthemum*, as additional notes to the "Birds of Japan," *Trans. As. Soc. Japan*, vol. x. part 1 (noticed in NATURE, vol. xxvi. p. 362).

In the way of *mammalia* late investigation points to the distinctness of Yezo from Japan proper. The Rev. Père Heude, who is now engaged upon a revision of the *Cervidæ* of Eastern Asia, has come to the conclusion that the common deer of Yezo is not *C. sika* of the "Fauna Japonica," but *C. manchuricus-minor*, or an undescribed species. Two parts are already published—very creditably got up at the Mission Press at Sikawei, near Shanghai—cf. "Mémoires concernant l'Histoire Naturelle de l'Empire Chinois," others being promised to follow.

NOTES

THE following is the list of fifteen candidates recommended for election by the Council of the Royal Society:—Surgeon-Major James Edward Tierney Aitchison, M.D., James Crichton Browne, M.D., LL.D., Surgeon-Major George Edward Dobson, M.B., James Matthews Duncan, M.D., Prof. George Francis Fitzgerald, M.A., Walter Flight, D.Sc., Rev. Percival Frost, M.A., David Gill, LL.D., Charles Edward Groves, F.C.S., Howard Grubb, F.R.A.S., John Newport Langley, M.A., Arnold William Reinold, M.A., Roland Trimen, F.L.S., F.Z.S., John Venn, M.A., John James Walker, M.A.

THE loss sustained by mathematical science in the premature death of Henry Stephen Smith is still fresh in the minds of our readers. They will find their best consolation in the fact that his successor in Oxford may possibly be Prof. Sylvester. Such an opportunity of recovering for England the services of one of her two greatest mathematicians is not likely to recur, and will, we doubt not, be eagerly turned to advantage. It has been a humiliating thought to many to whom the highest interest of science is dearer than the prosperity of mere mediocrity that, of the two greatest mathematicians that England has produced in the nineteenth century, one has altogether and another almost been obliged to seek for refuge in a foreign land.

UNIVERSAL regret will be felt at the sad intelligence which has just reached England by telegram, from Madeira, of the untimely death of Mr. William Alexander Forbes, B.A., Fellow of St. John's College, Cambridge, and Prosector to the Zoological Society of London. Mr. Forbes left England in July last, along with Mr. McIntosh and Mr. Ashbury, upon what was expected to be a three or four months' expedition in a steam-yacht up the river Niger. He died of dysentery at Shonga on January 14, aged 28.

THREE months ago Mr. Raphael Meldola, as retiring President of the Essex Field Club, gave an interesting address, which is now printed in a separate form, on "Darwin and Modern Evolution." It gives a clear and well-condensed account of Darwin's life and work. The following extract concerning the first publication of the theory of natural selection at the Linnean Society is of historical interest, and also, we think, of some instructive value:—"Mr. Wallace has narrated to me that one of his correspondents, a well-known entomologist, wrote to say that it was a general remark in natural history circles, with respect to the paper, that it was much to be regretted that the author had not more confined himself to statements of fact!" This shows that the naturalists of the Linnean Society at that time had the same intolerance of anything like speculative brain-spinning which still finds occasional expression. But to-day we have to thank the sagacity of the greatest of naturalists that, while cautious of speculation, he nevertheless courted it as a friend to the highest interests of science, while leaving "the well-known entomologists" to shun it as the worst of enemies. The truth is that in biology, as in all other branches of science, unless the only aim of a worker is to accumulate knowledge of details, he is bound to resort to hypotheses as feelers after principles. On the other hand, of course, speculation, like fire, while the most valuable of servants, may also be the most dangerous of masters. The truest scientific judgment, therefore, consists in using speculation as not abusing it; and if in particular cases it is asked how much latitude is thus to be allowed to speculative thinking, the answer must be that this is just the question which in all particular cases it requires the truest scientific judgment to decide. All that can be said, as a matter of general principle, is that quite as much and even more harm may arise from an over-nervousness of deductive method in biology, as may arise from an over-confidence in them; and also that the theory of evolution—at least in our opinion—is now sufficiently well established to admit of being used deductively in no stinted measure, without danger of violating the best methods of scientific procedure.

THE great and deserved success which has attended the Girls' Public Day School Company has now led to the formation of a similar company for establishing schools for boys. A meeting, under the presidency of Lord Aberdare, was held on the 24th inst. at the rooms of the Society of Arts, at which the objects of the company were explained. The basis of the new schools is that of a self-supporting company, independent alike of Government and charitable aid. It is stated that premises will shortly be secured in Kentish Town, where the first school will be opened in a few months.

CAPT. C. E. DUTTON, of the United States Geological Survey, who spent half of last year in Hawaii studying the volcanic phenomena there, and whose researches among the plateaux of Utah have brought to light so many interesting phases of volcanic action in that region, is about to undertake the exploration of a still more extensive volcanic region. He is organising his forces for a summer campaign in the Cascade Range, beginning at the southern end in California, among the volcanic piles of Mount Shasta, and working northwards across Oregon to the remote peaks of Mounts Hood and Rainier, in Washington Territory. In this way a preliminary survey of the region will this year be made, and the information will be gained that will serve as the basis for future more detailed exploration. That vast region contains possibly the most colossal outpouring of volcanic matter anywhere to be seen in the world. Geologists will rejoice that it is now to be systematically examined by one so competent as Capt. Dutton, who has specially trained himself for the task. The American Congress is to be congratulated on the enlightened spirit in which these surveys of the Western Territories are conceived and carried out.

THE Central Swedish Meteorological Observatory, in Stockholm, has issued a request, signed by Baron Nordenskjöld, calling upon those who may witness meteoric phenomena to send minute particulars of the same to him. He requests that the following details may be noted:—Time, duration, direction as well as height above horizon, whether the meteor had a tail, emitted smoke, burst, or simply disappeared from view, whether any sound and any fall of objects were observed. He also requests that a drawing of the phenomena may if possible be forwarded. In conclusion, he says: "There often appears a peculiar dry mist or 'sun-smoke' over extensive tracts of land in Sweden, sometimes accompanied by a remarkable smell extending for hundreds of square miles. The nature of this phenomenon has not yet been ascertained. As I am informed that it was recently noticed in certain parts of Norrland, I beg that any observer of the same will forward all particulars he may possess."

GEOLOGISTS will learn with regret that Mr. Alexander Murray, who has so long and so ably directed the Geological Survey of Newfoundland, feels himself compelled by advancing years and enfeebled health to retire from his duties. For many years he was one of the late Sir William Logan's chief officers in the Geological Survey of Canada, where he long ago gained his geological spurs. His iron constitution and indomitable enthusiasm have carried him through more hardships than have fallen to the lot of almost any living geological explorer, but they have been borne with a quiet courage and good-humoured indifference altogether admirable. May he find now the honourable rest and recognition to which his long devotion to the colony so justly entitles him. He will be succeeded by his present second in command, Mr. James P. Howley, in whose experienced hands the Surveying Department of Newfoundland will be excellently administered.

PROF. TYNDALL will on Thursday next, May 3, at the Royal Institution, give the first of a course of three lectures on "Count Rumford, Originator of the Royal Institution."

ALTHOUGH the circulation of books from Newcastle-upon-Tyne Free Library has not quite kept up this second year, yet the Report, with its account of the handsome new building, is a very satisfactory one. The carrying on of education in various ways, in combination with the Science and Art Department and with the City and Guilds of London Institute, by literary and commercial classes, including even a competition in oratory supported by a "Bequest," is valuable work that ought naturally to fall, as it has done here, into the same hands as control the library. The method of encouraging juvenile readers by permitting the use of the whole library to the more intelligent is good where these readers are sufficiently known to the librarian. There is no doubt that the true reason is given for the large increase in the issue of fiction, viz. that the committee have added to their stock in that class in a proportion twice as great as in any other class. "The love of" fiction "increases as much as the" fiction "itself increases."

THE Mitchell Library at Glasgow has taken the only method of repressing this circulation of fiction, viz. that of not buying the books! In this well-endowed and promising institution, only open five years, yet now containing 45,000 volumes, there are only 374 volumes of fiction; yet so great is the demand for this class of reading that every volume has been issued ninety-eight times, *i.e.* changed twice every week throughout the year! This library, however, supported mainly by the splendid bequest of 70,000*l*. left by Mr. Stephen Mitchell in 1874, has purposely relegated this department and that of branch libraries to the 1*d*. rate, able to produce in Glasgow 11,000*l*. a year, while itself takes the form of a great reference department, already the largest free library in Scotland, and promising in a second five years to be among the most important collections of books in the kingdom. One of the best functions of a public library in any town is to become the centre to which will gravitate all publications of any local value or interest. For since every subject or author is naturally connected with some locality, if this were well carried out all over the kingdom, information would gradually be as well arranged and as readily accessible as in a cyclopædia. The collections undertaken by the Mitchell Library at Glasgow are (1) the works of Burns and other Scotch poets and verse writers, one object of which will be "to preserve local dialects, local customs, and local memories"; (2) all papers which in any way illustrate the city's growth and life; (3) specimens of early Glasgow printing. The Scotch Covenanters is another subject in which a collection of publications has been commenced. Still nearly one-fifth of the volumes in the Mitchell Library, and more than one-fifth of the volumes in circulation, belong to the department of Arts and Sciences. The attendance of readers has been quite as large as the present premises will accommodate.

THE Council of the Society of British Artists opened their gallery in Suffolk Street on Sunday last to the members of the Sunday Society. A similar privilege has been granted for Sunday next, the 29th inst., when the public will be admitted during the afternoon and evening by free tickets, to be had by all who apply, inclosing a stamped and addressed envelope, to the Honorary Secretary, 8, Park Place Villas, W. The eighth annual meeting of the Sunday Society will be held in the Princes' Hall, Piccadilly, on May 5, under the presidency of Sir Coutts Lindsay.

WE have received the Memorandum of Association of the National Smoke Abatement Institution, signed by the Dukes of Westminster and Northumberland, Lord Mount-Temple, Sir W. F. Pollock, Sir Lyon Playfair, Sir Hussey Vivian, and Mr. Ernest Hart. The objects of this institution are already well known to all our readers.

THE diplomas and scholarships of the Spring Session of the Royal Agricultural College, Cirencester, were conferred on the successful students on the 19th inst. Among those on whom the diplomas were conferred were Messrs. Sen and Hossein, the two Indian scholars first sent to the College by the Government of India. It is worthy of note that one of these gentlemen, Mr. Sen, obtained the highest number of marks ever reached for the diploma.

A LARGE collection of weapons and implements from the Stone Age in Japan has, we are informed, arrived in London. The collector, Herr von Siebold, is an official of the Austrian Embassy in Japan, and has resided for many years in the latter country. The collection embraces, we believe, a large number of flint arrowheads, celts, axes, as well as numerous specimens of pottery taken from shell-heaps in various parts of Japan. The well known *magatama* and *kudatama* ornaments are also well represented. Except a few in the Christy Collection in the British Museum, and a small collection given by Herr von Siebold himself to the Copenhagen Museum, the Japanese Stone Age is not, we believe, fairly represented in any archæological museum in Europe.

MORE than 250 years ago the English residents in Japan were perplexing themselves, as they are to-day, on the subject of earthquakes. In the diary of Richard Cocks, just published for the Hakluyt Society by Mr. Maunde Thompson, we find, under date November 7, 1618, the following entry:—"And, as we retorned, about 10 a clock, hapned a greate earthquake, which caused many people to run out of their howses. And about the lyke hower the night following hapned an other, this cuntrey being much subject to them. And that which is comunely markd, they allwais hapen at a hie water (or full sea); so it is thought it chanseth per reason is much wind blowen into hollow caves under grownd at a loe water, and the sea flowing in after, and stoping the passage out, causeth these earthquakes, to fynd passage or vent for the wind shut up."

NEWS from Mr. Stanley dating down to the middle of December has just been received at Brussels. Stanley had reached the African coast, and, after having augmented his party by 223 natives from Zanzibar, under the leadership of the Belgian traveller, M. de Cambier, had started for Vivi, the first station established by the International African Society. At Vivi preparations were being made for the construction of a railway line to the landing-place on the river, but the work proceeded slowly, owing to the total absence of beasts of burden. Up to now seven stations have been established—Vivi, Isanghila, Manyangha, Lutété, Stanley Pool, Ibaka Nkoutou, and Bolobo; the latter is distant about 700 miles from the mouth of the Congo, and is the last one established. Of the four small steamers taken to Africa three had been launched, and the fourth was being transported from Manyangha to Leopoldsville. The seven stations already seem to become centres of civilisation, and exercise a beneficial influence upon the surrounding native tribes. Horned cattle had been introduced at Vivi, and at Leopoldsville agricultural work had begun, cabbage and lettuce thriving exceedingly in that locality. At Bolobo a fertile and well-populated country was reached, which extends far beyond the limits of De Brazza's journey. The progress of the latter was contemplated with equanimity, yet fears were entertained regarding the claims of the Portuguese Government, and also concerning the freedom of way and commerce.

SEVERAL Swedish officers have recently left Europe, being invited to join Mr. Stanley on the Congo.

THE Swedish Academy of Sciences has offered a reward to the vessel which first brings despatches, &c., to the meteorological observing party wintering at Spitzbergen.

THE despatch of the Swedish corvette *Vanadis* on a voyage round the world is contemplated. Several men of science will accompany her, among whom is Dr. Stolpe, the well-known ethnographist.

ON the 13th inst., between 8 and 9 a.m., a remarkable mirage was seen at Ölsta, in the parish of Sala, Sweden. It displayed distinctly a town in Eastern style situated by the sea, with temples and minarets, while to the left a forest of fine cypress trees was seen. In front was a train in motion, while a body of soldiers appeared marching along a road, with their bayonets flashing in the sun. The whole was visible for nearly an hour, when it gradually faded away.

THE French Academy of Sciences, at its meeting on Monday last, selected MM. Bonnet and Resal as candidates for filling the place vacated by the decease of M. Liouville, in the Bureau des Longitudes.

LAST week M. de Lesseps delivered several speeches at the Sorbonne and in other places, showing that the Roudaire Inland Sea will be useful and profitable, and the speaker met with very decided success. On Monday, M. Cosson, his usual antagonist, delivered a long speech, pointing out the danger of the operations, but the French Academy of Sciences took no notice of it, and no commission being appointed the matter dropped.

AT the March meeting of the Russian Physical Society, M. Sreznevsky read a communication on an instrument largely employed but the theory of which is from being established, namely, the hygrometer of Saussure. Its scale, usually traced by comparison with a psychrometer, varies with the month when the comparison is made, a difference which is probably due to influence of temperature, as already pointed out in 1783 by Saussure. The matter, however, has never yet received thorough investigation. The cause of the elongation of the air in consequence of an increase of moisture remained also unexplained. It might be explained now, however, as it is known that the air contains water in a liquid state in its microscopical cavities. The curvature of the surfaces of these microscopical meniscuses, which depends upon the tension of the vapour that incloses the air, must influence the tension on its surface and therefore change its length. Both these causes can be expressed mathematically, at least for the simplest cases, and if we admit a state of equilibrium we can easily see that the tension of the meniscuses on the surface of the air is a function of the relative moisture, and is proportionate to the logarithm of the moisture. The elongation of the air would thus be a function of the relative moisture of a capillary constant, of the coefficients of elasticity of the air, and of the suspended weight.

DR. ARNOD DODEL PORT has recently published the final part of his incomparable "Atlas der physiologischen Botanik." The six plates which constitute it illustrate: *Cystosira barbata*, I. Ag. (a genus of sea wracks), the archegonium and antheridium of *Marchantia* (one of the Liverworts), *Pinus laricio* (third plate), *Lavatera trimestris*, two plates (a genus of Malvaceæ), and *Datura stramonium*, L. (the common thorn-apple). Together with the plates is published the final part of the descriptive text.

IN the current number of the *Annales de l'Extrême Orient*, M. de Lucy-Fossarien draws attention to the interesting fact in connection with education in Japan, that a large part of its development is due to private assistance. In the past five years forty-two millions of francs have been given voluntarily by private persons for the extension of education. Even this large sum, however, is probably less than the value of the land, houses, &c., given in particular districts for the use of schools.

THE tenth annual Report of the Museum für Völkerkunde, at Leipzig, has just been published, and gives an interesting account of the flourishing condition of this excellent ethnographical institution. The Emperor of Germany again contributes a large sum to the funds of the Museum, and the Crown Prince of Austria has become a member of the institution; the collections have been largely increased, and there are no less than 106 gentlemen at work in various parts of the world extending the connections of and acquiring material for the Museum.

AN earthquake was observed at Tashkend on March 31, at 7 a.m. The shocks were of considerable violence. In the Etna district the volcanic phenomena continue. A violent earthquake occurred at Riposto on the 5th inst., and on the following day oscillations were felt also at Catania, Paternó, and Randazzo. A thick volume of steam emanates from the crater as well as from lateral openings. At Salinella the mud crater had resumed its activity and had caused considerable destruction of property.

DR. PAUL GÜSSFELDT of Berlin, the eminent traveller who started for South America some time ago in order to make geological and other scientific researches in the Cordilleras, reports that he is well satisfied with the results of his journey, and that he had discovered a glacier of the first order in the style of the Aletsch glacier. The glacier is between fifteen and twenty miles in length. Dr. Gussfeldt has measured many summits trigonometrically, made a collection of alpine plants (amongst them a wild potato from above the glacier), and another of geological specimens. On December 31 he intended to leave for the Argentine Republic; thence he proposed to return to Maipu, and then investigate the Aconcagua district.

A NUMBER of unusually bright and large meteors were observed at Prossnitz (Austria) and other places in the neighbourhood on the evening of March 13 last, between 6 and 11 p.m. Some lit up the whole sky and lasted five or six seconds. No trace of any meteoric stone has as yet been discovered.

THE additions to the Zoological Society's Gardens during the past week include a Leopard (*Felis pardus* ♀) from India, presented by Mr. A. P. Marsden; an Ocelot (*Felis pardalis*) from South America, presented by Mr. C. G. Leith; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. E. Dance; two Porto Rico Pigeons (*Columba corensis*) from the West Indies, a Common Boa (*Boa constrictor*) from Brazil, presented by Mr. C. A. Craven, C.M.Z.S.; an Osprey (*Pandion haliaetus*) from Australia, presented by Dr. Plummer; a White-bellied Sea Eagle (*Haliaetus leucogaster*) from Australia, presented by Mr. E. P. Ramsay, C.M.Z.S.; three Common Rheas (*Rhea americana*) from Monte Video, presented by Mr. John Fair; a Green Turtle (*Chelone viridis*) from the West Indies, presented by Mr. Fleetwood Sandeman; a Leopard (*Felis pardus* ♂) from India, a Small Hill Mynah (*Gracula religiosa*) from Southern India, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, a Gannet (*Sula bassana*), British, deposited; an Iceland Falcon (*Falco islandus*) from Iceland, purchased.

OUR ASTRONOMICAL COLUMN

SCHMIDT'S VARIABLE STAR NEAR SPICA.—On June 6, 1866, Dr. Julius Schmidt remarked to the south and east of Spica a conspicuous star which he estimated 5·4m., and which was wanting in Argelander's *Uranometria*. It was brighter than the neighbouring reddish-yellow star, 68 Virginis. He found its place for 1866·0, R.A. 13h. 27m. 33s., Decl. −12° 31′·5. It is Lalande 25086, estimated 6·7 on May 10, 1795, and Piazzi XIII. 126, called 8m. in the catalogue, but 7 and 6·7 in the *Storia Celeste*. It was not observed by Bessel nor Santini, but occurs in Lamont's Zone 355, 1846, May 22, when it was rated 8m. In Bremicker's Berlin Chart it is 7m., and 6·7 in Heis. But a special point of interest about this object is Schjellerup's inference that it is identical with the 19th star in Virgo in Ptolemy's Catalogue, as indicated in a note at p. 160 of the translation of the Catalogue of Abd-al-Rahman al-Sûfi, which the Persian astronomer says was of the smaller fifth magnitude, nearer the sixth, though Ptolemy calls it "absolutely of the fifth." In Baily's edition of Ptolemy's Catalogue in vol. xiii. of the *Memoirs of the Royal Astronomical Society*, the star in question is No. 515, and there identified with 68 *i* Virginis: it is called *ὁ νοτιώτερος τῆς ἑπομένης πλευρᾶς*. Schjellerup, translating from Al-Sûfi, says: "La 19^{e} est la méridionale du côté postérieur du quadrilatère, après *al-simâk*, s'inclinant vers le sud; elle est des moindres de la cinquième grandeur; Ptolémée la dit absolument de cinquième, mais elle est plus près de la sixième. Entre elle et *al-simâk* vers le sud-est, il y a environ une coudée et demie et entre elle et la 17^{e} il y a la même distance. Avec *al-simâk* et la 17^{e} elle forme un triangle isoscèle, cette étoile étant au sommet. La latitude de cette étoile, indiquée dans le livre de Ptolémée, se trouve erronée, parce que, au ciel, elle se fait voir autrement qu'elle ne tombe sur le globe. Car, d'après cela, elle devrait se faire voir au nord d'*al-simâk*, tandis que, en verité, elle se trouve au sud." Al-simâk is Spica, and the 17th star appears to be 76 Virginis. Baily in his Catalogue places the 19th star in longitude 178°, with 3° 0′ south latitude, but in a note he points out that in the edition of Ptolemy, published by Liechtenstein at Venice in 1515, the latitude is 0° 20′ and *north*; with the remark, "The star 68 Virginis agrees with the position given by Ptolemy; but it is difficult to make it accord with the description, as being in the 'latus sequens' of the quadrilateral figure."

Both the variable and 68 Virginis are found in Mr. Stone's Southern Catalogue, the epoch of which is 1880. The auxiliary quantities for the reduction of positions for this year to the assigned epoch of Ptolemy's Catalogue, the first of Antoninus, are, in the usual notation—

A ... 168° 47′·3 ... A′ ... 191° 0′·8 ... θ ... 9° 40′·6, whence with the obliquity of the ecliptic = 23° 41′·1, Stone's places for A.D. 138 become—

	Longitude.		Latitude
Var. Schmidt (Piazzi, xiii. 126) ...	180° 52′	...	−2° 58′
68 Virginis	178° 53′	...	−3° 14′

As we have seen, Ptolemy's 19th star of Virgo is placed in longitude 178° 0′, latitude −3° 0′; but, as is well known, the longitudes of the Almagest are about one degree too small. Hence Schjellerup's identification of the variable with Ptolemy's star is likely to be correct; the object deserves frequent attention.

D'ARREST'S COMET.—With reference to the remarks last week in this column on the first announcement of the observation of D'Arrest's comet in the Dun Echt Circular, Prof. Krueger, Director of the Observatory at Kiel, writes us from that establishment, as the "Centralstelle für astronomische Telegramme," as follows:—"I wish to state with reference to No. 703, p. 589, as I have done in *A. N.* No. 2507 [not yet received], that Dr. Hartwig had not telegraphed any daily motion of the supposed comet D'Arrest on the 4th April. The hypothetical daily motion was added by myself in the cable-telegram to Cambridge, U.S., because I assumed that the American astronomers were not in possession of an ephemeris. Lord Crawford received, as usual, the same telegram as Cambridge, U.S., with the additional note (in order to avoid double-telegrams) that the telegram had been sent to America. European astronomers received only Dr. Hartwig's original communication."

ON THE SENSE OF COLOUR AMONGST SOME OF THE LOWER ANIMALS[1]

AT the meeting of the Linnean Society on Thursday, April 19, Sir John Lubbock read a paper on this subject. Some years ago M. Paul Bert made a series of interesting experiments with the common Daphnia, or water-flea, which is so abundant in our ditches and pools. He exposed them to light of different colours, and he thought himself justified in concluding from his observations that their limits of vision at both ends of the spectrum are the same as our own, being limited by the red at one end, and the violet at the other.

In a previous communication Sir John Lubbock showed, on the contrary, that they are not insensible to the ultra-violet rays, and that at that end of the spectrum their eyes were affected by light which we are unable to perceive. These experiments have recently been repeated by M. Merezkowski, who, however, maintains that, though the Daphnias prefer the yellow rays, which are the brightest of the spectrum, they are, in fact, attracted, not by the colour, but by the brightness; that, while conscious of the intensity of the light, they have no power to distinguish colours. Given an animal which prefers the brightest rays, it may seem difficult to distinguish between a mere preference for light itself rather than for any particular colour. To test this, however, Sir John Lubbock took porcelain troughs about an inch deep, eight inches long, and three broad. In these he put fifty Daphnias, and then, in a darkened chamber, threw upon them an electric spectrum arranged so that on each side of a given line the light was equal, and he found that an immense majority of the Daphnias preferred the green to the red end of the spectrum. Again, to select one out of many experiments, he took four troughs, and covered one-half of the first with a yellow solution, half of the second with a green solution, half of the third with an opaque plate, and he threw over half of the fourth a certain amount of extra light by means of a mirror. He then found that in the first trough a large majority of the Daphnias preferred being under the yellow liquid rather than in the exposed half; that in the second a large majority preferred being under the green liquid rather than in the exposed half; that in the third a large majority preferred the exposed half to that which was shaded; and in the fourth that a large majority preferred the half on which the extra amount of light was thrown.

It is evident, then, that in the first and second troughs the Daphnias did not go under the solution for the sake of the shade, because other Daphnias placed by their side under similar conditions preferred a somewhat brighter light.

It seems clear, therefore, that they were able to distinguish the yellow and green light, and that they preferred it to white light. No such result was given with blue or red solutions. In such cases the Daphnias always preferred the uncovered half of the trough.

It is, of course, impossible absolutely to prove that they perceive colours, but these experiments certainly show that rays of various wave-lengths produce distinct impressions on their eyes; that they prefer rays of light of such wave-lengths as produce upon our eyes the impression of green and yellow. It is, of course, possible that rays of different wave-lengths produce different impressions upon their eyes; but yet that such impressions differ in a manner of which we have no conception. This, however, seems improbable, and on the whole, therefore, it certainly does appear that Daphnias can distinguish not only different degrees of brightness, but also differences of colour.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Prof. Dewar commenced a short course on Chemical Technology in its relation to Organic Chemistry on April 23.

Mr. Sedgwick is lecturing on the Embryology of Mammals and Birds, and Mr. Caldwell on the Morphology of Gephyrea, Brachiopoda, Polyzoa, Chœtognatha, and Larval Forms, practical work accompanying both courses.

Dr. Hans Gadow is lecturing on the Tegumentary and Muscular Systems of the Vertebrata.

Prof. Darwin's lectures on the Theory of the Potential will include an account of Gauss's treatment of those problems generally associated with the name of Green.

The Demonstrator of Mechanism is giving a course of Mechanics applied to the strains in winding, pumping, and blast engines, and in other machines. A practical class is being formed for instruction in Surveying.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 12.—"Introductory Note on Communications to be presented on the Physiology of the Carbohydrates in the Animal System." By F. W. Pavy, M.D., F.R.S.

My last communication (*Proc. Roy. Soc.*, vol. xxxii. p. 418) was entitled "A New Line of Research bearing on the Physiology of Sugar in the Animal System."

During the time which has since elapsed, I have been actively continuing my investigations in the direction started, and the results obtained give an entirely new aspect to the whole subject of the physiology of the carbohydrates in the animal system.

Modern research has shown that, besides the well-known carbohydrate principles, such as sugar, &c., there are several dextrins distinguishable by their optical properties and their cupric oxide reducing power.

From the colloidal principle starch, which has no cupric oxide reducing power, principles (dextrins) are producible by the action of ferments possessing gradually-increasing cupric oxide reducing power until maltose is reached, which constitutes the final product, and which possess a little more than half the cupric oxide reducing power of glucose.

This is one foundation point connected with the researches I have been conducting upon the physiology of the carbohydrates in the animal system.

The other foundation point is that the various members of the carbohydrate group are brought into glucose by the agency of sulphuric acid and heat.

Proceeding upon these facts, and taking the cupric oxide reducing power before and after subjection to the converting action of sulphuric acid and heat, I have prosecuted investigations upon the transformation of the carbohydrates within the animal system with the result of acquiring knowledge of an altogether unexpected nature.

Hitherto what has been observed as regards the transformation of carbohydrates by the action of ferments and chemical agents, has been a change attended with increased hydration—for example, the passage of starch into the successive forms of dextrin and maltose and cane-sugar into glucose.

The issue of the researches, however, which I have been conducting recently, is to demonstrate the passage of carbohydrates exactly in the opposite direction by the action of certain ferments existing within the animal system.

Alike in the alimentary canal, the circulatory system, and the liver, the conditions exist by which this kind of transformation is effected.

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[1] By Sir John Lubbock, Bart., M.P.

From the mucous membrane of the alimentary canal a ferment is obtainable which converts (1) glucose into a body possessing the same kind of cupric oxide reducing power as maltose; (2) cane-sugar into maltose, and not glucose as formerly asserted; and (3) starch either into maltose or a dextrin of low cupric oxide reducing power.

The presence of carbonate of soda modifies the action of a maltose-forming ferment, and leads to starch passing into a dextrin of low cupric oxide reducing power instead of into maltose.

The portal blood contains a ferment which possesses a maltose or a dextrin-producing power, and the contents of the portal system during digestion are charged with a notable amount of maltose sometimes, and at other times a low cupric oxide reducing dextrin.

After the introduction of glucose into the circulatory system, I have observed the presence of maltose.

The liver also contains a ferment capable, under certain conditions, of carrying glucose into maltose, and I have further witnessed, by the same kind of action as the sugars and dextrins are moved from one to the other, the conversion of a carbohydrate into the colloidal material belonging to the animal system (glycogen) which holds the analogous position of starch in the vegetable system.

Evidence has likewise been supplied that by an action of the same nature as that which moves the carbohydrates from one to the other in the carbohydrate group, they are, under certain conditions, carried into a body out of the group, and thence not susceptible of being brought into glucose by the converting action of sulphuric acid; and, on the other hand, under other conditions a substance is brought into the carbohydrate group, and its nature made recognisable by the converting action of sulphuric acid and its cupric oxide reducing power.

The subject as it even now presents itself is a large one, and I propose to deal with it in detail in a series of communications. The first will be devoted to that which refers to the alimentary canal.

Linnean Society, April 5.—Sir John Kirk, vice-president, in the chair.—Messrs. R. M. Barrington, G. E. Comerford-Casey, F. V. Dickins, and E. Cambridge Phillips were elected Fellows of the Society.—Mr. E. M. Holmes exhibited a specimen of birch-tree sap which had been found to exude from a cut branch one inch in diameter, at the rate of 4 oz. per hour during the night and 7 oz. to 8 oz. per hour during the day before the leaf buds had expanded, showing that the rapid rise of the sap was in this case not dependent on transpiration, but probably on endosmose accelerated by the expansion of the wood caused by solar heat. The sap had been collected and analysed by Dr. Attfield, and its contents recorded in the *Pharmaceutical Journal*.—There was exhibited for Mr. R. Morton Middleton a well-marked example of wood showing the extensive ravages of the Isopod, *Limnoria lignorum*. The wood was from the pier piles of West Hartlepool, where the said Crustacean's depredations are very destructive.—The Secretary read a paper on the indiarubber-tree of the Gold Coast, by Capt. Alf. Moloney. In this the author stated that the *Landolphia owariensis* grows extensively in the countries of Akim, Aquapim, and Croboe; and he strongly recommended the natives and traders of Lagos to encourage rubber as an article of trade instead of solely depending as at present on palm oil. He described the habit of the live plant, and the method employed in extracting the rubber therefrom.—Mr. F. W. Phillips in a communication described a new species of freshwater Infusorian allied to the genus *Gerda*. It was proposed provisionally to name the new form *G. caudata*. It was obtained at Hertford, and in company with the rotifer *Æcistes pilula*.—A paper was read on *Hemicarex*, Benth., and its allies, by Mr. C. B. Clarke; in this he gives a revision of the genera and species of *Kobresia*, *Hemicarex*, *Schœnoxiphium*, and *Uncinia*.

Zoological Society, April 3.—St. George Mivart, F.R.S., vice-president, in the chair.—The Secretary read some extracts from a letter he had received from Mr. J. Sarbo in reference to the Gayal. The writer observed that *Bos gaurus* (the Gaur) and not *Bos frontalis* (the Gayal) is the Wild Ox of Assam, and that the *B. frontalis* is not known in a wild state, but only as a semi-domesticated animal owned by various wild tribes from Assam to Arracan.—Mr. Sclater called the attention of the meeting to the skin of a Brown Crow from Australia, which had been sent to him for examination by Mr. Albert A. C. Le Souef, C.M.Z.S., and which he was inclined to regard as a variety in plumage of *Corvus australis*.—Mr. A. G. Butler read a paper containing an account of a collection of Indian Lepidoptera made by Lieut.-Col. Charles Swinhoe, chiefly at Kurrachee, Solun, and Mhow. Thirty-two new species were described, and numerous field-notes by Col. Swinhoe were incorporated in the paper.—Col. J. A. Grant read some notes on the Zebra met with by the Speke and Grant Expedition in the interior of Central Africa in 1860-63, which certainly belonged either to the true Zebra (*Equus zebra*) or to its closely allied northern form, the recently described *Equus grevyi*.

Meteorological Society, April 18.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—T. G. Bowick, E. C. Clifton, H. Culley, Dr. W. Doberck, A. N. Pearson, Prof. H. Robinson, and J. E. Worth were elected Fellows of the Society.—The following papers were read:—On cirrus and cirro-cumulus, by the Hon. F. A. Rollo Russell, M.A., F.M.S. The author points out that next to frequent readings of the barometer and a knowledge of the distribution of atmospheric pressure, observation of the character of clouds, especially of cirrus, is of the greatest use in attempting to forecast coming weather. Observation of cirrus can plainly be made use of in a telegraphic system of weather forecasts as easily as observation of the barometer, and the employment of a number of scattered cirrus observers largely increases the probability of this form of cloud being noted. The paper contains a description of twelve different varieties of cirrus, with the weather they signify or at least precede, as observed by the author during the last eighteen years.—Some notes on waterspouts, their occurrence and formation, by George Attwood, F.G.S. This contains an account of several waterspouts observed in the Pacific Ocean, and also one seen in the Atlantic Ocean. The author believes that the waterspouts in the Pacific Ocean were caused by a cloud heavily charged with cooled moisture drifting from the high mountains of Costa Rica coming into contact with air-currents and clouds travelling in a different direction, and of a warmer temperature; by which contact the cloud heavily charged with moisture was given a rotatory motion, causing it to discharge part of its moisture and make it assume a cylindrical figure and fall down by its own gravity.—Records of bright sunshine, by W. W. Rundell, F.M.S. This is a discussion of the sunshine records made in the United Kingdom during the years 1881 and 1882, from which it appears that there is more bright sunshine upon the coast than there is inland.—Note on wind, cloudiness, and halos; also on results from a Redier's barograph, by E. T. Dowson, F.M.S.—On the cold weather of March, 1883, by W. Marriott, F.M.S. The weather of this month will long be remembered for its very cold, dry, and windy character. The winter had been very mild, dull, and wet, and continued so to the beginning of March. A sudden change took place, however, on the 6th. A severe northerly gale set in on that day, accompanied with snow showers and a keen biting wind This gale was most violent in the North Sea, and caused sad havoc among the fishing fleet on the east coast, no less than 382 men and boys being drowned. The temperature fell considerably, the maximum being below 40° almost all over the country, and in the North of England only a trifle above the freezing point. The same conditions prevailed for the next two or three days, the temperature however falling still lower, and on the 10th the minimum occurred in the central and northern districts. The most remarkable weather of the month took place from the 21st to the 24th. Owing to a brisk fall of the barometer over France an easterly gale was experienced over this country, and as the temperature was low and the air very dry the wind was exceedingly bitter and keen, and its effect upon the human frame was most distressing.

SYDNEY

Linnean Society of New South Wales, February 28.—C. S. Wilkinson, F.G.S., president, in the chair.—The following papers were read:—On the coal flora of Australia, by the Rev. J. E. Tenison-Woods, F.L.S., F.G.S., &c. This was a complete monograph of all the known fossil coal plants, including the new species recently discovered by the author. A diagnosis of each genus and species was given, together with a history of the subject and its literature. The author also added his own views with reference to the classification, in which he regards some of the Newcastle beds as Pervian, some as Trias, and the Ipswich beds (Queensland), the Victorian carbonaceous

(Bellerine, Cape Otway, Apollo Bay, Colac and the Wannon), Tasmanian (Jerusalem), and the Hawkesbury sandstone as Jurassic or Lower Oolite. He expresses a doubt whether the Wianamatta beds can be regarded as a distinct formation, his own opinion being that they are shales distributed at various levels all through the Hawkesbury sandstone. The new species of plants described are: *Phyllotheca concinna*, *Equisetum rotiferum*, *Vertebraria tivoliensis*, *V. towarrensis*, *Sphenopteris* (*Aneimoides*) *flabellifolia*, *S.* (*A.*) *f.* var. *erecta*, *Trichomanides laxum*, *T. spinifolium*, *Thinnfeldia media*, *T. australis*, *T. falcata*, *Alethopteris currani*, *Tæniopteris carruthersi*, *Gleichenia* (?) *lineata*, *Jeanpaulia bidens*, *Ptilophyllum oligonerum*, *Brachyphyllum crassum* (which the author thinks may be a variety of *B. mamidare*), *Sequoites australis*, *Walchia milneana*, *Cunninghamites australis*. Besides these new species, the following Indian or European fossils are new to Australia:—*Podozamites lanceolatus*, Lindley and Hutton; *Merianopsis major*, Feist; *Angiopteridium ensis*, Oldham. The monograph is meant to be a complete reference for students on the subject of Australian coal fossils, and is illustrated by six plates of heliographs and two of lithographs.—Further contributions to the flora of Queensland, by the Rev. B. Scortechini, F.L.S.—Descriptions of two new fungi, by the Rev. Carl Kalchbrenner. The species described are *Polyporus Pentzkei* and *Paxillus hirtulus*, both from the Daintree River, Queensland.—Notes on the fructification of the Bunya Bunya in Sydney, by the Hon. James Norton, M.L.C.—Descriptions of some new fishes from Port Jackson, by E. P. Ramsay, F.L.S.—The President read some notes on the Tuena Gold Reefs, by M. F. Rate, mining engineer.

BERLIN

Physical Society, March 16.—Dr. Frölich exhibited a torsion galvanometer prepared in Messrs. Siemens and Halske's establishment for measuring electricity mechanically, in which the deflection of the magnetic needle is indicated by the corresponding torsion of a spring whose constant expansion power is known. The torsion galvanometer was at first constructed for measuring the current of the large dynamoelectric machine fitted up in Ocker for copper electroplating, and which at least resistance possesses a power of 800 amperes. Here it was impossible to employ either a dynamometer, owing to the irregularity of the mercurial contact, or a tangent compass, which has to be directly inserted in the main circuit. Hence measurement could be effected only by lateral closing, and as Dr. Frolich fully explained, the determination of the potential at any required number of points in the circuit, as rendered possible by the new apparatus, gives the data for ascertaining the electromotor strength, the resistance, and the power of the current. He described in great detail the construction and adjustment of the new appliance, in which, after insertion of determined resistances in the lateral circuit, the number of volts can be read off, and from these the amperes and ohms determined in the simplest manner. The torsion galvanometer is prepared in two forms, vertical with a magnet suspended to a cocoa fibre, and horizontal with a magnet resting on an edge. The latter form is intended especially for cases in which the apparatus undergoes no delicate manipulation.—Prof. Neesen briefly mentioned modifications which he has introduced both in the heat regulator used by him and in his ice calorimeter, illustrating them with diagrams. He has found them work well in practice.

PARIS

Academy of Sciences, April 16.—M. Blanchard in the chair.—M. Jordan read a note on the works of the late Prof. H. Smith, and M. Bertrand added some remarks on the award of the mathematical prize.—Two new methods for determination of the right ascension of polar stars, and of the inclination of the axis of a meridian above the equator, by M. Lœwy.—Memoir on the temperature at the surface of the ground and of the earth to 36m. depth, as also of the temperature of two pieces of ground, one bare, the other covered with turf, during 1882, by MM. Becquerel. This confirms previous results.—Graphic demonstration of a theorem of Euler concerning the partition of numbers, by Prof. Sylvester.—On the project of the interior African Sea, by M. de Lesseps. After a visit to the region, he affirms (with several associates) the urgency and feasibility of the scheme.—M. Wolf was elected Member in the Section of Astronomy in place of the late M. Liouville.—On the evolution of malignant pustule in man and its treatment with iodised injections, by M. Richet. So long as general infection has not commenced, by bacteria or their spores entering the blood, active local treatment with tincture of iodine is efficacious.—Experiments on caustic anæsthesia, and observation of a case of ulcerated tumour of the breast operated with the aid of this method, by M. Guérin. A space was cauterised round the tumour with Vienna caustic and incised throughout; then the tumour was detached.—Mechanical action produced by magnets and by terrestrial magnetism (second memoir), by M. Le Cordier.—Calculus of a double integral, by M. Callandreau.—Observations of the Swift-Brooks comet at Lyons Observatory, by M. Gonessiat.—Law of periods (continued), by M. de Jonquières.—On the groups of transformations of linear differential equations, by M. Picard.—On functions with lacunar spaces, by M. Poincaré.—On a generalisation of the theorem of Fermat, by M. Picquet.—On the heat of combination of glycolates and the law of thermal constants of substitution, by M. Tommasi.—On the liquefaction of oxygen and nitrogen, and on the solidification of sulphide of carbon and alcohol, by MM. Wroblewski and Olszewski. By making ethylene boil in vacuo, they obtained temperatures as low as − 136° C. Liquid oxygen was obtained easily; it is colourless and transparent like carbonic acid; is very mobile and forms a very distinct meniscus. Sulphide of carbon freezes about − 116° C. Alcohol solidifies (after being viscous about − 129°) about − 130° 5, forming a white body. Liquid nitrogen (colourless, and with visible meniscus) was obtained later.—Researches on phosphates, by MM. Hautefeuille and Margottet.—On artificial Hausmannite, by M. Gorgeu.—On the chloride of pyrosulphuryl, by M. Konovaloff.—On the difference of reactional aptitude, &c. (continued), by M. Henry.—Researches on the essence of Angelica of roots (*Angelica officinalis*), by M. Maudin.—Some effects of climate on the rapidity of growth of plants, by M. Capus. His measurements of various trees and shrubs in the botanical garden of Samarcand show the remarkable rapidity of growth there in April, May, and June.—Orientation of leaves with reference to light, by M. Mer. Certain parts of leaves (the border generally) receive the luminous impression, while other parts (petioles, motor-enlargements) perform the movements necessary to place the former in a favourable position.—Contribution to the experimental study of the elongation of nerves, by M. Minor. He supports the view that this stretching is a purely local operation, a sort of incomplete section of a nerve.—Experimental studies on the physiological action of iodoform, by M. Rummo.—New experimental researches on the physiological action of veratrine, by MM. Pecholier and Redier.—The synthesis of the heavens and the earth, by M. Moigno. He deduces all from ether, first forming hydrogen. Universal gravitation is the direct effect of impulsions of ether.—A *frontal electric photophore*, for medical use, was described by MM. Helot and Trouvé. It is an incandescent lamp, supplied from a bichromate battery, and fitted with a reflector and convergent lens. It is attached to the forehead.

CONTENTS

PAGE
SCIENTIFIC WORTHIES, XXI.—WILLIAM SPOTTISWOODE (*With Steel Plate Engraving*) 597
"A MANUAL OF THE INFUSORIA." By Prof. E. RAY LANKESTER, F.R.S. 601

OUR BOOK SHELF:—

Griffith and Henfrey's "Micrographic Dictionary" 603

LETTERS TO THE EDITOR:—

Speke and Grant's Zebra.—Sir J. FAYRER, K.C.S.I., F.R.S. . . 604
Leaves and their Environment.—GRANT ALLEN 604
Forms of Leaves.—Sir JOHN LUBBOCK, Bart., M.P. 605
The Fohn.—A. IRVING 605
The Zodiacal Light (?)—W. H. ROBINSON; ROBERT DWARRIS GIBNEY; E. BROWN; D. J. ROWAN 605
On the Value of the "Neoarctic" as one of the Primary Zoological Regions.—Prof. ANGELO HEILPRIN 606
Mock Moons.—F. T. MOTT 606
Benevolence in Animals.—GEORGE J. ROMANES, F.R.S. 607
"Medioscribed Circle."—R. T. 607
AGRICULTURE IN MADRAS 607
ANTHROPOLOGICAL NOTES IN THE SOLOMON ISLANDS. By Surgeon-Major H. B. GUPPY (*With Illustration*) 607
ON A FINE SPECIMEN OF APATITE FROM TYROL, LATELY IN THE POSSESSION OF MR. SAMUEL HENSON (*With Illustration*) . . . 608
THE EVOLUTION OF THE AMERICAN TROTTING-HORSE. By WM. H. BREWER 609
INSTITUTION OF MECHANICAL ENGINEERS 611
CORONERS' SCIENCE IN CHINA. By ROBERT K. DOUGLAS 612
ZOOLOGY IN JAPAN 614
NOTES 614

OUR ASTRONOMICAL COLUMN:—

Schmidt's Variable Star near Spica 617
D'Arrest's Comet 618
ON THE SENSE OF COLOUR AMONGST SOME OF THE LOWER ANIMALS. By Sir JOHN LUBBOCK, Bart., M.P. 618
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 618
SOCIETIES AND ACADEMIES 618

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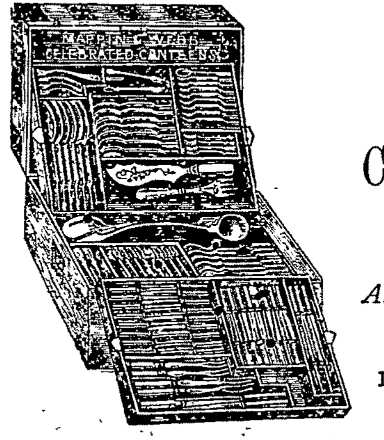

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CONTENTS

ORIGINAL COMMUNICATIONS.—
On the Treatment of the Night-sweating of Phthisis. By William Murrell, M.D., M.R.C.P.
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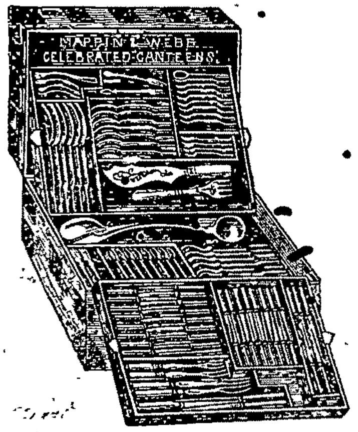

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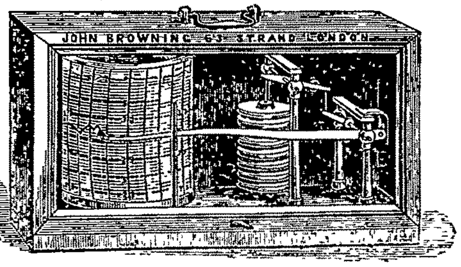

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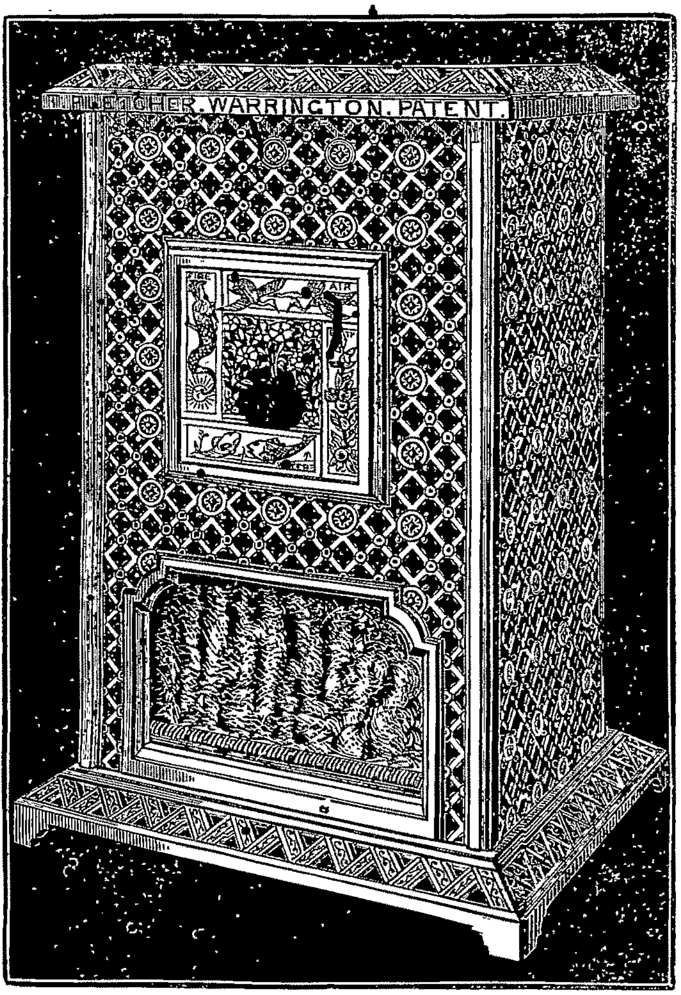

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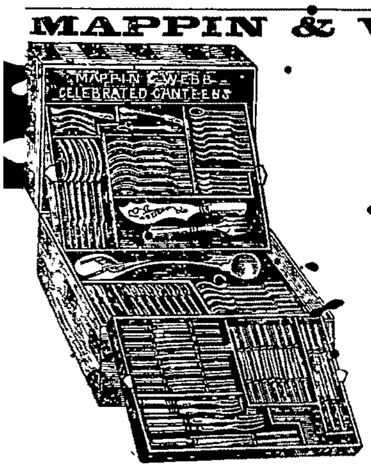

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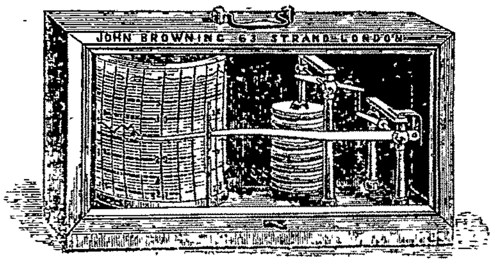

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SUNDAY LECTURE SOCIETY, at 4 —Metamorphoses of Insects: Prof P. M. Duncan.

MONDAY, NOVEMBER 20

ARISTOTELIAN SOCIETY, at 7.30 —Locke to Berkeley · G. White —Berkeley to Hume: A. M. Ogilvie

TUESDAY, NOVEMBER 21.

STATISTICAL SOCIETY, at 7 45

WEDNESDAY, NOVEMBER 22.

SOCIETY OF ARTS, at 8.—Ice-making and Refrigerating Dr. Hopkinson, F R.S.

THURSDAY, NOVEMBER 23

ROYAL SOCIETY, at 4 30

SOCIETY OF TELEGRAPH ENGINEERS, at 8 —Notes on the Telegraphs used during the Operations of the Expeditionary Force in Egypt Lieut -Col. Webber, R E.

FRIDAY, NOVEMBER 24.

QUEKETT MICROSCOPICAL CLUB, at 8.—The Statoblasts of Freshwater Sponges: B. W. Priest.

SATURDAY, NOVEMBER 25.

PHYSICAL SOCIETY, at 3.—On Liquid Slabs: Dr F. Guthrie.—On Rainbow formed by Reflected Light W. Ackroyd

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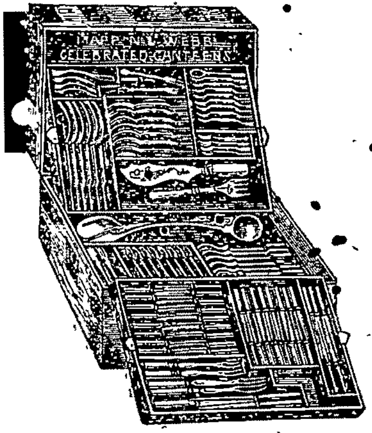

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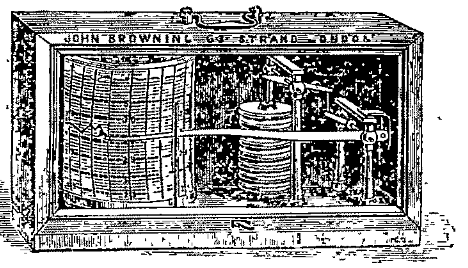

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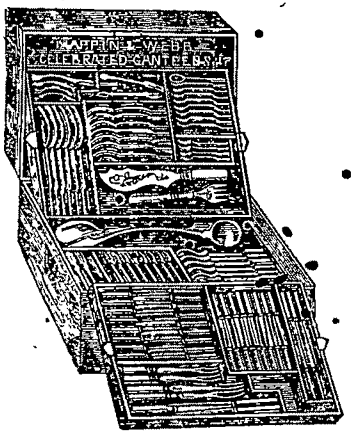

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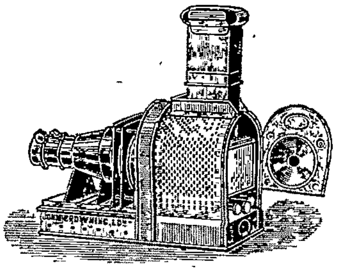

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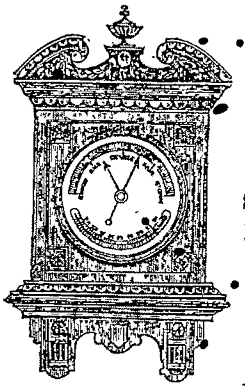

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BACHELOR OF SCIENCE.—Intermediate, Monday, July 16
B.Sc., Monday, October 15.
DOCTOR OF SCIENCE.—Within the first twenty-one days of June.
BACHELOR OF LAWS.—Intermediate, LL.B. } Tuesday, January 2.
DOCTOR OF LAWS.—Tuesday, January 16
BACHELOR OF MEDICINE.—Preliminary Scientific, Monday, July 16.
Intermediate, Monday, July 30.
M.B., Monday, November 5.
BACHELOR OF SURGERY.—Tuesday, December 4
MASTER IN SURGERY.—Monday, December 3.
DOCTOR OF MEDICINE.—Monday, December 3
SUBJECTS RELATING TO PUBLIC HEALTH.—Monday, December 10
BACHELOR OF MUSIC.—Intermediate, Monday, December 10.
B.Mus., Monday, December 17
DOCTOR OF MUSIC.—Intermediate, Monday, December 10
D.Mus., Monday, December 17.

ART, &c., OF TEACHING.—Tuesday, March 6.

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SUNDAY, DECEMBER 10.

SUNDAY LECTURE SOCIETY, at 4.—The Struggle for Land: Sir A. Hobhouse.

MONDAY, DECEMBER 11.

ROYAL GEOGRAPHICAL SOCIETY, at 8 30

LONDON INSTITUTION, at 5.—Thomas Carlyle: J Cotter Morison.

SOCIETY OF ARTS, at 8.—Dynamo-Electric Machinery: Prof. S. P. Thompson.

ARISTOTELIAN SOCIETY, at 7.30.—Discussion of the President's Address.

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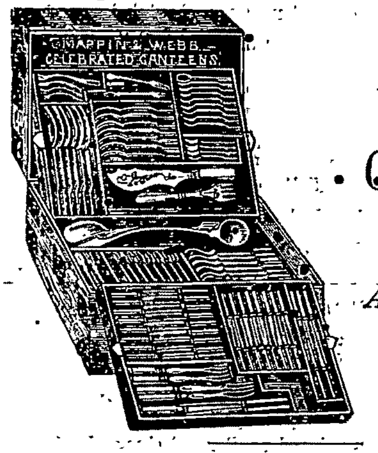

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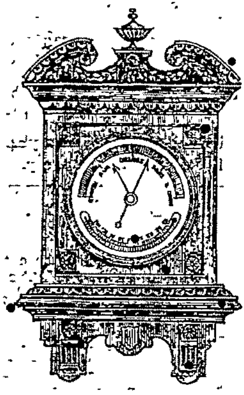

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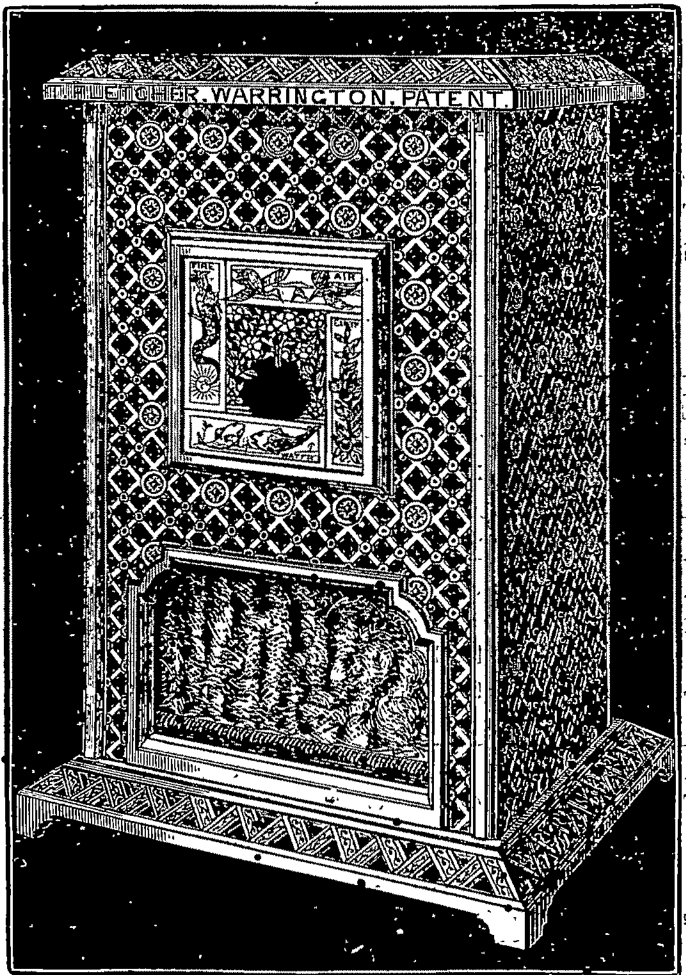

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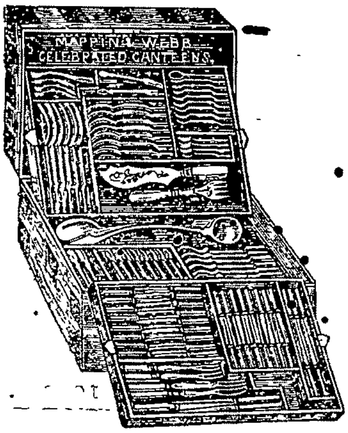

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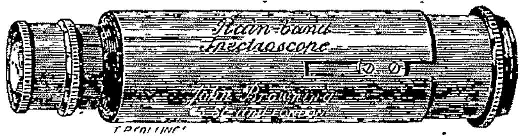

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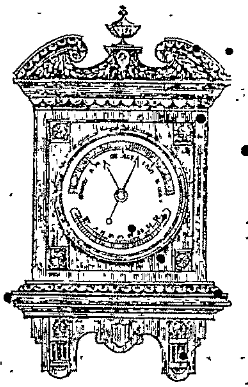

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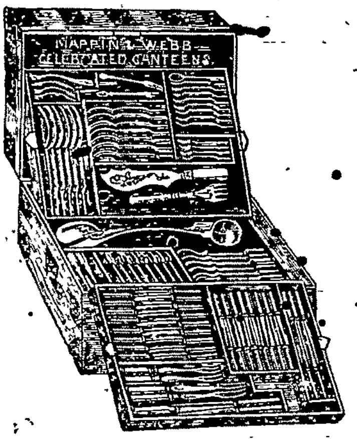

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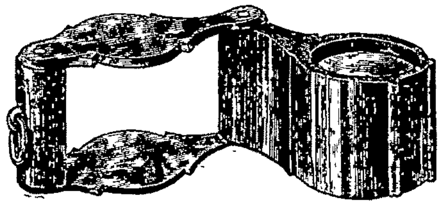

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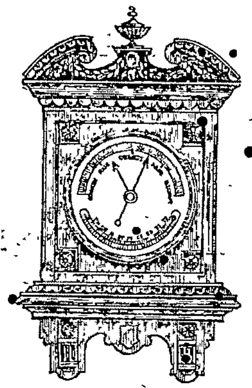

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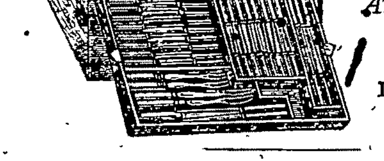

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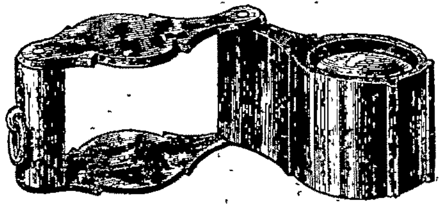

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ROYAL ASTRONOMICAL SOCIETY, at 8.

SUNDAY, JANUARY 14.

SUNDAY LECTURE SOCIETY, at 4.—Personal Adornment among Savage and Civilised People: Arthur Nicols.

MONDAY, JANUARY 15.

ROYAL GEOGRAPHICAL SOCIETY, at 8 30

VICTORIA INSTITUTE, at 8 —Paper by Prof G C Stokes.

LONDON INSTITUTION, at 5 —The Races of India: James Cotton.

ARISTOTELIAN SOCIETY, at 7.30 —Discussion of the President's Address

TUESDAY, JANUARY 16.

ZOOLOGICAL SOCIETY, at 8 30.—On Freshwater Shells from Socotra: Lieut Colonel H. H Godwin-Austen, F.R S —On the Right Cardiac Valves of Echidna and Ornithorhynchus: Prof Ray Lankester, F R S —Descriptions of New Genera and Species of Asiatic Lepidoptera Heterocera: F. Moore

STATISTICAL SOCIETY, at 7 45 —Statistics of Agricultural Production: Major Patrick G. Craigie.

ROYAL INSTITUTION, at 3 —Primeval Ancestors of Existing Vegetation: Prof. Williamson

WEDNESDAY, JANUARY 17.

METEOROLOGICAL SOCIETY, at 7 —Annual Meeting.

SOCIETY OF ARTS, at 8.—Sanitary Inspection of Houses: W. K. Burton.

THURSDAY, JANUARY 18.

ROYAL SOCIETY, at 4 30.

LINNEAN SOCIETY, at 8.—On the Fall of Branchlets in the Aspen: S. G Shattock —Certain Points in the Anatomy of *Polynöe clava*: A. G. Bourne.—The Internal Hard Parts of the Fungidæ: Prof. P. M. Duncan. —Observations on the Physiology of the Echinoderms · G. J. Romanes.

ROYAL INSTITUTION, at 3 —The Spectroscope: Prof. Dewar.

LONDON INSTITUTION, at 7.—English War Poetry; Prof. H. Morley.

FRIDAY, JANUARY 19

ROYAL INSTITUTION, at 9.—Lord Lawrence in India: R. B. Smith.

SATURDAY, JANUARY 20.

ROYAL INSTITUTION, at 3.—Lord Lawrence: R. B Smith.

SOCIETY OF TELEGRAPH ENGINEERS, at 8 —Inaugural Address: Willoughby Smith.

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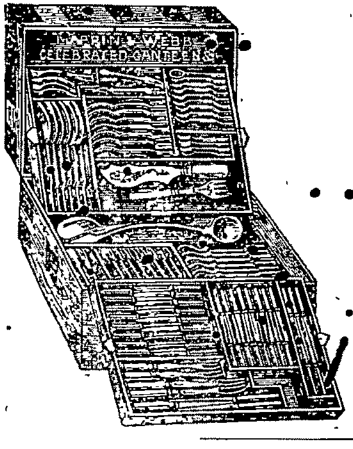

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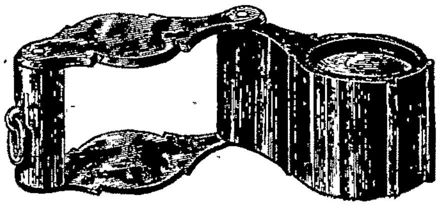

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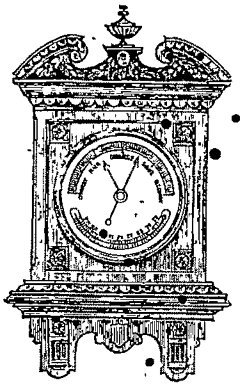

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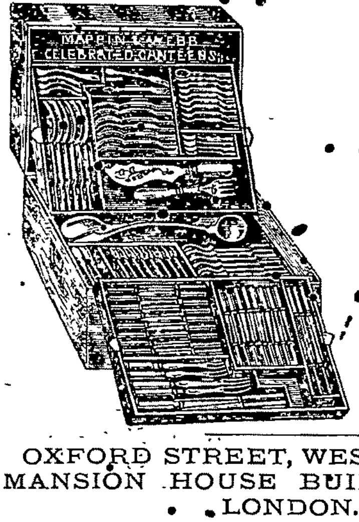

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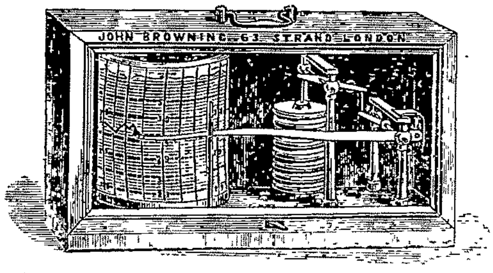

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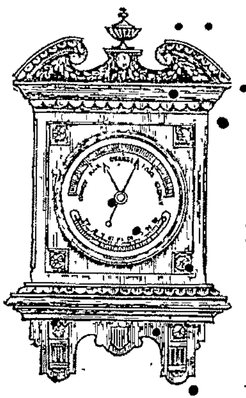

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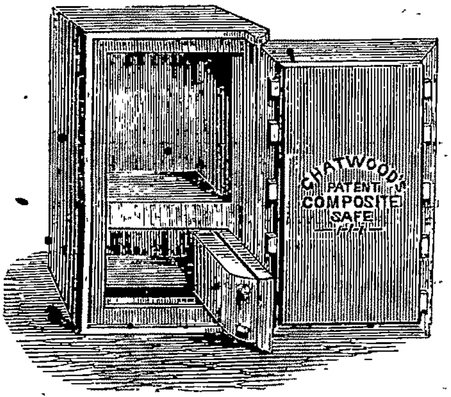

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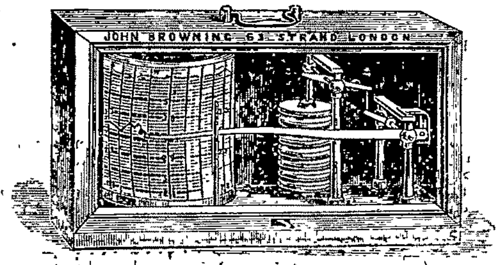

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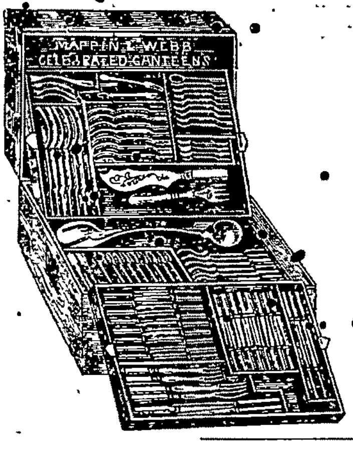

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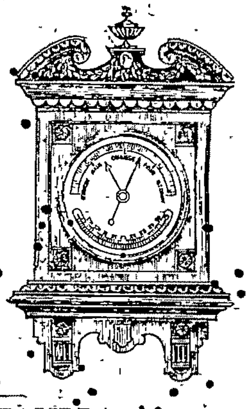

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LONDON INSTITUTION, at 5.—The Physiology of the Brain: Aubrey Husband.

SOCIETY OF ARTS, at 8.—Solid and Liquid Illuminating Agents. Leopold Field.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

SOCIETY OF CHEMICAL INDUSTRY, at 8.

TUESDAY, FEBRUARY 6.

ZOOLOGICAL SOCIETY, at 8.30.—On Additions to the Menagerie in January, —On a New Species of the Genus Otidiphaps: Messrs. Salvin and Godman.—Further Notes on *Tragelaphus gratus*; Mr. Sclater —Supplementary Notes on the Avifauna of the Argentine Republic: E. W. White,

ROYAL INSTITUTION, at 3.—Primeval Ancestors of Existing Vegetation: Prof. W. C. Williamson

WEDNESDAY, FEBRUARY 7.

GEOLOGICAL SOCIETY, at 8.—On the Metamorphic and Overlying Rocks in parts of Ross and and Inverness-shire: Henry Hicks, M D., with Petrological Notes by Prof. T. G. Bonney, F R.S.—On the Lower Carboniferous Rocks in the Forest of Dean, as represented in Typical Sections at Drybrook: E. Wethered.—On the relation of the so-called "Northampton Sand" of North Oxon to the Clypeous Grit: E. A. Walford.

ENTOMOLOGICAL SOCIETY, at 7.

SOCIETY OF ARTS, at 8.—The Modern Lathe: J H Evans.

THURSDAY, FEBRUARY 8.

ROYAL SOCIETY, at 4.30

MATHEMATICAL SOCIETY, at 8.—The Sylvester-Kempe Quadruplane: H. Hart.—Curves obtained by an Extension of Maclaurin's Method of constructing Conics: S Roberts, F R S.—A Method for reducing a certain Differential Expression to the Standard Form: J. Griffiths.—The Use of certain Differential Operators in the Theory of Equations. J. Hammond.

SOCIETY OF TELEGRAPH ENGINEERS, at 8.—On the Magnetic Storm of November 17, 1882: James Graves. Communicated by H Weaver —On a Magnetic Storm in India: E. O. Walker.—On Earth Currents in India: E. O Walker —Earth Currents (Third Paper), Sun-spots, and Electric Storms: Alex J. S. Adams.

ROYAL INSTITUTION, at 3 —The Spectroscope: Prof. Dewar

LONDON INSTITUTION, at 7.—Modern Classical Architecture: Prof. R. Kerr.

FRIDAY, FEBRUARY 9.

ROYAL INSTITUTION, at 9.—Emerson: M. D. Conway.

SATURDAY, FEBRUARY 10.

ROYAL INSTITUTION, at 3 —Lord Lawrence: R. B Smith.

PHYSICAL SOCIETY, at 3.—Annual General Meeting —On the Graphic Representation of the Duty and Efficiency of Electric Motors: Prof. Silvanus Thompson.

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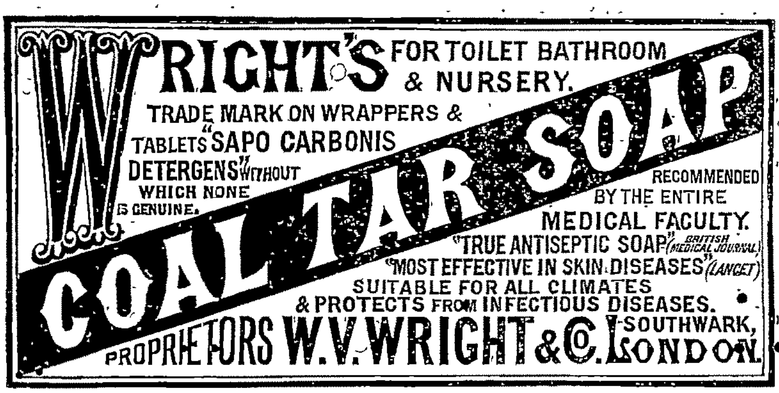

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LINNEAN SOCIETY, at 8.—Outer Peridium of Broomeia: G. Murray.—The Manna or Lerp Insect: J. G. Otto Tepper.—Elongation of Pedicel of an Orchid after Flowering: W. B. Hemsley.—Ceylon Corals: W. C. Ondaatje.—Flora of Madagascar, III.: J. G. Baker.

CHEMICAL SOCIETY, at 8.—On some Derivatives of Diphenylene-ketone Oxide: A. G. Perkin.

LONDON INSTITUTION, at 7.—Europe since Napoleon's Fall: C. A. Fyffe.

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FRIDAY, FEBRUARY 16.

ROYAL INSTITUTION, at 9.—Anomalous Forms of Primæval Vegetation: Prof. W. C. Williamson.

GEOLOGICAL SOCIETY, at 1.—Anniversary.

SATURDAY, FEBRUARY 17.

ROYAL INSTITUTION, at 3.—Singing, Speaking, Stammering: Dr. W. H. Stone.

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ZOOLOGICAL SOCIETY, at 8.30.—On Birds from Timor Laut, collected by Mr. Henry Forbes: Mr. Sclater.—On some New or Rare Species of Echinodermata: Prof. J. Jeffrey Bell.—On the Lingual and Hyoid Apparatus of Birds: Dr. Hans Gadow.—On some Points in the Anatomy of the Laniidæ, Paridæ, and Tenuirostres: Dr. Hans Gadow.

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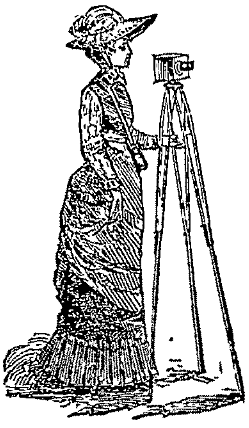

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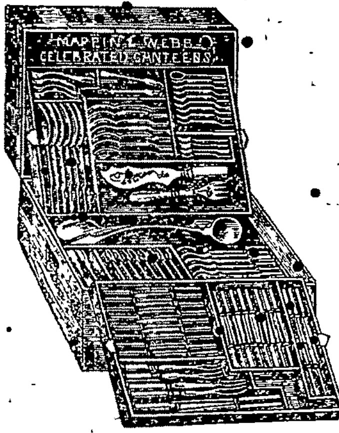

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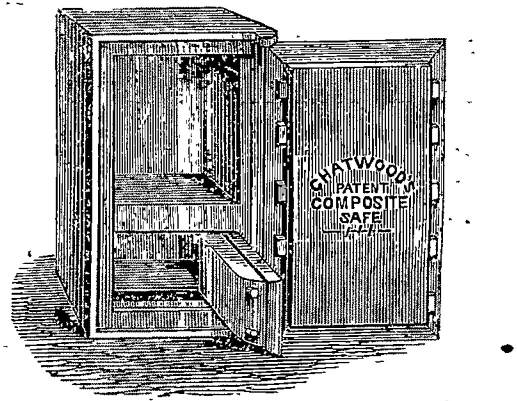

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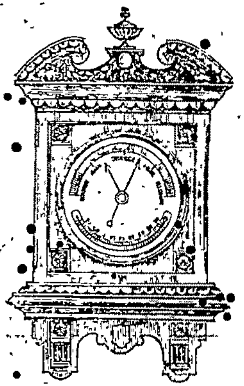

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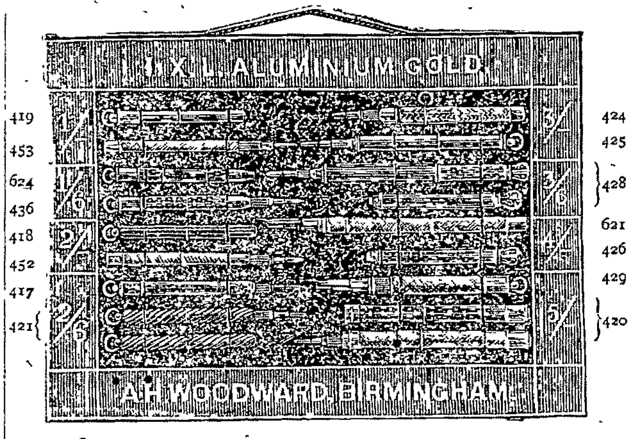

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ROYAL ASTRONOMICAL SOCIETY, at 8.

SATURDAY, MARCH 10.

PHYSICAL SOCIETY, at 3.—On a Method of Measuring Electrical Resistances with a Constant Current: Shelford Bidwell.—On Certain Molecular Constants: Prof. Guthrie, F.R.S.

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SUNDAY, MARCH 11.

SUNDAY LECTURE SOCIETY, at 4.—Religious Liberty: Miss Orme.

MONDAY, MARCH 12.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 5.—The Great Pyramid: R. A. Proctor.

TUESDAY, MARCH 13.

ANTHROPOLOGICAL INSTITUTE, at 8.—Report on the Ethnology of Timorlaut: H. O. Forbes (communicated by the Committee of the British Association through John Evans, F.R.S.).—On the Classification of Languages: Dr. Gustav Oppert.

PHOTOGRAPHIC SOCIETY, at 8.

HORTICULTURAL SOCIETY, at 1.—Scientific Committee.

ROYAL INSTITUTION, at 3.—The Supreme Discoveries in Astronomy (The Astronomical Significance of Heat): Prof. R. S. Ball.

WEDNESDAY, MARCH 14.

ROYAL MICROSCOPICAL SOCIETY, at 8.—On a Batch of New Floscules: Dr. C. T. Hudson.

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LINNEAN SOCIETY, at 8.—On *Simondsia paradoxa* and *Sphærularia bombi*: Dr. T. Spencer Cobbold.—Moths of the family Urapteridæ: A. G. Butler.—Mollusca of *Challenger* Expedition (part 18): Rev. R. Boog Watson.

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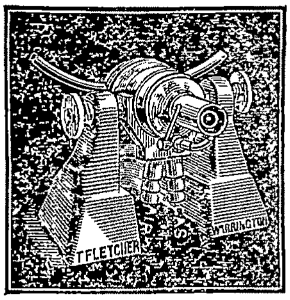

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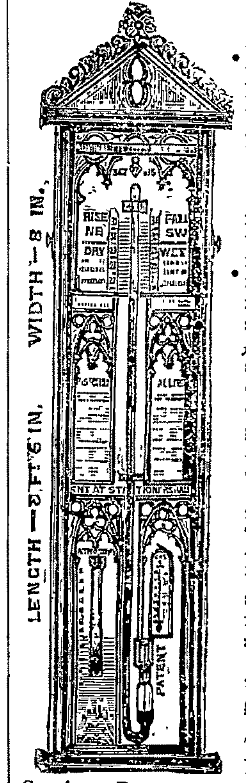

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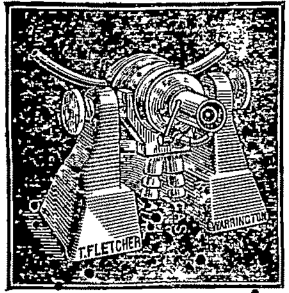

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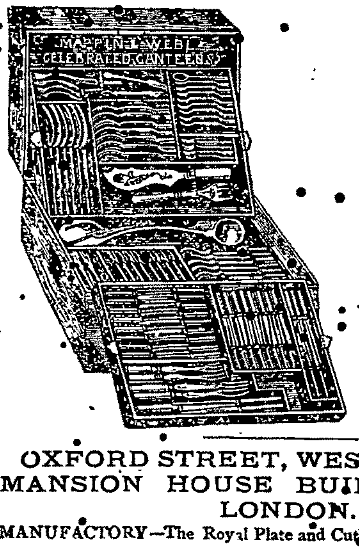

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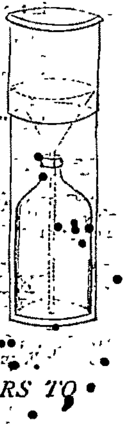

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ZOOLOGICAL SOCIETY, at 8.30.—On the Arrangement of the Orders and Families of Mammals: Prof Flower, F R.S.—On the Limnaina and Euplœina, two Groups of Diurnal Lepidoptera belonging to the Subfamily Euplœinæ, with Descriptions of New Genera and Species: F Moore

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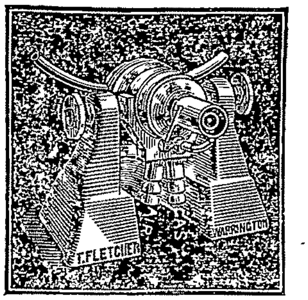

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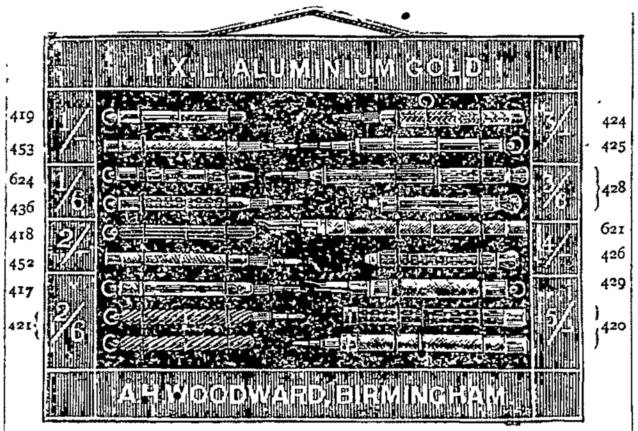

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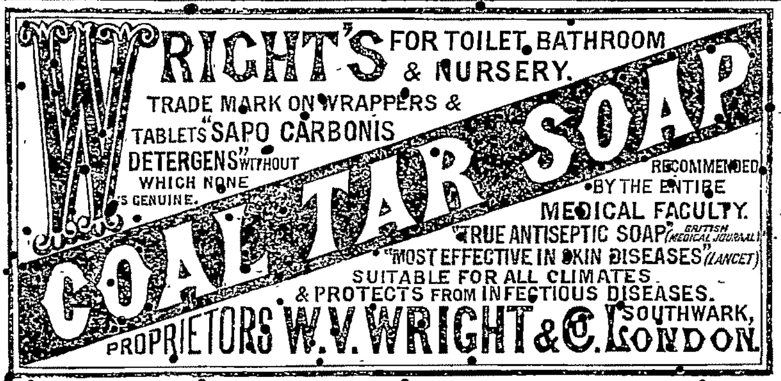

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III.—THE ENGLISH STAGE.
IV.—JAMES NASMYTH
V.—ILLUSTRIOUS WOMEN.
VI.—THE FRENCH REPUBLIC.
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FRIDAY, APRIL 13.

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SATURDAY, APRIL 14.

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ARISTOTELIAN SOCIETY, at 7.30.—Kant's Critic of Pure Reason: E. B. Bax.

VICTORIA INSTITUTE, at 8.

TUESDAY, APRIL 17.

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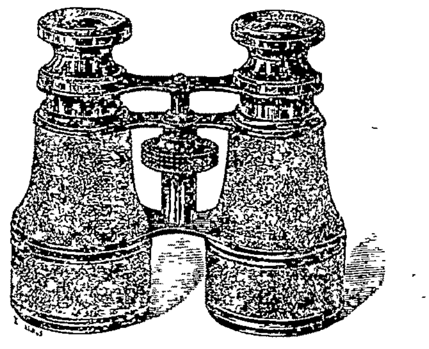

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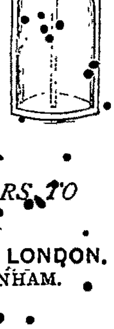

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FRIDAY, April 20.

Society of Arts, at 8.—Fisheries of India: Surgeon-General F. Day.

Royal Institution, at 9.—The Island of Socotra: Prof Balfour.

SATURDAY, April 21.

Royal Institution, at 3.—Geographical Evolution. A. Geikie.

MONDAY, April 23.

Royal Geographical Society, at 8 30.

Society of Arts, at 8.—The Transmission of Energy: Osborne Reynolds, F.R.S.

TUESDAY, April 24.

Royal Horticultural Society, at 11.—Scientific Committee.

Anthropological Institute, at 8.—The Mechanical Methods of the Egyptians: W. M. Flinders Petrie.—On some Palæolithic Knapping Tools and Modes of Using them: F. C J. Spurrell, F G S.

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Photographic Society, at 8.

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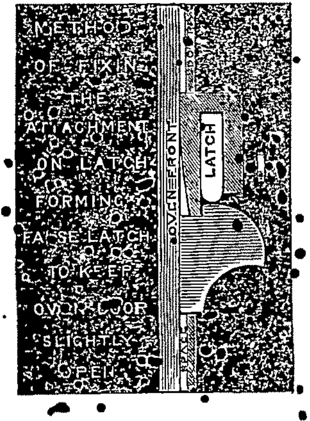
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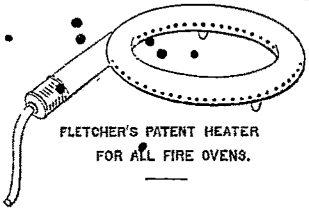

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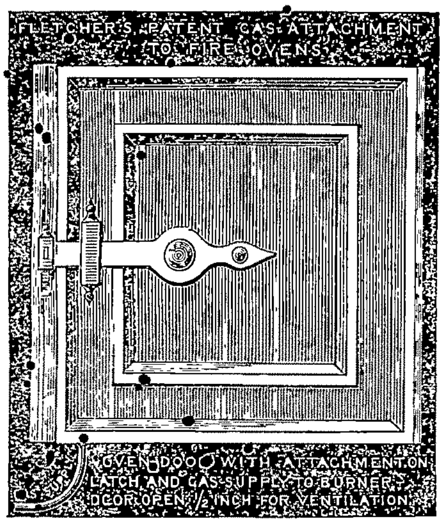
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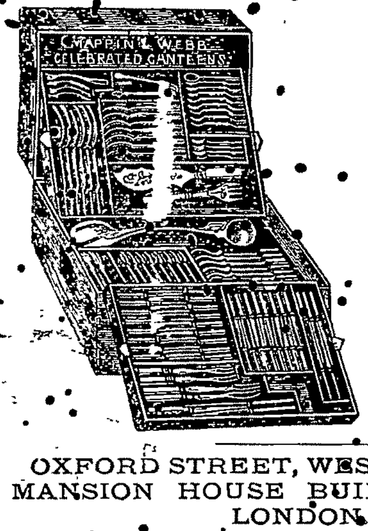

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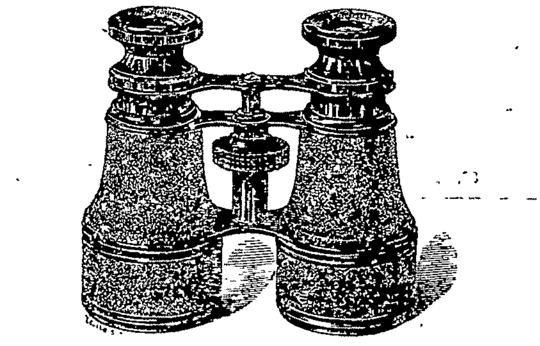

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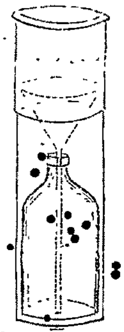

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